# British Association for the Advancement of Science.

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## ADDRESS

BY

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**PRESIDENT.**

In its beneficent progress through these islands the British Association for the Advancement of Science now for the fourth time receives a welcome in this ancient capital. Once again, under the shadow of these antique towers, crowded memories of a romantic past fill our thoughts. The stormy annals of Scotland seem to move in procession before our eyes as we walk these streets, whose names and traditions have been made familiar to the civilised world by the genius of literature. At every turn, too, we are reminded, by the monuments which a grateful city has erected, that for many generations the pursuits which we are now assembled to foster have had here their congenial home. Literature, philosophy, science, have each in turn been guided by the influence of the great masters who have lived here, and whose renown is the brightest gem in the chaplet around the brow of this 'Queen of the North.'

Lingering for a moment over these local associations, we shall find a peculiar appropriateness in the time of this renewed visit of the Association to Edinburgh. A hundred years ago a remarkable group of men was discussing here the great problem of the history of the earth. James Hutton, after many years of travel and reflection, had communi-

cated to the Royal Society of this city, in the year 1785, the first outlines of his famous ' Theory of the Earth.'  Among those with whom he took counsel in the elaboration of his doctrines were Black, the illustrious discoverer of ' fixed air ' and ' latent heat '; Clerk, the sagacious inventor of the system of breaking the enemy's line in naval tactics; Hall, whose fertile ingenuity devised the first system of experiments in illustration of the structure and origin of rocks; and Playfair, through whose sympathetic enthusiasm and literary skill Hutton's views came ultimately to be understood and appreciated by the world at large.  With these friends, so well able to comprehend and criticise his efforts to pierce the veil that shrouded the history of this globe, he paced the streets amid which we are now gathered together; with them he sought the crags and ravines around us, wherein Nature has laid open so many impressive records of her past; with them he sallied forth on those memorable expeditions to distant parts of Scotland, whence he returned laden with treasures from a field of observation which, though now so familiar, was then almost untrodden.  The centenary of Hutton's ' Theory of the Earth ' is an event in the annals of science which seems most fittingly celebrated by a meeting of the British Association in Edinburgh.

In choosing from among the many subjects which might properly engage your attention on the present occasion, I have thought that it would not be inappropriate nor uninteresting to consider the more salient features of that ' Theory,' and to mark how much in certain departments of inquiry has sprung from the fruitful teaching of its author and his associates.

It was a fundamental doctrine of Hutton and his school that this globe has not always worn the aspect which it bears at present; that, on the contrary, proofs may everywhere be culled that the land which we now see has been formed out of the wreck of an older land.  Among these proofs, the most obvious are supplied by some of the more familiar kinds of rock, which teach us that, though they are now portions of the dry land, they were originally sheets of gravel, sand, and mud, which had been worn from the face of long-vanished continents, and after being spread out over the floor of the sea were consolidated into compact stone, and were finally broken up and raised once more to form part of the dry land.  This cycle of change involved two great systems of natural processes.  On the one hand, men were taught that by the action of running water the materials of the solid land are in a state of continual decay and transport to the ocean.  On the other hand, the ocean-floor is

liable from time to time to be upheaved by some stupendous internal force akin to that which gives rise to the volcano and the earthquake. Hutton further perceived that not only had the consolidated materials been disrupted and elevated, but that masses of molten rock had been thrust upward among them, and had cooled and crystallised in large bodies of granite and other eruptive rocks which form so prominent a feature on the earth's surface.

It was a special characteristic of this philosophical system that it sought in the changes now in progress on the earth's surface an explanation of those which occurred in older times. Its founder refused to invent causes or modes of operation, for those with which he was familiar seemed to him adequate to solve the problems with which he attempted to deal. Nowhere was the profoundness of his insight more astonishing than in the clear, definite way in which he proclaimed and reiterated his doctrine, that every part of the surface of the continents, from mountain-top to sea-shore, is continually undergoing decay, and is thus slowly travelling to the sea. He saw that no sooner will the sea-floor be elevated into new land than it must necessarily become a prey to this universal and unceasing degradation. He perceived that, as the transport of disintegrated material is carried on chiefly by running water, rivers must slowly dig out for themselves the channels in which they flow, and thus that a system of valleys, radiating from the water-parting of a country, must necessarily result from the descent of the streams from the mountain crests to the sea. He discerned that this ceaseless and wide-spread decay would eventually lead to the entire demolition of the dry land, but he contended that from time to time this catastrophe is prevented by the operation of the underground forces, whereby new continents are upheaved from the bed of the ocean. And thus in his system a due proportion is maintained between land and water, and the condition of the earth as a habitable globe is preserved.

A theory of the earth so simple in outline, so bold in conception, so full of suggestion, and resting on so broad a base of observation and reflection, ought, we might think, to have commanded at once the attention of men of science, even if it did not immediately awaken the interest of the outside world; but, as Playfair sorrowfully admitted, it attracted notice only very slowly, and several years elapsed before anyone showed himself publicly concerned about it, either as an enemy or a friend. Some of its earliest critics assailed it for what they asserted to be its irreligious tendency—an accusation which Hutton repudiated with much warmth. The sneer levelled by Cowper a few years earlier at all inquiries

into the history of the universe was perfectly natural and intelligible from that poet's point of view. There was then a wide-spread belief that this world came into existence some six thousand years ago, and that any attempt greatly to increase that antiquity was meant as a blow to the authority of Holy Writ. So far, however, from aiming at the overthrow of orthodox beliefs, Hutton evidently regarded his 'Theory' as an important contribution in aid of natural religion. He dwelt with unfeigned pleasure on the multitude of proofs which he was able to accumulate of an orderly design in the operations of nature, decay and renovation being so nicely balanced as to maintain the habitable condition of the planet. But as he refused to admit the predominance of violent action in terrestrial changes, and on the contrary contended for the efficacy of the quiet, continuous processes which we can even now see at work around us, he was constrained to require an unlimited duration of past time for the production of those revolutions of which he perceived such clear and abundant proofs in the crust of the earth. The general public, however, failed to comprehend that the doctrine of the high antiquity of the globe was not inconsistent with the comparatively recent appearance of man—a distinction which seems so obvious now.

Hutton died in 1797, beloved and regretted by the circle of friends who had learnt to appreciate his estimable character and to admire his genius, but with little recognition from the world at large. Men knew not then that a great master had passed away from their midst, who had laid broad and deep the foundations of a new science; that his name would become a household word in after generations, and that pilgrims would come from distant lands to visit the scenes from which he drew his inspiration.

Many years might have elapsed before Hutton's teaching met with wide acceptance, had its recognition depended solely on the writings of the philosopher himself. For, despite his firm grasp of general principles and his mastery of the minutest details, he had acquired a literary style which, it must be admitted, was singularly unattractive. Fortunately for his fame, as well as for the cause of science, his devoted friend and disciple, Playfair, at once set himself to draw up an exposition of Hutton's views. After five years of labour on this task there appeared the classic 'Illustrations of the Huttonian Theory,' a work which for luminous treatment and graceful diction stands still without a rival in English geological literature. Though professing merely to set forth his friend's doctrines, Playfair's treatise was in many respects an original contribution to science of the highest value. It placed for the first time in the

clearest light the whole philosophy of Hutton regarding the history of the earth, and enforced it with a wealth of reasoning and copiousness of illustration which obtained for it a wide appreciation. From long converse with Hutton, and from profound reflection himself, Playfair gained such a comprehension of the whole subject that, discarding the non-essential parts of his master's teaching, he was able to give so lucid and accurate an exposition of the general scheme of Nature's operations on the surface of the globe, that with only slight corrections and expansions his treatise may serve as a text-book to-day. In some respects, indeed, his volume was long in advance of its time. Only, for example, within the present generation has the truth of his teaching in regard to the origin of valleys been generally admitted.

Various causes contributed to retard the progress of the Huttonian doctrines. Especially potent was the influence of the teaching of Werner, who, though he perceived that a definite order of sequence could be recognised among the materials of the earth's crust, had formed singularly narrow conceptions of the great processes whereby that crust has been built up. His enthusiasm, however, fired his disciples with the zeal of proselytes, and they spread themselves over Europe to preach everywhere the artificial system which they had learnt in Saxony. By a curious fate Edinburgh became one of the great headquarters of Wernerism. The friends and followers of Hutton found themselves attacked in their own city by zealots who, proud of superior mineralogical acquirements, turned their most cherished ideas upside down and assailed them in the uncouth jargon of Freiberg. Inasmuch as subterranean heat had been invoked by Hutton as a force largely instrumental in consolidating and upheaving the ancient sediments that now form so great a part of the dry land, his followers were nicknamed Plutonists. On the other hand, as the agency of water was almost alone admitted by Werner, who believed the rocks of the earth's crust to have been chiefly chemical precipitates from a primeval universal ocean, those who adopted his views received the equally descriptive name of Neptunists. The battle of these two contending schools raged fiercely here for some years, and though mainly from the youth, zeal, and energy of Jameson, and the influence which his position as Professor in the University gave him, the Wernerian doctrines continued to hold their place, they were eventually abandoned even by Jameson himself, and the debt due to the memory of Hutton and Playfair was tardily acknowledged.

The pursuits and the quarrels of philosophers have from early times been a favourite subject of merriment to the outside world. Such a feud

as that between the Plutonists and Neptunists would be sure to furnish abundant matter for the gratification of this propensity. Turning over the pages of Kay's 'Portraits,' where so much that was distinctive of Edinburgh society a hundred years ago is embalmed, we find Hutton's personal peculiarities and pursuits touched off in good-humoured caricature. In one plate he stands with arms folded and hammer in hand, meditating on the face of a cliff, from which rocky prominences in shape of human faces, perhaps grotesque likenesses of his scientific opponents, grin at him. In another engraving he sits in conclave with his friend Black, possibly arranging for that famous banquet of garden-snails which the two worthies had persuaded themselves to look upon as a strangely neglected form of human food. More than a generation later, when the Huttonists and Wernerists were at the height of their antagonism, the humorous side of the controversy did not escape the notice of the author of 'Waverley,' who, you will remember, when he makes Meg Dods recount the various kinds of wise folk brought by Lady Penelope Pennfeather from Edinburgh to St. Ronan's Well, does not forget to include those who 'rin uphill and down dale, knapping the chucky-stanes to pieces wi' hammers, like sae mony road-makers run daft, to see how the warld was made.'

Among the names of the friends and followers of Hutton there is one which on this occasion deserves to be held in especial honour, that of Sir James Hall, of Dunglass. Having accompanied Hutton in some of his excursions, and having discussed with him the problems presented by the rocks of Scotland, Hall was familiar with the views of his master, and was able to supply him with fresh illustrations of them from different parts of the country. Gifted with remarkable originality and ingenuity, he soon perceived that some of the questions involved in the theory of the earth could probably be solved by direct physical experiment. Hutton, however, mistrusted any attempt 'to judge of the great operations of Nature by merely kindling a fire and looking into the bottom of a little crucible.' Out of deference to this prejudice Hall delayed to carry out his intention during Hutton's lifetime. But afterwards he instituted a remarkable series of researches which are memorable in the history of science as the first methodical endeavour to test the value of geological speculation by an appeal to actual experiment. The Neptunists, in ridiculing the Huttonian doctrine that basalt and similar rocks had once been molten, asserted that, had such been their origin, these masses would now be found in the condition of glass or slag. Hall, however, triumphantly vindicated his friend's view by proving that basalt could be fused

and thereafter by slow cooling could be made to resume a stony texture. Again, Hutton had asserted that under the vast pressures which must be effective deep within the earth's crust, chemical reactions must be powerfully influenced, and that under such conditions even limestone may conceivably be melted without losing its carbonic acid. Various specious arguments had been adduced against this proposition, but by an ingeniously devised series of experiments Hall succeeded in converting limestone under great pressure into a kind of marble, and even fused it, and found that it then acted vigorously on other rocks. These admirable researches, which laid the foundations of experimental geology, constitute not the least memorable of the services rendered by the Huttonian school to the progress of science.

Clear as was the insight and sagacious the inferences of these great masters in regard to the history of the globe, their vision was necessarily limited by the comparatively narrow range of ascertained fact which up to their time had been established. They taught men to recognise that the present world is built of the ruins of an earlier one, and they explained with admirable perspicacity the operation of the processes whereby the degradation and renovation of land are brought about. But they never dreamed that a long and orderly series of such successive destructions and renewals had taken place, and had left their records in the crust of the earth. They never imagined that from these records it would be possible to establish a determinate chronology that could be read everywhere, and applied to the elucidation of the remotest quarter of the globe. It was by the memorable observations and generalisations of William Smith that this vast extension of our knowledge of the past history of the earth became possible. While the Scottish philosophers were building up their theory here, Smith was quietly ascertaining by extended journeys that the stratified rocks of the West of England occur in a definite sequence, and that each well-marked group of them can be discriminated from the others and identified across the country by means of its enclosed organic remains. It is nearly a hundred years since he made known his views, so that by a curious coincidence we may fitly celebrate on this occasion the centenary of William Smith as well as that of James Hutton. No single discovery has ever had a more momentous and far-reaching influence on the progress of a science than that law of organic succession which Smith established. At first it served merely to determine the order of the stratified rocks of England. But it soon proved to possess a world-wide value, for it was found to furnish the key to the structure of the whole stratified crust of the earth. It showed that

within that crust lie the chronicles of a long history of plant and animal life upon this planet, it supplied the means of arranging the materials for this history in true chronological sequence, and it thus opened out a magnificent vista through a vast series of ages, each marked by its own distinctive types of organic life, which, in proportion to their antiquity, departed more and more from the aspect of the living world.

Thus a hundred years ago, by the brilliant theory of Hutton and the fruitful generalisation of Smith, the study of the earth received in our country the impetus which has given birth to the modern science of geology.

To review the marvellous progress which this science has made during the first century of its existence would require not one but many hours for adequate treatment. The march of discovery has advanced along a multitude of different paths, and the domains of Nature which have been included within the growing territories of human knowledge have been many and ample. Nevertheless, there are certain departments of investigation to which we may profitably restrict our attention on the present occasion, and wherein we may see how the leading principles that were proclaimed in this city a hundred years ago have germinated and borne fruit all over the world.

From the earliest times the natural features of the earth's surface have arrested the attention of mankind. The rugged mountain, the cleft ravine, the scarped cliff, the solitary boulder, have stimulated curiosity and prompted many a speculation as to their origin. The shells embedded by millions in the solid rocks of hills far removed from the sea have still further pressed home these 'obstinate questionings.' But for many long centuries the advance of inquiry into such matters was arrested by the paramount influence of orthodox theology. It was not merely that the Church opposed itself to the simple and obvious interpretation of those natural phenomena. So implicit had faith become in the accepted views of the earth's age and of the history of creation, that even laymen of intelligence and learning set themselves unbidden and in perfect good faith to explain away the difficulties which Nature so persistently raised up, and to reconcile her teachings with those of the theologians. In the various theories thus originating, the amount of knowledge of natural law usually stood in inverse ratio to the share played in them by an uncontrolled imagination. The speculations, for example, of Burnet, Whiston, Whitehurst, and others in this country, cannot be read now without a smile. In no sense were they scientific researches; they can only be looked upon as exercitations of learned ignorance. Springing

mainly out of a laudable desire to promote what was believed to be the cause of true religion, they helped to retard inquiry, and exercised in that respect a baneful influence on intellectual progress.

It is the special glory of the Edinburgh school of geology to have cast aside all this fanciful trifling. Hutton boldly proclaimed that it was no part of his philosophy to account for the beginning of things. His concern lay only with the evidence furnished by the earth itself as to its origin With the intuition of true genius he early perceived that the only solid basis from which to explore what has taken place in bygone time is a knowledge of what is taking place to-day. He thus founded his system upon a careful study of the processes whereby geological changes are now brought about. He felt assured that Nature must be consistent and uniform in her working, and that only in proportion as her operations at the present time are watched and understood will the ancient history of the earth become intelligible. Thus, in his hands, the investigation of the Present became the key to the interpretation of the Past. The establishment of this great truth was the first step towards the inauguration of a true science of the earth. The doctrine of the uniformity of causation in Nature became the fruitful principle on which the structure of modern geology could be built up.

Fresh life was now breathed into the study of the earth. A new spirit seemed to animate the advance along every pathway of inquiry. Facts that had long been familiar came to possess a wider and deeper meaning when their connection with each other was recognised as parts of one great harmonious system of continuous change. In no department of Nature, for example, was this broader vision more remarkably displayed than in that wherein the circulation of water between land and sea plays the most conspicuous part. From the earliest times men had watched the coming of clouds, the fall of rain, the flow of rivers, and had recognised that on this nicely adjusted machinery the beauty and fertility of the land depend. But they now learnt that this beauty and fertility involve a continual decay of the terrestrial surface; that the soil is a measure of this decay, and would cease to afford us maintenance were it not continually removed and renewed; that through the ceaseless transport of soil by rivers to the sea the face of the land is slowly lowered in level and carved into mountain and valley, and that the materials thus borne outwards to the floor of the ocean are not lost but accumulate there to form rocks, which in the end will be upraised into new lands. Decay and renovation, in well-balanced proportions, were thus shown to be the system on which the existence of the earth as a habitable globe had been

established. It was impossible to conceive that the economy of the planet could be maintained on any other basis. Without the circulation of water the life of plants and animals would be impossible, and with that circulation the decay of the surface of the land and the renovation of its disintegrated materials are necessarily involved.

As it is now so must it have been in past time. Hutton and Playfair pointed to the stratified rocks of the earth's crust as demonstrations that the same processes which are at work to-day have been in operation from a remote antiquity. By thus placing their theory on a basis of actual observation, and providing in the study of existing operations a guide to the interpretation of those in past times, they rescued the investigation of the history of the earth from the speculations of theologians and cosmologists, and established a place for it among the recognised inductive sciences. To the guiding influence of their philosophical system the prodigious strides made by modern geology are in large measure to be attributed. And here in their own city, after the lapse of a hundred years, let us offer to their memory the grateful homage of all who have profited by their labours.

But while we recognise with admiration the far-reaching influence of the doctrine of uniformity of causation in the investigation of the history of the earth, we must upon reflection admit that the doctrine has been pushed to an extreme perhaps not contemplated by its original founders. To take the existing conditions of Nature as a platform of actual knowledge from which to start in an inquiry into former conditions was logical and prudent. Obviously, however, human experience, in the few centuries during which attention has been turned to such subjects, has been too brief to warrant any dogmatic assumption that the various natural processes must have been carried on in the past with the same energy and at the same rate as they are carried on now. Variations in energy might have been legitimately conceded as possible, though not to be allowed without reasonable proof in their favour. It was right to refuse to admit the operation of speculative causes of change when the phenomena were capable of natural and adequate explanation by reference to causes that can be watched and investigated. But it was an error to take for granted that no other kind of process or influence, nor any variation in the rate of activity save those of which man has had actual cognisance, has played a part in the terrestrial economy. The uniformitarian writers laid themselves open to the charge of maintaining a kind of perpetual motion in the machinery of Nature. They could find in the records of the earth's history no evidence of a beginning, no prospect of an end. They

saw that many successive renovations and destructions had been effected on the earth's surface, and that this long line of vicissitudes formed a series of which the earliest were lost in antiquity, while the latest were still in progress towards an apparently illimitable future.

The discoveries of William Smith, had they been adequately understood, would have been seen to offer a corrective to this rigidly uniformitarian conception, for they revealed that the crust of the earth contains the long record of an unmistakable order of progression in organic types. They proved that plants and animals have varied widely in successive periods of the earth's history, the present condition of organic life being only the latest phase of a long preceding series, each stage of which recedes further from the existing aspect of things as we trace it backward into the past. And though no relic had yet been found, or indeed was ever likely to be found, of the first living things that appeared upon the earth's surface, the manifest simplification of types in the older formations pointed irresistibly to some beginning from which the long procession had taken its start. If then it could thus be demonstrated that there had been upon the globe an orderly 'march of living forms from the lowliest grades in early times to man himself to-day, and thus that in one department of her domain, extending through the greater portion of the records of the earth's history, Nature had not been uniform but had followed a vast and noble plan of evolution, surely it might have been expected that those who discovered and made known this plan would seek to ascertain whether some analogous physical progression from a definite beginning might not be discernible in the framework of the globe itself.

But the early masters of the science laboured under two great disadvantages. In the first place, they found the oldest records of the earth's history so broken up and effaced as to be no longer legible. And in the second place, they lived under the spell of that strong reaction against speculation which followed the bitter controversy between the Neptunists and Plutonists in the earlier decades of the century. They considered themselves bound to search for facts, not to build up theories; and as in the crust of the earth they could find no facts which threw any light upon the primeval constitution and subsequent development of our planet, they shut their ears to any theoretical interpretations that might be offered from other departments of science. It was enough for them to maintain, as Hutton had done, that in the visible structure of the earth itself no trace can be found of the beginning of things, and that the oldest terrestrial records reveal no physical conditions essentially different from

those in which we still live.   They doubtless listened with interest to the
speculations of Kant, Laplace, and Herschel, on the probable evolution
of nebulæ, suns, and planets, but it was with the languid interest attach-
ing to ideas that lay outside of their own domain of research.   They re-
cognised no practical connection between such speculations and the data
furnished by the earth itself as to its own history and progress

This curious lethargy with respect to theory on the part of men who
were popularly regarded as among the most speculative followers of
science would probably not have been speedily dispelled by any discovery
made within their own field of observation.   Even now, after many years
of the most diligent research, the first chapters of our planet's history
remain undiscovered or undecipherable.   On the great terrestrial palimp-
sest the earliest inscriptions seem to have been hopelessly effaced by those
of later ages.   But the question of the primeval condition and subsequent
history of the planet might be considered from the side of astronomy and
physics.   And it was by investigations of this nature that the geological
torpor was eventually dissipated.   To our illustrious former President,
Lord Kelvin, who occupied this chair when the Association last met in
Edinburgh, is mainly due the rousing of attention to this subject.   By
the most convincing arguments he showed how impossible it was to
believe in the extreme doctrine of uniformitarianism.   And though,
owing to uncertainty in regard to some of the data, wide limits of time
were postulated by him, he insisted that within these limits the whole
evolution of the earth and its inhabitants must have been comprised.
While, therefore, the geological doctrine that the present order of Nature
must be our guide to the interpretation of the past remained as true and
fruitful as ever, it had now to be widened by the reception of evidence
furnished by a study of the earth as a planetary body.   The secular loss
of heat, which demonstrably takes place both from the earth and the sun,
made it quite certain that the present could not have been the original
condition of the system.   This diminution of temperature with all its
consequences is not a mere matter of speculation, but a physical fact of
the present time as much as any of the familiar physical agencies that
affect the surface of the globe.   It points with unmistakable direct-
ness to that beginning of things of which Hutton and his followers could
find no sign.

Another modification or enlargement of the uniformitarian doctrine
was brought about by continued investigation of the terrestrial crust and
consequent increase of knowledge respecting the history of the earth.

Though Hutton and Playfair believed in periodical catastrophes, and indeed required these to recur in order to renew and preserve the habitable condition of our planet, their successors gradually came to view with repugnance any appeal to abnormal, and especially to violent manifestations of terrestrial vigour, and even persuaded themselves that such slow and comparatively feeble action as had been witnessed by man could alone be recognised in the evidence from which geological history must be compiled. Well do I remember in my own boyhood what a cardinal article of faith this prepossession had become. We were taught by our great and honoured master, Lyell, to believe implicitly in gentle and uniform operations, extended over indefinite periods of time, though possibly some, with the zeal of partisans, carried this belief to an extreme which Lyell himself did not approve. The most stupendous marks of terrestrial disturbance, such as the structure of great mountain chains, were deemed to be more satisfactorily accounted for by slow movements prolonged through indefinite ages than by any sudden convulsion.

What the more extreme members of the uniformitarian school failed to perceive was the absence of all evidence that terrestrial catastrophes even on a colossal scale might not be a part of the present economy of this globe. Such occurrences might never seriously affect the whole earth at one time, and might return at such wide intervals that no example of them has yet been chronicled by man. But that they have occurred again and again, and even within comparatively recent geological times, hardly admits of serious doubt. How far at different epochs and in various degrees they may have included the operation of cosmical influences lying wholly outside the planet, and how far they have resulted from movements within the body of the planet itself, must remain for further inquiry. Yet the admission that they have played a part in geological history may be freely made without impairing the real value of the Huttonian doctrine, that in the interpretation of this history our main guide must be a knowledge of the existing processes of terrestrial change.

As the most recent and best known of these great transformations, the Ice Age stands out conspicuously before us. If any one sixty years ago had ventured to affirm that at no very distant date the snows and glaciers of the Arctic regions stretched southwards into France, he would have been treated as a mere visionary theorist. Many of the facts to which he would have appealed in support of his statement were already well known, but they had received various other interpretations. By some observers, notably by Hutton's friend, Sir James Hall, they were believed to be due to violent debacles of water that swept over the face

of the land. By others they were attributed to the strong tides and currents of the sea when the land stood at a lower level. The uniformitarian school of Lyell had no difficulty in elevating or depressing land to any required extent. Indeed, when we consider how averse these philosophers were to admit any kind or degree of natural operation other than those of which there was some human experience, we may well wonder at the boldness with which, on sometimes the slenderest evidence, they made land and sea change places, on the one hand submerging mountain-ranges, and on the other placing great barriers of land where a deep ocean rolls. They took such liberties with geography because only well-established processes of change were invoked in the operations. Knowing that during the passage of an earthquake a territory bordering the sea may be upraised or sunk a few feet, they drew the sweeping inference that any amount of upheaval or depression of any part of the earth's surface might be claimed in explanation of geological problems. The progress of inquiry, while it has somewhat curtailed this geographical license, has now made known in great detail the strange story of the Ice Age.

There cannot be any doubt that after man had become a denizen of the earth, a great physical change came over the northern hemisphere. The climate, which had previously been so mild that evergreen trees flourished within ten or twelve degrees of the north pole, now became so severe that vast sheets of snow and ice covered the north of Europe and crept southward beyond the south coast of Ireland, almost as far as the southern shores of England, and across the Baltic into France and Germany. This Arctic transformation was not an episode that lasted merely a few seasons, and left the land to resume thereafter its ancient aspect. With various successive fluctuations it must have endured for many thousands of years. When it began to disappear it probably faded away as slowly and imperceptibly as it had advanced, and when it finally vanished it left Europe and North America profoundly changed in the character alike of their scenery and of their inhabitants. The rugged rocky contours of earlier times were ground smooth and polished by the march of the ice across them, while the lower grounds were buried under wide and thick sheets of clay, gravel, and sand, left behind by the melting ice. The varied and abundant flora which had spread so far within the Arctic circle was driven away into more southern and less ungenial climes. But most memorable of all was the extirpation of the prominent large animals which, before the advent of the ice, had roamed over Europe. The lions, hyænas, wild horses,

hippopotami and other creatures either became entirely extinct or were driven into the Mediterranean basin and into Africa. In their place came northern forms—the reindeer, glutton, musk ox, woolly rhinoceros, and mammoth.

Such a marvellous transformation in climate, in scenery, in vegetation and in inhabitants, within what was after all but a brief portion of geological time, though it may have involved no sudden or violent convulsion, is surely entitled to rank as a catastrophe in the history of the globe. It was probably brought about mainly if not entirely by the operation of forces external to the earth. No similar calamity having befallen the continents within the time during which man has been recording his experience, the Ice Age might be cited as a contradiction to the doctrine of uniformity. And yet it manifestly arrived as part of the established order of Nature. Whether or not we grant that other ice ages preceded the last great one, we must admit that the conditions under which it arose, so far as we know them, might conceivably have occurred before and may occur again. The various agencies called into play by the extensive refrigeration of the northern hemisphere were not different from those with which we are familiar. Snow fell and glaciers crept as they do to-day. Ice scored and polished rocks exactly as it still does among the Alps and in Norway. There was nothing abnormal in the phenomena save the scale on which they were manifested. And thus, taking a broad view of the whole subject, we recognise the catastrophe, while at the same time we see in its progress the operation of those same natural processes which we know to be integral parts of the machinery whereby the surface of the earth is continually transformed.

Among the debts which science owes to the Huttonian school, not the least memorable is the promulgation of the first well-founded conceptions of the high antiquity of the globe. Some six thousand years had previously been believed to comprise the whole life of the planet, and indeed of the entire universe. When the curtain was then first raised that had veiled the history of the earth, and men, looking beyond the brief span within which they had supposed that history to have been transacted, beheld the records of a long vista of ages stretching far away into a dim illimitable past, the prospect vividly impressed their imagination. Astronomy had made known the immeasurable fields of space; the new science of geology seemed now to reveal boundless distances of time. The more the terrestrial chronicles were studied the farther could the eye range into an antiquity so vast as to defy all attempts to measure or

define it.   The progress of research continually furnished additional evidence of the enormous duration of the ages that preceded the coming of man, while, as knowledge increased, periods that were thought to have followed each other consecutively were found to have been separated by prolonged intervals of time.   Thus the idea arose and gained universal acceptance that, just as no boundary could be set to the astronomer in his free range through space, so the whole of bygone eternity lay open to the requirements of the geologist.   Playfair, re-echoing and expanding Hutton's language, had declared that neither among the records of the earth nor in the planetary motions can any trace be discovered of the beginning or of the end of the present order of things ; that no symptom of infancy or of old age has been allowed to appear on the face of Nature, nor any sign by which either the past or the future duration of the universe can be estimated ; and that although the Creator may put an end, as He no doubt gave a beginning, to the present system, such a catastrophe will not be brought about by any of the laws now existing, and is not indicated by anything which we perceive.   This doctrine was naturally espoused with warmth by the extreme uniformitarian school, which required an unlimited duration of time for the accomplishment of such slow and quiet cycles of change as they conceived to be alone recognisable in the records of the earth's past history.

It was Lord Kelvin who, in the writings to which I have already referred, first called attention to the fundamentally erroneous nature of these conceptions.   He pointed out that from the high internal temperature of our globe, increasing inwards as it does, and from the rate of loss of its heat, a limit may be fixed to the planet's antiquity.   He showed that so far from there being no sign of a beginning, and no prospect of an end to the present economy, every lineament of the solar system bears witness to a gradual dissipation of energy from some definite starting-point.   No very precise data were then, or indeed are now, available for computing the interval which has elapsed since that remote commencement, but he estimated that the surface of the globe could not have consolidated less than twenty millions of years ago, for the rate of increase of temperature inwards would in that case have been higher than it actually is ; nor more than 400 millions of years ago, for then there would have been no sensible increase at all.   He was inclined, when first dealing with the subject, to believe that from a review of all the evidence then available, some such period as 100 millions of years would embrace the whole geological history of the globe.

It is not a pleasant experience to discover that a fortune which one

has unconcernedly believed to be ample has somehow taken to itself wings and disappeared. When the geologist was suddenly awakened by the energetic warning of the physicist, who assured him that he had enormously overdrawn his account with past time, it was but natural under the circumstances that he should think the accountant to be mistaken, who thus returned to him dishonoured the large drafts he had made on eternity. He saw how wide were the limits of time deducible from physical considerations, how vague the data from which they had been calculated. And though he could not help admitting that a limit must be fixed beyond which his chronology could not be extended, he consoled himself with the reflection that after all a hundred millions of years was a tolerably ample period of time, and might possibly have been quite sufficient for the transaction of all the prolonged sequence of events recorded in the crust of the earth. He was therefore disposed to acquiesce in the limitation thus imposed upon geological history.

But physical inquiry continued to be pushed forward with regard to the early history and the antiquity of the earth. Further consideration of the influence of tidal friction in retarding the earth's rotation, and of the sun's rate of cooling, led to sweeping reductions of the time allowable for the evolution of the planet. The geologist found himself in the plight of Lear when his bodyguard of one hundred knights was cut down. 'What need you five-and-twenty, ten or five?' demands the inexorable physicist, as he remorselessly strikes slice after slice from his allowance of geological time. Lord Kelvin is willing, I believe, to grant us some twenty millions of years, but Professor Tait would have us content with less than ten millions.

In scientific as in other mundane questions there may often be two sides, and the truth may ultimately be found not to lie wholly with either. I frankly confess that the demands of the early geologists for an unlimited series of ages were extravagant, and even, for their own purposes, unnecessary, and that the physicist did good service in reducing them. It may also be freely admitted that the latest conclusions from physical considerations of the extent of geological time require that the interpretation given to the record of the rocks should be rigorously revised, with the view of ascertaining how far that interpretation may be capable of modification or amendment. But we must also remember that the geological record constitutes a voluminous body of evidence regarding the earth's history which cannot be ignored, and must be explained in accordance with ascertained natural laws. If the conclusions derived from the most careful study of this record cannot be reconciled with those drawn from physical

considerations, it is surely not too much to ask that the latter should be also revised. It has been well said that the mathematical mill is an admirable piece of machinery, but that the value of what it yields depends upon the quality of what is put into it. That there must be some flaw in the physical argument I can, for my own part, hardly doubt, though I do not pretend to be able to say where it is to be found. Some assumption, it seems to me, has been made, or some consideration has been left out of sight, which will eventually be seen to vitiate the conclusions, and which when duly taken into account will allow time enough for any reasonable interpretation of the geological record.

In problems of this nature, where geological data capable of numerical statement are so needful, it is hardly possible to obtain trustworthy computations of time. We can only measure the rate of changes in progress now, and infer from these changes the length of time required for the completion of results achieved by the same processes in the past. There is fortunately one great cycle of movement which admits of careful investigation, and which has been made to furnish valuable materials for estimates of this kind. The universal degradation of the land, so notable a characteristic of the earth's surface, has been regarded as an extremely slow process. Though it goes on without ceasing, yet from century to century it seems to leave hardly any perceptible trace on the landscapes of a country. Mountains and plains, hills and valleys, appear to wear the same familiar aspect which is indicated in the oldest pages of history. This obvious slowness in one of the most important departments of geological activity, doubtless contributed in large measure to form and foster a vague belief in the vastness of the antiquity required for the evolution of the earth.

But, as geologists eventually came to perceive, the rate of degradation of the land is capable of actual measurement. The amount of material worn away from the surface of any drainage-basin and carried in the form of mud, sand, or gravel, by the main river into the sea, represents the extent to which that surface has been lowered by waste in any given period of time. But denudation and deposition must be equivalent to each other. As much material must be laid down in sedimentary accumulations as has been mechanically removed, so that in measuring the annual bulk of sediment borne into the sea by a river, we obtain a clue not only to the rate of denudation of the land, but also to the rate at which the deposition of new sedimentary formations takes place.

As might be expected, the activities involved in the lowering of the surface of the land are not everywhere equally energetic. They are naturally more vigorous where the rainfall is heavy, where the daily

range of temperature is large, and where frosts are severe. Hence they are obviously much more effective in mountainous regions than on plains ; and their results must constantly vary, not only in different basins of drainage, but even, and sometimes widely, within the same basin. Actual measurement of the proportion of sediment in river water shows that while in some cases the lowering of the surface of the land may be as much as $\frac{1}{730}$ of a foot in a year, in others it falls as low as $\frac{1}{6800}$. In other words, the rate of deposition of new sedimentary formations, over an area of sea-floor equivalent to that which has yielded the sediment, may vary from one foot in 730 years to one foot in 6,800 years.

If now we take these results and apply them as measures of the length of time required for the deposition of the various sedimentary masses that form the outer part of the earth's crust, we obtain some indication of the duration of geological history. On a reasonable computation these stratified masses, where most fully developed, attain a united thickness of not less than 100,000 feet. If they were all laid down at the most rapid recorded rate of denudation, they would require a period of seventy-three millions of years for their completion. If they were laid down at the slowest rate they would demand a period of not less than 680 millions.

But it may be argued that all kinds of terrestrial energy are growing feeble, that the most active denudation now in progress is much less vigorous than that of bygone ages, and hence that the stratified part of the earth's crust may have been put together in a much briefer space of time than modern events might lead us to suppose. Such arguments are easily adduced and look sufficiently specious, but no confirmation of them can be gathered from the rocks. On the contrary, no one can thoughtfully study the various systems of stratified formations without being impressed by the fulness of their evidence that, on the whole, the accumulation of sediment has been extremely slow. Again and again we encounter groups of strata composed of thin paper-like laminæ of the finest silt, which evidently settled down quietly and at intervals on the sea bottom. We find successive layers covered with ripple-marks and sun-cracks, and we recognise in them memorials of ancient shores where sand and mud tranquilly gathered as they do in sheltered estuaries at the present day. We can see no proof whatever, nor even any evidence which suggests, that on the whole the rate of waste and sedimentation was more rapid during Mesozoic and Palæozoic time than it is to-day. Had there been any marked difference in this rate from ancient to modern times, it would be incredible that no clear proof of it should have been recorded in the crust of the earth.

But in actual fact the testimony in favour of the slow accumulation and high antiquity of the geological record is much stronger than might be inferred from the mere thickness of the stratified formations. These sedimentary deposits have not been laid down in one unbroken sequence, but have had their continuity interrupted again and again by upheaval and depression. So fragmentary are they in some regions, that we can easily demonstrate the length of time represented there by still existing sedimentary strata to be vastly less than the time indicated by the gaps in the series.

There is yet a further and impressive body of evidence furnished by the successive races of plants and animals which have lived upon the earth and have left their remains sealed up within its rocky crust. No one now believes in the exploded doctrine that successive creations and universal destructions of organic life are chronicled in the stratified rocks. It is everywhere admitted that, from the remotest times up to the present day, there has been an onward march of development, type succeeding type in one long continuous progression. As to the rate of this evolution precise data are wanting. There is, however, the important negative argument furnished by the absence of evidence of recognisable specific variations of organic forms since man began to observe and record. We know that within human experience a few species have become extinct, but there is no conclusive proof that a single new species has come into existence, nor are appreciable variations readily apparent in forms that live in a wild state. The seeds and plants found with Egyptian mummies, and the flowers and fruits depicted on Egyptian tombs, are easily identified with the vegetation of modern Egypt. The embalmed bodies of animals found in that country show no sensible divergence from the structure or proportions of the same animals at the present day. The human races of Northern Africa and Western Asia were already as distinct when portrayed by the ancient Egyptian artists as they are now, and they do not seem to have undergone any perceptible change since then. Thus a lapse of four or five thousand years has not been accompanied by any recognisable variation in such forms of plant and animal life as can be tendered in evidence. Absence of sensible change in these instances is, of course, no proof that considerable alteration may not have been accomplished in other forms more exposed to vicissitudes of climate and other external influences. But it furnishes at least a presumption in favour of the extremely tardy progress of organic variation.

If, however, we extend our vision beyond the narrow range of human

history, and look at the remains of the plants and animals preserved in those younger formations which, though recent when regarded as parts of the whole geological record, must be many thousands of years older than the very oldest of human monuments, we encounter the most impressive proofs of the persistence of specific forms. Shells which lived in our seas before the coming of the Ice Age present the very same peculiarities of form, structure, and ornament which their descendants still possess. The lapse of so enormous an interval of time has not sufficed seriously to modify them. So too with the plants and the higher animals which still survive. Some forms have become extinct, but few or none which remain display any transitional gradations into new species. We must admit that such transitions have occurred, that indeed they have been in progress ever since organised existence began upon our planet, and are doubtless taking place now. But we cannot detect them on the way, and we feel constrained to believe that their march must be excessively slow.

There is no reason to think that the rate of organic evolution has ever seriously varied; at least no proof has been adduced of such variation. Taken in connection with the testimony of the sedimentary rocks, the inferences deducible from fossils entirely bear out the opinion that the building up of the stratified crust of the earth has been extremely gradual. If the many thousands of years which have elapsed since the Ice Age have produced no appreciable modification of surviving plants and animals, how vast a period must have been required for that marvellous scheme of organic development which is chronicled in the rocks!

After careful reflection on the subject, I affirm that the geological record furnishes a mass of evidence which no arguments drawn from other departments of Nature can explain away, and which, it seems to me, cannot be satisfactorily interpreted save with an allowance of time much beyond the narrow limits which recent physical speculation would concede.

I have reserved for final consideration a branch of the history of the earth which, while it has become, within the lifetime of the present generation, one of the most interesting and fascinating departments of geological inquiry, owed its first impulse to the far-seeing intellects of Hutton and Playfair. With the penetration of genius these illustrious teachers perceived that if the broad masses of land and the great chains of mountains owe their origin to stupendous movements which from time to time

have convulsed the earth, their details of contour must be mainly due to
the eroding power of running water. They recognised that as the surface
of the land is continually worn down, it is essentially by a process of
sculpture that the physiognomy of every country has been developed,
valleys being hollowed out and hills left standing, and that these in-
equalities in topographical detail are only varying and local accidents in
the progress of the one great process of the degradation of the land.

From the broad and guiding outlines of theory thus sketched we have
now advanced amid ever-widening multiplicity of detail into a fuller and
nobler conception of the origin of scenery. The law of evolution is
written as legibly on the landscapes of the earth as on any other page of
the Book of Nature. Not only do we recognise that the existing topo-
graphy of the continents, instead of being primeval in origin, has gradu-
ally been developed after many precedent mutations, but we are enabled
to trace these earlier revolutions in the structure of every hill and glen.
Each mountain-chain is thus found to be a memorial of many successive
stages in geographical evolution. Within certain limits, land and sea
have changed places again and again. Volcanoes have broken out and
have become extinct in many countries long before the advent of man.
Whole tribes of plants and animals have meanwhile come and gone, and
in leaving their remains behind them as monuments at once of the slow
development of organic types, and of the prolonged vicissitudes of the
terrestrial surface, have furnished materials for a chronological arrange-
ment of the earth's topographical features. Nor is it only from the
organisms of former epochs that broad generalisations may be drawn
regarding revolutions in geography. The living plants and animals of
to-day have been discovered to be eloquent of ancient geographical
features that have long since vanished. In their distribution they tell
us that climates have changed, that islands have been disjoined from
continents, that oceans once united have been divided from each other,
or once separate have now been joined; that some tracts of land have
disappeared, while others for prolonged periods of time have remained in
isolation. The present and the past are thus linked together not merely
by dead matter, but by the world of living things, into one vast system
of continuous progression.

In this marvellous increase of knowledge regarding the transforma-
tions of the earth's surface, one of the most impressive features, to my
mind, is the power now given to us of perceiving the many striking
contrasts between the present and former aspects of topography and
scenery. We seem to be endowed with a new sense. What is seen by

the bodily eye—mountain, valley, or plain—serves but as a veil, beyond which, as we raise it, visions of long-lost lands and seas rise before us in a far-retreating vista. Pictures of the most diverse and opposite character are beheld, as it were, through each other, their lineaments subtly interwoven and even their most vivid contrasts subdued into one blended harmony. Like the poet, ' we see, but not by sight alone '; and the ' ray of fancy ' which, as a sunbeam, lightened up his landscape, is for us broadened and brightened by that play of the imagination which science can so vividly excite and prolong.

Admirable illustrations of this modern interpretation of scenery are supplied by the district wherein we are now assembled. On every side of us rise the most convincing proofs of the reality and potency of that ceaseless sculpture by which the elements of landscape have been carved into their present shapes. Turn where we may, our eyes rest on hills that project above the lowland, not because they have been upheaved into these positions, but because their stubborn materials have enabled them better to withstand the degradation which has worn down the softer strata into the plains around them. Inch by inch the surface of the land has been lowered, and each hard rock successively laid bare has communicated its own characteristics of form and colour to the scenery.

If, standing on the Castle Rock, the central and oldest site in Edinburgh, we allow the bodily eye to wander over the fair landscape, and the mental vision to range through the long vista of earlier landscapes which science here reveals to us, what a strange series of pictures passes before our gaze ! The busy streets of to-day seem to fade away into the mingled copsewood and forest of prehistoric time. Lakes that have long since vanished gleam through the woodlands, and a rude canoe pushing from the shore startles the red deer that had come to drink. While we look, the picture changes to a polar scene, with bushes of stunted Arctic willow and birch, among which herds of reindeer browse and the huge mammoth makes his home. Thick sheets of snow are draped all over the hills around, and far to the north-west the distant gleam of glaciers and snow-fields marks the line of the Highland mountains. As we muse on this strange contrast to the living world of to-day the scene appears to grow more Arctic in aspect, until every hill is buried under one vast sheet of ice, 2,000 feet or more in thickness, which fills up the whole midland valley of Scotland and creeps slowly eastward into the basin of the North Sea. Here the curtain drops upon our moving pageant, for in the geological record of this part of the country an enormous gap occurs before the coming of the Ice Age.

When once more the spectacle resumes its movement the scene is found to have utterly changed. The familiar hills and valleys of the Lothians have disappeared. Dense jungles of a strange vegetation—tall reeds, club-mosses, and tree-ferns—spread over the steaming swamps that stretch for leagues in all directions. Broad lagoons and open seas are dotted with little volcanic cones which throw out their streams of lava and showers of ashes. Beyond these, in dimmer outline and older in date, we descry a wide lake or inland sea, covering the whole midland valley and marked with long lines of active volcanoes, some of them several thousand feet in height. And still further and fainter over the same region, we may catch a glimpse of that still earlier expanse of sea which in Silurian times overspread most of Britain. But beyond this scene our vision fails. We have reached the limit across which no geological evidence exists to lead the imagination into the primeval darkness beyond.

Such in briefest outline is the succession of mental pictures which modern science enables us to frame out of the landscapes around Edinburgh. They may be taken as illustrations of what may be drawn, and sometimes with even greater fulness and vividness, from any district in these islands. But I cite them especially because of their local interest in connection with the present meeting of the Association, and because the rocks that yield them gave inspiration to those great masters whose claims on our recollection, not least for their explanation of the origin of scenery, I have tried to recount this evening. But I am further impelled to dwell on these scenes from an overmastering personal feeling to which I trust I may be permitted to give expression. It was these green hills and grey crags that gave me in boyhood the impulse that has furnished the work and joy of my life. To them, amid changes of scene and surroundings, my heart ever fondly turns, and here I desire gratefully to acknowledge that it is to their influence that I am indebted for any claim I may possess to stand in the proud position in which your choice has placed me.

# ADDRESS

## TO THE

# MATHEMATICAL AND PHYSICAL SECTION

### OF THE

# BRITISH ASSOCIATION,

### BY

## Professor ARTHUR SCHUSTER, Ph.D., F.R.S., F.R.A.S.

### PRESIDENT OF THE SECTION.

In opening the proceedings of our Annual Meeting the temptation is great to look back on the year which has passed and to select for special consideration such work published during its course as may seem to be of the greatest importance. I fear, however, that a year is too short a time to allow us to form a fair estimate of the value of a scientific investigation. The mushroom, which shoots up quickly, only to disappear again. impresses us more than the slow-growing seedling which will live to be a tree, and it is difficult to recognise the scientific fungus in its early stage But, although I do not feel competent to give you a review of the progress made in our subject during the last twelve months, there is one event to which some allusion should be made. It has been the sad duty of many of my predecessors to announce the death of successful workers in the field of science, but I believe I am unique in having the pleasure of recording the birth of a scientific man. At the beginning of this year there came into the world a being so brilliant that he could, without preparation, take up the work of the most eminent man amongst us. Believers in the transmigration of souls have speculated on the fact that Galileo's death and Newton's birth fell within a year of each other; but no event has ever happened so striking as that which took place on the 1st of January, when the mantle of Sir William Thomson fell on the infant Lord Kelvin. Those who have attended these meetings will feel with me that the honour done to our foremost representative, an honour which has been a source of pride and satisfaction to every student of science, could not altogether remain unnoticed in the section which owes him so much.

We are chiefly concerned here with the increase of scientific knowledge, and we derive pleasure in contrasting the minor state of ignorance of our own time with that which prevailed a hundred years ago. But when we contrast at the same time the refined opportunities of a modern research laboratory with the crude conditions under which the experimentalist had to work at the beginning of the century, we may fairly ask ourselves whether it is possible by means of any systematic course of study or by means of any organisation to accelerate our progress into the dark continent of science. A number of serious considerations arise in connection with this subject, and though I am not going to weary you by attempting an exhaustive discussion, I should like to draw your attention to a few matters which seem to me to be well worthy of the consideration of this Association. Changes are constantly made and proposed in our existing institutions, or new ones are suggested which are to serve the purpose of a more rapid accumulation of knowledge. I need only allude to the alterations in the curriculum of the science schools in our old Universities, made partly for the purpose of fitting their graduates for the conduct of original research, or to the national laboratory proposed by my predecessor in this chair for carrying out a certain kind of scien-

tific investigation, which at present is left undone, or is done by private enterprise. Even our own Association has not escaped the evil eye of the reformer, and, like other institutions, it may be capable of improvement. But in choosing the direction in which a change may best be made, I think we may learn something from the way in which Nature improves its organisms. We are taught by biologists that natural selection acts by developing those qualities which enable each species best to survive the struggle for existence; useless organs die off or become rudimentary. Nature teaches us, therefore, how a beautiful complex of beings, mutually dependent on each other, is formed by improving those parts which are best and most useful, and letting the rest take care of itself. But in many of the changes which have been made or are proposed the process of reform is very different. The weakest points are selected, our attention is drawn to some failure or something in which we are excelled by other nations, and attempts are made to cure what perhaps had better be left to become rudimentary. The proceeding is not objectionable as long as the nourishment which is applied to develop the weaker organs is not taken from those parts which we should specially take care to preserve. To apply these reflections to the question with which we are specially concerned, I should like to see it more generally recognised that although there is no struggle for existence between different nations, yet each nation, owing to a number of circumstances, possesses its own peculiarities, which render it better fitted than its neighbours to do some particular part of the work on which the progress of science depends. No country, for instance, has rivalled France in the domain of accurate measurement, with which the names of Regnault and Amagat are associated, and the International Bureau of Weights and Measures has its fitting home in Paris.[1] The best work of the German Universities seems to me to consist in the following up of some theory to its logical conclusions and submitting it to the test of experiment. I doubt whether the efforts to transplant the research work of German Universities into this country will prove successful. Does it not seem well to let each country take that share of work for which the natural growth of its character and its educational establishment best adapt it? Is it wise to remedy some weak point, to fill up undoubted gaps, if the soil that fills the gaps has to be taken from the hills and elevations which rise above the surrounding level?

As far as the work of this section is concerned the strongest domain of this country has been that of mathematical physics. But it is not to this that I wish specially to refer. Look at the work done in Great Britain during the last two centuries; the work not only in physics, but in astronomy, chemistry, biology. Is it not true that the one distinctive feature which separates this from all other countries in the world is the prominent part played by the scientific amateur, and is it not also true that our modern system of education tends to destroy the amateur?

By amateur I do not necessarily mean a man who has other occupations and only takes up science in his leisure hours, but rather one who has had no academical training, at any rate in that branch of knowledge which he finally selects for study. He has probably been brought up for some profession unconnected with science, and only begins his study when his mind is sufficiently developed to form an entirely unbiassed opinion. We may, perhaps, best define an amateur as one who learns his science as he wants it and when he wants it. I should call Faraday an amateur. He would have been impossible in another country; perhaps he would be impossible in the days of the Science and Art Department. Other names will occur to you, the most typical and eminent being that of Joule. It is not my purpose to discuss why distinguished amateurs have been so numerous in this country, but I am anxious to point out that we are in danger of losing one great and necessary factor in the origination of scientific ideas.

[1] Much of the good work done by this Bureau remains unknown, owing to the miserly way in which their publications are circulated. No copies are supplied even to the University libraries. The explanation, of course, is 'want of funds.' In other words, England, France, and Germany, together with other nations, unite to do a certain kind of work, but cannot afford to distribute a few copies of the publication to the public for whose benefit the work is undertaken.

One of the distinctive features of an amateur is this, that he carries not the weight of theories, often not the weight of knowledge, and, if I am right, there is a distinct advantage in having one section of scientific men beginning their work untrammelled by preconceived notions, which a systematic training in science is bound to instil. Whatever is taught in early age must necessarily be taught in a more or less dogmatic manner, and, in whatever way it is taught, experience shows that it is nearly always received in a dogmatic spirit. It seems important, therefore, to confine the early training to those subjects in which preconceived notions are considered an advantage. It is to me an uncongenial task to sound a note of warning to our old Universities, for the chief difficulties in which they are placed at present are due to the fact that they have given way too much to outside advice; but I cannot help expressing a strong conviction that their highly specialised entrance examinations are a curse to all sound school education, and will prove a still more fatal curse to what concerns us most nearly, the progress of scientific knowledge. If school examinations could be more general, if scientific theories could only be taught at an age when a man is able to form an independent judgment, there might be some hope of retaining that originality of ideas which has been a distinctive feature of this country, and enabled our amateurs to hold a prominent position in the history of science. At present a knowledge of scientific theories seems to me to kill all knowledge of scientific facts.

It is by no means true that a complete knowledge of everything that has a bearing on a particular subject is always necessary to success in an original investigation. In many cases such knowledge is essential, in others it is a hindrance. Different types of men incline to different types of research, and it is well to preserve the dual struggle. The engine which works out the great problems of nature may be likened to a thermodynamic machine. The amateur supplies the steam and the Universities' supply the cold water; the former, boiling over often with ill-considered and fanciful ideas, does not like the icy douche, and the professional scientist does not like the latent heat of the condensing steam, but nevertheless the hotter the steam and the colder the water the better works the machine. Sometimes it happens that boiler and cooler are both contained in the same brain, and each country can boast of a few such in a century, but most of us have to remain satisfied with forming only an incomplete part of the engine of research.

But while it is necessary to recognise the great work done by the unprofessional scientists, it seems not untimely to draw their attention to the damage done to themselves if they overstep their legitimate boundaries, and especially if they seek popular support for their theories, which have not received the approval of those who are competent to judge. An appeal from Alexander sober to Alexander drunk will not prove successful in the end.

The gradual disappearance of the amateur may be a necessary consequence of our increased educational facilities, and we must inquire whether any marked advantages are offered to us in exchange There is one direction in which it would seem at first sight, at any rate, that a proper course of study could do much to facilitate the progress of research.

On another occasion I pointed out that two parties are necessary for every advance in science, the one that makes it and the one that believes in it. If the discoverer is born, and cannot be made, would it not be possible at any rate to train the judgment of our students so that they may form a sound opinion on the new theories and ideas which are presented to them? It is too early as yet to judge in how far our generation is better in this respect than the one that has gone before them, but on closer examination it does not seem to me to be obvious that any marked improvement is possible. Every new idea revolutionising our opinions on some important question must necessarily take time before it takes a proper hold on the scientific world. Is it not true that anyone who can at once see the full importance of a new theory, and accept it in place of the one in which he has been brought up, must stand at a height almost equal to that of the originator? The more startling and fresh the new conception the fewer must be those who are ready to adopt it. But looking back at the history of science during the present century, is there much evidence that great discoveries have been seriously delayed

by want of proper appreciation ?    We may hear of cases where important papers
have been rejected by scientific societies, and occasionally a man of novel ideas
may have been too much neglected by his contemporaries.    I doubt whether such
cases of apparent injustice can ever be avoided, and, simply looking back on the
great changes involved in matters of primary importance, such as the undulatory
theory of light, the conservation of energy, and the second law of thermo-
dynamics, I cannot admit that there is much reason to be dissatisfied with
the rate at which new theories have been received. Those who experience a tempo-
rary check, owing to the fact that public opinion is not ripe for their ideas,
are often amply rewarded after the lapse of a few years    The disappointment
which Joule may have felt during the time his views met with adverse criticisms
from the official world of science was no doubt amply compensated by the pleasure
with which he watched the subsequent progress of research in the new domain
which his discoveries have opened out.

The point is not one of academic interest only, for the fear of repressing some
important new discovery has a detrimental influence in another direction.    The
judgment of the scientific world seems to me to be tending too much towards
leniency to apparently absurd theories, because there is a remote chance that they
may contain some germ of real value.    A new truth will not be found to suffer
ultimately by adverse and even unreasonable criticism, while bad theories and bad
reasoning, supported by the benevolent neutrality of those to whose judgment the
scientific world looks for guidance, are harmful in many ways.    They block the
way to an independent advance and encourage hasty and ill-considered generalisa-
tions.    The conclusion I should draw from the considerations I have placed before
you are these: I believe that a reasonable censorship exercised by our scientific
societies is good and necessary ; that those whose fate it is to be called on to
express an opinion on some work or theory should do so fearlessly according to
their best judgment.    Their opinion may be warped by prejudice, but I think it is
better that they should incur the risk of being ultimately found to be wrong than
that they should help in the propagation of bad reasoning.    There is one matter,
however, on which all opinions must agree.    Worse than bad theory or logic is
bad experimental work.    Should we then not rigorously preserve any influence
or incentive which encourages the beginner to avoid carelessness and to consider
neither time nor trouble to secure accuracy ?    There is no doubt to my mind that
the prospect of admission to the Royal Society has been most beneficial in this
respect, and that the honourable ambition to see his paper published in the
'Transactions ' of that Society has preserved many a student from the premature
publication of unfinished work.

One of the principal obstacles to the rapid diffusion of a new idea lies in the
difficulty of finding suitable expressions to convey its essential point to other minds.
Words may have to be strained into a new sense, and scientific controversies
constantly resolve themselves into differences about the meaning of words.    On
the other hand, a happy nomenclature has sometimes been more powerful than
rigorous logic in allowing a new train of thought to be quickly and generally
accepted.

A good example is furnished by the history of the science of energy.    The
principle of the conservation of energy has undoubtedly gained a more rapid and
general acceptance than it would otherwise have had by the introduction of the
word potential energy.    A great theorem, which in itself seems to me to be
an intricate one, has been simplified by calling something energy which, in the
first place, is only a deficiency of kinetic energy.    The only record I can find on
the history of the expression is given in Tait's 'Thermodynamics,' wherein the
term statical energy is ascribed to Lord Kelvin, and that of potential energy to
Rankine.    It would be of interest to have a more detailed account on the origin of
an expression which has undoubtedly had a marked influence not only on the
physics, but also on the metaphysics of our time.    But while fully recognising the
very great advantage we have derived from this term 'Potential Energy,' we ought
not, at the same time, to lose sight of the fact that it implies something more than
can be said to be proved.    It is easy to overstep the legitimate use of the word.

Thus, when Professor Lodge[1] attempts to prove that action at a distance is not consistent with the doctrine of energy, he cannot, in my opinion, justify his position except by assuming that all energy is ultimately kinetic. That is a plausible but by no means a necessary theory. Efforts have been made to look on energy as on something which can be labelled and identified through its various transformations. Thus we may feel a certain bit of energy radiating from a coal-fire, and if our knowledge was complete, we ought to be able to fix the time at which that identical bit of energy left the sun and arrived on the surface of the earth, setting up a chemical action in the leaves of the plant from which the coal has been derived. If we push this view to a logical conclusion, it seems to me that we must finally arrive at an atomic conception of energy which some may consider an absurdity.

Let, for instance, a number of particles $P_1$, $P_2$, &c , in succession, strike another particle Q. How can we in the translatory energy of the latter identify the parts which $P_1$, $P_2$, &c , have contributed ? According to Professor Lodge's view, we should be able to do so, for if the particle Q in its turn gives up its energy to others, say $R_1$, $R_2$, $R_3$, &c., we ought to be able to say whether the energy of $P_1$ has ultimately gone into $R_1$ or into $R_2$, or is divided between them. It is only by imagining that all energy is made up of a finite number of bits, which pass from one body to another, that we can defend the idea of considering energy as capable of being 'labelled.'

In the expressions we adopt to describe physical phenomena we necessarily hover between two extremes. We either have to choose a word which implies more than we can prove, or we have to use vague and general terms which hide the essential point, instead of bringing it out. The history of electrical theories furnishes a good example The terms positive and negative electricity committed us to something definite, we could reckon about quantities of electricity, and form some definite notion of electrical currents as a motion of the two kinds of electricity in opposite directions. Now we have changed all that ; we speak of electric displacements, but safeguard ourselves by saying that a displacement only means a vector quantity, and not necessarily an actual displacement. We speak of lines and tubes of force not only as a help to realise more clearly certain analytical results, but as implying a physical theory to which, at the same time, we do not wish to commit ourselves. I do not find any fault with this, for it is a perfectly legitimate and necessary process to state the known connection between physical phenomena in some form which introduces the smallest number of assumptions. But the great question ' What is electricity ? ' is not touched by these general considerations. The brilliant success with which Maxwell's investigations have been crowned is apt to make us overrate the progress made in the solution of that question. Maxwell and his followers have proved the important fact that optical and electrical actions are transmitted through the same medium. We may be said to have arrived in the subject of electricity at the stage in which optics was placed before Young and Fresnel hit on the idea of transverse vibrations, but there is no theory of electricity in the sense in which there is an elastic solid theory of light.

If the term electrical displacement was taken in its literal sense, it would mean that the electric current consists of the motion of the ether through the conductor. This is a plausible hypothesis. and one respecting which we may obtain experimental evidence. The experiments of Rayleigh and others have shown that the velocity of light in an electrolyte, through which an electric current is passing, is, within experimental limits, the same with and against the current. This result shows that if an electrical current means a motion of the ether the velocity of the medium cannot exceed ten metres a second for a current density of one ampère per square centimetre. This, then, is the upper limit for a possible velocity of the medium ; can we find a lower limit ? The answer to that question depends on the interpretation of a well-known experiment of Fizeau's, who found that the speed of light is increased if it travels through water which moves in the same direction as the

[1] *Phil. Mag.* vol. xi. p 36 (1881)

A 2

light.   If this experiment implies that the water carries the ether with it, and if a motion of the ether means an electric current, we should be led to the conclusion that a current of water should deflect a magnet in its neighbourhood.   An experiment made to that effect would almost certainly give a negative result, and would give us a lower limit for the velocity of the medium corresponding to a given current.   Such an experiment, together with that of Rayleigh, would probably dispose of the theory that an electric current is due to a translatory velocity of the medium.   This would be an important step, and it would be worth while to arrive at a final settlement of the question.[1]   The whole question of the relation between the motion of matter and motion of the medium is a vital one, and we shall probably not make any serious advances until experiment has found a new opening.   But we must expect many negative results before some clue is discovered.   Nor can we attach much importance to negative results unless they are made by some one in whose care and judgment we place full reliance.   We should all the more, therefore, recognise the courage and perseverance of those who spend their valuable time in such investigations as Professor Lodge has recently undertaken.   That ultimately some relation will be found between moving matter and electrical action there is no reasonable doubt.

One of the most hopeful openings for new investigations has always been found in the pursuing of a theory to its logical conclusions, and there is one result of the electromagnetic theory of light which has not, in my opinion, received the share of attention which it deserves.

When sound passes through air it is propagated more quickly with the wind than against it, and we may easily find the velocity relative to the earth by combining the ordinary sound velocity with the velocity of the wind.   Similarly, when any waves pass through a medium moving with uniform velocity, the waves being due to internal stresses in the medium, we may treat of the velocity of the waves independently of that of the medium, and say that the wave-velocity in the direction of motion of the medium, and relative to a fixed body, is the sum of the wave-velocity calculated on the supposition that the medium is at rest and the velocity of the medium.   Professor J. J. Thomson,[2] applying Maxwell's equations, has arrived at a different result for electromagnetic waves, and has come to the conclusion that in order to get the velocity of light along a stream of flowing water we have to add to the velocity of light only half the velocity of water.   The following considerations suggest themselves to me with respect to this result.   Maxwell's theory is founded on certain observed effects, which all depend on the relative motion of matter.   A result such as the one referred to implies actions depending on absolute motion, and appears therefore to point to something which has been introduced into the equations for which there is no experimental evidence.   The only assumption clearly put down by Maxwell is that electromagnetic actions are transmitted through the medium, and it is possible that that assumption necessarily carries Professor J. J. Thomson's result with it.   If a careful examination of the subject should show that this is the case, we are brought face to face with a serious difficulty.   It is said, with justice, to be one of the great advantages of Maxwell's theory that it does away with action at a distance ; but what do we gain if we replace action at a distance by something infinitely more difficult to conceive, namely, internal stresses of a medium depending on the velocity of the medium through space ?   I can only see one loophole through which to escape, namely, that Maxwell's medium is not homogeneous, but consists of two parts, and that if we speak of the medium as moving, we mean the motion of one of these parts relative to the other.

While we may hope to obtain important results from an investigation of the

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[1] Fizeau's result must either be due to the motion of matter through the medium or to the fact that moving matter carries the ether with it.   If it is due to the former cause, and matter does *not* carry the ether with it, may we not consider that matter moving through the ether, that is a relative motion of matter and ether, must produce effects equal and opposite to those of ether moving through matter ?   In that case the reasoning in the text would, *mutatis mutandis*, hold good.

[2] *Phil. Mag.* vol. ix. p. 284 (1880).

relation between what we call electricity and the medium, we must not lose sight of another avenue, namely, the relation between electricity and chemical effects. The passage of electricity through gases presents us with a complicated problem to which a number of physicists have given their attention of late years. There seems no reasonable doubt that electricity in a gas is conveyed by the diffusion of particles conveying high charges, probably identical with those carried by the electrolytic ion. The fact that this convection is a process of diffusion with comparatively small velocity is shown by the experimental result that the path of the discharge is affected by any bodily motion of the gas which conveys the current. Even the convection currents due to the heat produced by the discharge itself are sufficient to deflect the luminous column which marks the passage of the current.

The most puzzling fact, however, connected with the discharge of electricity through gases consists in the absence of symmetry at the positive and negative poles. There must be some difference between a positively and negatively charged atom which seems of fundamental importance in the relation between matter and what we call electricity. A discussion of the various phenomena attending the discharge of electricity through gases seems to me to point to a conclusion which may possibly prove a step in the right direction.

A surface of separation between bodies having different conductivities becomes electrified by the passage of a current, while at the surface between two chemically distinct bodies we have, according to Helmholtz, a sheet covered at the two sides with opposite electricities. These surface electrifications are not merely imaginary layers invented to satisfy mathematical surface conditions. They can be proved to be realities. Thus, when one electrolyte floats on another, the specific resistances being different, we often observe secondary chemical effects due to the action of the ions which carry the surface electrification.

If the passage of electricity from the solid to the gas involves some work done, we must expect a double sheet of electricity at the boundary, the gas in contact with the kathode becoming positively, and that in contact with the anode negatively, electrified. *A priori* we can form no idea how a layer of gas, the atoms of which carry charges, will behave. The ordinary proof that all electrification must be confined to the surface implies that all forces act according to the law of the inverse square, but where we have also to consider molecular forces, I see no reason why the electrification at a surface may not stretch across a layer having a thickness comparable with the mean free path of the molecule. It is here that there seems to be the fundamental difference between positive and negative electricity. A negative electrification of the gas, like that of a solid or a liquid, seems always confined to the surface, and no one has ever observed a volume electrification of negative electricity. The case is different for the positively electrified part of the gas. Wherever from other considerations we should expect a positively electrified surface sheet, we always get a layer of finite thickness. The result implies a different law of impact between positively and negatively electrified ions, but I see no inherent improbability in this. That the kathode let into a gas is surrounded by a positively electrified layer of finite thickness extending outwards must be considered as an established fact, and several of the characteristic features of the discharge are explained by it. The large fall of potential at the kathode can also be explained on the view which I have put forward, for in order to keep up the discharge there must be a sufficient normal force at the surface, and if this force is not confined to the surface, but necessarily stretches across a finite layer, the fall of potential must be multiplied a great number of times. Similarly Goldstein has shown that some of the phenomena of the kathode are observed at every place at which the positive current flows from a wide to a narrow part of a column of gas. At such places we should expect a positive surface electrification, and here, again, the whole appearance tends to show that we are dealing with a positive volume electrification. No corresponding phenomena are observed when the current passes from the narrow to the wide part.

The fact that in all cases experimented upon positive volume electrifications are observed but never similar negative electrifications is surely of significance.

Some of the results recently brought to light by investigations on the dis-

charge of electricity have interesting cosmical applications. Thus it is found that such a discharge through any part of a vessel containing a gas converts the whole gas into a conductor.[1] The dissociation which we imagine to take place in a liquid before electrolytic conduction takes place must be artificially produced in a gas by the discharge itself. We may imitate in gases which have thus been rendered conductive many of the phenomena hitherto restricted to liquids: thus I hope to bring to the notice of this meeting cases of primary and secondary cells in which the electrolyte is a gas    There are other ways in which a gas can be put into that sensitive state in which we may treat it as a conductor, and we have every reason to suppose that the upper regions of our atmosphere are in this state. The principal part of the daily variation of the magnetic needle is due to causes lying outside the surface of the earth, and is in all probability only an electro-magnetic effect due to that bodily motion in our atmosphere which shows itself in the diurnal changes of the barometer. A favourite idea of the late Professor Balfour Stewart will thus probably be confirmed. The difference in the diurnal range between times of maximum and times of minimum sun-spots is accounted for by the fact that the atmosphere is a better conductor at times of maximum sun-spots.

The mention of sun-spots raises a point not altogether new to this section. Careful observations of celestial phenomena may suggest to us the solution of many mysteries which are now puzzling us. Consider, for instance, how long it would have taken to prove the universal property of gravitational attraction if the record of planetary motion had not come to the philosopher's help. And surely the most casual observation of cosmical effects teaches us how much we have yet to learn.

The statement of a problem occasionally helps to clear it up, and I may be allowed, therefore, to put before you some questions, the solution of which seems not beyond the reach of our powers.

1. Is every large rotating mass a magnet? If it is, the sun must be a powerful magnet. The comets' tails, which eclipse observations show stretching out from our sun in all directions, probably consist of electric discharges. The effect of a magnet on the discharge is known, and careful investigations of the streamers of the solar corona ought to give an answer to the question which I have put [2]

2. Is there sufficient matter in interplanetary space to make it a conductor of electricity? I believe the evidence to be in favour of that view. But the conductivity can only be small, for otherwise the earth would gradually set itself to revolve about its magnetic pole. Suppose the electric resistance of interplanetary space to be so great that no appreciable change in the earth's axis of rotation could have taken place within historical times, is it not possible that the currents induced in planetary space by the earth's revolution may, by their electromagnetic action, cause the secular variation of terrestrial magnetism? There seems to me to be here a definite question capable of a definite answer, and as far as I can judge without a strict mathematical investigation the answer is in the affirmative.

3. What is a sunspot? It is, I believe, generally assumed that it is analogous to one of our cyclones. The general appearance of a sunspot does not show any marked cyclonic motion, though what we see is really determined by the distribution of temperature and not by the lines of flow. But a number of cyclones clustering together like the sunspots in a group should move round each other in a definite way, and it seems to me that the close study of the relative positions of a group of spots should give decisive evidence for or against the cyclone theory.

4. If the spot is not due to cyclonic motion, is it not possible that electric discharges setting out from the sun, and accelerating artificially evaporation at the sun's surface, might cool those parts from which the discharge starts, and thus

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[1] An experiment by Hittorf (*Wied. Ann*, vii, p 614) suggested the probability of this fact, which was proved independently by Arrhenius and myself.

[2] The efforts of Mr. Bigelow have a bearing on this point, also some remarks which I have made in a lecture before the Royal Institution (*Proc. Roy. Inst.* 1891), but nothing decisive can be asserted at present.

produce a sunspot?  The effects of electric discharges on matters of solar physics have already been discussed by Dr. Huggins

5. May not the periodicity of sunspots, and the connection between two such dissimilar phenomena as spots on the sun and magnetic disturbances on the earth, be due to a periodically recurring increase in the electric conductivity of the parts of space surrounding the sun?  Such an increase of conductivity might be produced by meteoric matter circulating round the sun.

6. What causes the anomalous law of rotation of the solar photosphere?  It has long been known that groups of spots at the solar equator perform their revolution in a shorter time than those in a higher latitude; but spots are disturbances which may have their own proper motions.  Duner [1] has shown, however, from the displacement of the Fraunhofer lines, that the whole of the layer which produces these lines follows the same anomalous law, the angular velocity at a latitude of 75° being 30 per cent. less than near the equator.[2]  As all causes acting within the sun might cause the angular velocity of the sun to be smaller at the equator than at other latitudes, but could not make it greater, the only explanation open to us is an outside effect either by an influx of meteoric matter, as suggested by Lord Kelvin, or in some other way.  If we are to trust Dr. Welsing's result that faculæ which have their seat below the photosphere revolve in all latitudes with the same velocity, which is that of the spot velocity in the equatorial region, we should have to find a cause for a retardation in higher latitudes rather than for an acceleration at the equator.  The exceptional behaviour of the solar surface seems to me to deserve very careful attention from solar physicists.  Its explanation will probably carry with it that of many other phenomena.

In conclusion, I should like to return for an instant to the question whether it is possible by any means to render the progress of science more smooth and swift.  If there is any truth in the idea that two types of mind are necessary, the one corresponding to the boiler and the other to the cooler of a steam engine, it must also be true that some place must be found where the two may bring their influence to bear on each other.  I venture to think that no better ground can be chosen than that supplied by our meetings  We hear it said that the British Association has fulfilled its object; we are told that it was originally founded to create a general interest in scientific problems in the towns in which it meets; and now that popular lectures and popular literature are supposed to perform that work more satisfactorily, we are politely asked to commit the happy despatch.  There is no need to go back to the original intention of those who have founded this institution, which has at any rate adapted itself sufficiently well to the altered circumstances to maintain a beneficial influence in scientific research.

The free discussion which takes place in our sections, the interchange of ideas between men who during the rest of the year have occupied their minds, perhaps too much, with some special problem, the personal intercourse between those who are beginning their work with sanguine expectations, and those who have lost the first freshness of their enthusiasm, should surely one and all ensure a long prosperity to our meetings.  If we cannot claim any longer to sow the seeds of scientific interest in the towns we visit, because the interest is established, we can at any rate assure those who so kindly offer us hospitality that they are helping powerfully in the promotion of the great object which we all have at heart.

[1] Oefvers af Kongl. Veterik. Ak. Forhandl., 47, 1890.

[2] Although the importance of M Duner's results would make an independent investigation desirable, the measurements of Mr. Crew, who by a much inferior method arrived at other results, cannot have much weight as compared with those of Duner.

# ADDRESS

## TO THE

# CHEMICAL SECTION

### OF THE

# BRITISH ASSOCIATION.

### BY

#### PROFESSOR HERBERT McLEOD, F.R.S., F.C.S.

##### PRESIDENT OF THE SECTION.

IN endeavouring to prepare myself to properly fulfil the duties of President of this Section, to which I have been elected, and for which honour I am much indebted to the council and members of the Association (although I am only too well aware that the position might have been more efficiently filled by many others), I naturally looked at the reports of the previous meetings held in Edinburgh in 1834, 1850, and 1871, and it appears that on the first two occasions an address was not given by the president, a custom the discontinuance of which I have, at the present moment, much reason to regret.

At the meeting in 1834 a committee was appointed consisting of Dr. Dalton, Dr. Hope, Dr. T. Thomson, Mr. Whewell, Dr. Turner, Professor Miller, Dr. Gregory, Dr. Christison, Mr. R. Phillips, Mr. Graham, Professor Johnston, Dr. Faraday, Professor Daniell, Dr. Clark, Professor Cumming, and Dr. Prout, to report at the next meeting their opinion on the adoption of an uniform set of chemical symbols. Dr. Turner to be secretary.

In the following year the report contains: 'Report of the Committee on Chemical Notation. Dr. Turner, the chairman of the committee appointed to take into consideration the adoption of an uniform system of chemical notation, made a report to the following effect:—

' 1. That the majority of the Committee concur in approving of the employment of that system of notation which is already in general use on the Continent, though there exists among them some difference of opinion on points of detail.

' 2. That they think it desirable not to deviate in the manner of notation from algebraic usage except so far as convenience requires.

' 3. That they are of opinion that it would save much confusion if every chemist would always state explicitly the exact *quantities* which he intends to represent by his symbols.

Dr. Dalton stated to the Chemical Section his reasons for preferring the symbols which he had himself used from the commencement of the atomic theory in 1803, to the Berzelian system of notation subsequently introduced. In his opinion regard must be had to the arrangement and equilibrium of the atoms (especially elastic atoms) in every compound atom, as well as to their number and weights. A system either of *arrangements* without *weights,* or of *weights* without *arrangements,* he considered only half of what it should be.'

1892.                                                                                              B

We can all sympathise with the members of the section of 1834 in their desire to obtain a uniform system of chemical notation, for at that time several very different systems seem to have been in use. Although the report is a short one, it probably directed the attention of chemists to the desirability of avoiding confusion by the use of various systems, and since that period many advances have been made.

There is now little necessity for every chemist to 'state explicitly the exact *quantities* which he intends to represent by his symbols' for the accurate determinations of atomic weights by many chemists—and we must not omit to mention the work of Stas (whose death we have had to deplore since the last meeting of the British Association)—have given us a series of numbers which are in the hands of all chemists, so that, except in the cases where great refinement is requisite (or when the atomic weight has not been universally accepted) there is no need to state the values of the symbols.

That great advances have been made in chemical notation is well known to all; even in my own short experience I have had to learn several different methods. When I began to work at chemistry I was told that sulphate of lead was to be expressed by the formula $PbO,SO_3$. Hofmann taught me that it should be $PbSO_4$; then Gerhardt doubled the atomic weights of oxygen and sulphur and the formula became $Pb_2SO_4$; Cannizzaro showed that the atomic weight of lead should also be doubled, and the formula again became $PbSO_4$, but representing twice as much as formerly; then Frankland taught me to write $SO_2Pbo''$ as the expression of the graphic formula—

$$\overset{'}{\underset{O}{\Big\rangle}} S \Big\langle \overset{}{\underset{O}{\Big\rangle}} Pb,$$
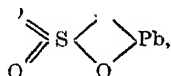
which not only states that the compound contains 207 of lead, 32 of sulphur, and 64 of oxygen, but that the sulphur is hexad, and is combined with two atoms of dyad oxygen, and with a dyad compound radical containing one atom of lead and two of oxygen; and of all the formulæ just given this is the only one which satisfies the requirements which Dalton thought necessary in 1835, namely, to indicate not only the weights of the elements present, but also their arrangement. It may be objected that we do not know that this formula really represents the arrangements of the atoms in plumbic sulphate, but there can be very little doubt that the four atoms of oxygen in the compound are not all in the same condition, for if we examine the properties of sulphuric acid (from which the sulphate of lead is derived by the replacement of the hydrogen by lead), we find that two of the atoms of oxygen are more closely associated with the hydrogen than are the other two, and as there is some evidence, although perhaps not very conclusive, that sulphur may be capable of combining with six monad atoms, although no such compound is yet known, it does not seem unreasonable to suppose that sulphuric acid is really—

$$\overset{O}{\underset{}{\Big\rangle}} S \Big\langle \overset{O---H}{\underset{O---H}{}}$$
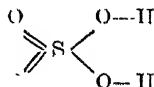
What the nature of the attraction that holds the atoms together may be is not known, but it is more probably of a character similar to that of gravity which holds together sun and planets, than of the nature of cohesion which would hold the atoms rigidly together; the atoms in each molecule are therefore most probably in a state of rotation around, or of vibration to and from, the central atom which holds them together. The pictorial representation in a plane does not therefore truly express the position of the atoms, but merely the relations existing between them In organic chemistry the use of formulæ expressing such a relation has

become indispensable, and in inorganic chemistry I believe such a system is very useful.

Recently this system has been found insufficient for the requirements of organic chemistry, and recourse has been had to the figure of a tetrahedron to represent the atom of carbon, other atoms being attached to the solid angles; in this way the position of the atoms in space is more or less expressed.

There are many cases, however, in which the atomicity theory fails us. At first it seemed probable that the atomicity of an element varied in pairs of attractions, that is, an element might be monad, triad, or pentad, but not dyad or tetrad; or it might be dyad, tetrad, or hexad, but not triad or pentad; but some great difficulties have been encountered. Thus nitrogen, which is pentad in ammonic chloride and triad in ammonia, forms the compound nitric oxide, NO, in which it would appear to be dyad; it has been suggested, however, that in this body the nitrogen is really triad, and that it possesses a 'free bond.' Now the idea of a 'free bond' seems contrary to the principles of atomicity, since it is on the belief that such a free bond is impossible that the explanation of the existence of elementary molecules is formed, for it is said that when hydrogen is liberated two atoms unite to form a molecule, so that their mutual attractions may be satisfied. Nevertheless nitric oxide is a very active body, uniting readily with other substances, so the free bond seems to be on the look out for other kinds of matter, but to have no attraction for the free bond of another molecule of nitric oxide. As the molecule of nitric peroxide is variable by alterations of temperature, being $N_2O_4$ at low and $NO_2$ at high temperatures, it seemed not impossible that at the ordinary atmospheric temperature nitric oxide was a simplified or dissociated molecule, and that if the temperature were sufficiently reduced it would be found that its molecule would be $N_2O_2$, and thus it would contain triad nitrogen without a free bond. The density of the gas has, however, been determined at a temperature as low as $-73°$ and the molecule is still NO. Another important exception to the variation of the atomicity of an element in pairs was furnished by the investigations of Sir Henry Roscoe on the chlorides of vanadium; this element which, from analogy, should be a triad or a pentad, appears to form a chloride of the composition $VCl_4$. Again, the molecule of peroxide of chlorine is $ClO_2$, which would make chlorine a tetrad or the compound must have a free bond.
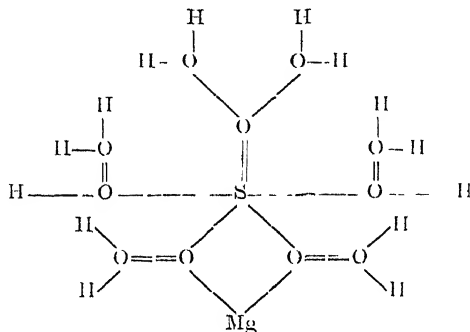
Another set of phenomena which the atomicity theory will not explain is the existence of well-defined crystalline salts containing what is called water of crystallisation. This water is in many cases held with considerable pertinacity, the body appearing to be a veritable chemical compound. But water appears to be a saturated body, the attractions of the oxygen being satisfied by those of the hydrogen. It is true that water acts vigorously on other compounds, as on metallic oxides to form hydrates, and on some anhydrides to form acids, but these appear to be phenomena of double decomposition; thus the combination of water with sodic oxide and nitric anhydride respectively may be expressed by the equations

$$OH_2 + ONa_2 = OHNa + ONaH \text{ and}$$
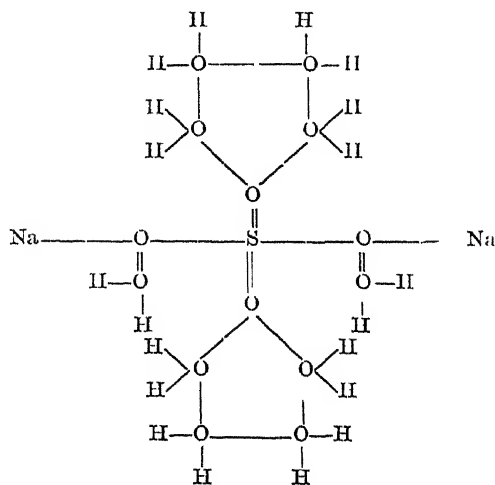$$OH_2 + O(NO_2)_2 = OH(NO_2) + O(NO_2)H.$$
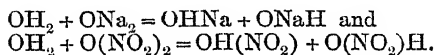
In the combination of water with an anhydrous salt, a phenomenon often accompanied by great rise of temperature, there does not appear to be a double decomposition. That there is a chemical combination of some sort is shown by the changes of properties produced, crystalline form and colour being both sometimes altered. Compounds so produced have been called 'molecular compounds' to imply that saturated molecules are in some way or another combined, the combination being different from 'atomic combination,' in which the atoms are directly united according to their valencies. Another explanation has been suggested by assuming that there is some 'residual affinity' not saturated by the constituents of the body, and that this residual affinity enables bodies to unite in a less stable manner than in most compounds. But are not these terms—'molecular combination' and 'residual affinity'—analogous to the term 'catalysis,' merely *words* to express—not to explain

—what we do not understand ?  If 'residual affinity' really exists, it must reside in the oxygen of the water, or in the hydrogen, or in both ; if so, what will happen to some of the complex constitutional formulæ of the organic chemist in which the carbon is tetrad, the oxygen dyad, and the hydrogen monad ?  If any of these elements have a residual affinity should we not expect to find additional unions between some of the atoms of the same molecule over and above those represented by the formula?
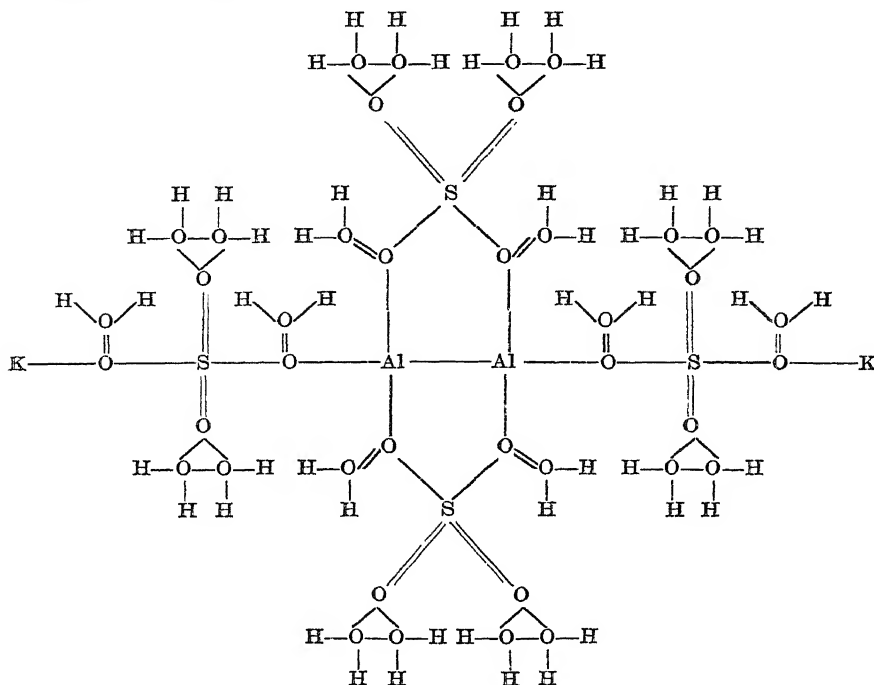
Oxygen may be tetrad, for which there is evidence in $OAg_4$.  Under these circumstances water is by no means a saturated compound, and there would be no difficulty in explaining the combination of water with oxygen salts.  Thus crystallised magnesic sulphate, $MgSO_4, 7OH_2$ or $SOHoMgo'', 6OH_2$ would be—

$$
\begin{array}{c}
\text{H} \qquad\qquad \text{H} \\
| \qquad\qquad\quad | \\
\text{H} - \text{O} \qquad\qquad \text{O} - \text{H} \\
\diagdown \qquad\quad \diagup \\
\text{O} \\
\text{H} \qquad\quad \| \qquad\quad \text{H} \\
| \qquad\qquad \qquad | \\
\text{H} - \text{O} \qquad\qquad\qquad \text{O} - \text{H} \\
\| \qquad\qquad\qquad\qquad \| \\
\text{H} \text{---} \text{O} \text{------} \text{S} \text{------} \text{O} \text{--} \text{H} \\
\text{H} \diagdown \qquad\qquad\qquad \diagup \text{H} \\
\text{O} == \text{O} \qquad\quad \text{O} == \text{O} \\
\text{H} \diagup \qquad\qquad\qquad \diagdown \text{H} \\
\diagdown \qquad\qquad \diagup \\
\text{Mg}
\end{array}
$$

and sodic sulphate, $Na_2SO_4, 10OH_2$ :—

$$
\begin{array}{c}
\text{H} \qquad\qquad \text{H} \\
| \qquad\qquad\quad | \\
\text{H} - \text{O} \text{----} \text{----} \text{O} - \text{H} \\
\text{H} \diagdown \qquad\qquad \diagup \text{H} \\
\text{O} \qquad\qquad \text{O} \\
\text{H} \diagup \qquad\qquad \diagdown \text{H} \\
\diagdown \qquad \diagup \\
\text{O} \\
\| \\
\text{Na} \text{------} \text{O} \text{------} \text{S} \text{------} \text{O} \text{------} \text{Na} \\
\| \qquad\quad \| \qquad\quad \| \\
\text{H} - \text{O} \qquad\quad \text{O} \qquad\quad \text{O} - \text{H} \\
| \qquad\qquad\qquad\qquad | \\
\text{H} \qquad\qquad\qquad\qquad \text{H} \\
\text{H} \diagdown \qquad\qquad\qquad \diagup \text{H} \\
\text{O} \qquad\qquad \text{O} \\
\text{H} \diagup \qquad\qquad\qquad \diagdown \text{H} \\
\text{H} - \text{O} \qquad\qquad \text{O} - \text{H} \\
| \qquad\qquad\qquad | \\
\text{H} \qquad\qquad\qquad \text{H}
\end{array}
$$

Even alum, with its 24 molecules of water of crystallisation, may be expressed by an appalling formula :—

$$
\begin{array}{c}
\text{H\ \ H}\qquad\qquad\text{H\ \ H}\\
\text{H—O—O—H\ \ H—O—O—H}\\
\text{O}\qquad\qquad\text{O}\\
\diagdown\qquad\diagup\\
\text{S}\\
\end{array}
$$

*(Graphic formula of alum showing K, S, Al, Al, S, K framework surrounded by 24 molecules of water of crystallisation)*

There is certainly a symmetry about the formula, and it will be found that 16 of the molecules of water are in a different position from the remaining 8; this probably has no significance, although Graham found that crystallised alum at a temperature of 61° lost 18 molecules of water; if he had used a temperature a few degrees lower he might have found that only 16 passed off!

By a little stretching of the imagination and altering the atomicities of the elements to suit each particular case, no doubt graphic formulæ might be made for all crystalline salts, but they would be perfectly artificial, and not much good is likely to come from the attempt.

I fear we are driven to the conclusion that notwithstanding all the progress that has been made in chemical science during the last fifty-eight years, we have not yet reached a method of notation that would have satisfied Dr. Dalton in 1834.

But since that time we have learnt that our formulæ ought to show even more than the number and position of the atoms of a compound; we should like them to indicate the amount of potential energy residing in a body, and our equations ought to indicate the amount of heat generated by a chemical change. Let us hope that before the next meeting of the British Association in Edinburgh these desirable developments will have been accomplished.

A short time ago I mentioned the word Catalysis as being employed to express certain chemical actions which cannot be explained. It is applied to those phenomena which take place in the presence of a body which appears to be entirely unchanged by the action. Happily these catalytic actions are being explained one after another, so that soon the name itself may become obsolete. An example of this action of presence may be given. When a mixture of sulphuric acid and alcohol is heated to a temperature of about 140° to 150°, ether passes over. Now

alcohol contains $C_2H_6O$, and if from two molecules of alcohol one molecule of water is subtracted a molecule of ether results :—$2C_2H_6O = OH_2 + C_4H_{10}O$. As sulphuric acid is known to have a great attraction for water, it is easy to imagine that the acid combines with the water and ether passes off. But it is found that a small quantity of sulphuric acid at the temperature of 140°–150° will transform a very large amount of alcohol into ether and water, much more than can be explained by assuming that the acid has combined with the water. If a mixture of sulphuric acid and alcohol is heated to a temperature of 140°–150°, and alcohol allowed to flow into the liquid, a mixture of ether and water vapours passes over, and after a large quantity of alcohol has been transformed, the amount of sulphuric acid is found to be unaltered. At first glance this seems very difficult to explain, but on further investigation it is found that alcohol and sulphuric acid act one on another to form ethyl-sulphuric or sulphovinic acid :—

$$SO_2Ho_2 + EtHo = SO_2HoEto + OH_2$$

but when ethyl-sulphuric acid is heated with alcohol, ether is formed with the reproduction of sulphuric acid—

$$SO_2HoEto + EtHo = OEt_2 + SO_2Ho_2$$
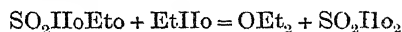
the sulphuric acid is then able to produce ethyl-sulphuric acid by acting on more alcohol, so a continuous production of ether and water takes place without loss of sulphuric acid. Another well-known action is the combination of oxygen and hydrogen under the influence of spongy platinum. In this case the platinum remains apparently unaltered, and is capable of causing the combination of any quantity of mixed gases. As spongy platinum possesses the power of absorbing large quantities of gases, it is usually said that the molecules of oxygen and hydrogen are so much condensed in the platinum that they are brought within the sphere of one another's attractions, and consequently combine

Another instance of an action of this kind is afforded by the oxidation of ammonia in the presence of chromic oxide. When ammonic dichromate is heated an evolution of gas occurs, and a residue of chromic oxide is left which bears a striking resemblance to a mixture of black and green tea ; when some of this substance is placed on a piece of wire gauze, heated and then supported over a vessel containing a strong solution of ammonia, the oxide glows, in a manner similar to the glowing of spongy platinum under the influence of a mixture of hydrogen and air. Under these conditions the chromic oxide facilitates the oxidation of the ammonia, but it becomes changed during the process, instead of having the appearance above described it acquires a bright-green colour. Now, we know that chromium is capable of forming several combinations with oxygen. Is it therefore too much to suppose that the chromium is alternately oxidised by the oxygen of the air, and reduced by the hydrogen of the ammonia, so that although in the end it has the same composition as at the beginning, nevertheless it has been continuously decomposed and reproduced ? Now, may not a similar change take place during the action of spongy platinum on a mixture of hydrogen and oxygen ? The alteration of the platinum is very slight, but I believe I have observed a slight modification of the appearance of a fragment of spongy platinum, that was kept glowing by a small jet of purified hydrogen for some hours; the gas not being allowed to burn so as to heat the platinum to a very high temperature, the metal appears to be compacted and to be covered by minute spherules of glistening metal. Now, may not the platinum have entered into combination with one or other of the gases and been subsequently reduced ? If this is the true explanation then we have in this case a continuous series of chemical changes and the 'catalysis' is explained.

We all know the ease with which oxygen is obtained from potassic chlorate when heated with a small quantity of oxide of manganese: the quantity of peroxide is the same at the end of the process as at the beginning, and it may be used over and over again to assist in the decomposition of fresh potassic chlorate. The oxide

of manganese undergoes a molecular alteration; if a crystalline variety is employed, it is found, at the end of the process, to have been transformed into fine powder.

I hope I have proved to the satisfaction of my brother chemists that potassic permanganate is first formed and subsequently decomposed with the reproduction of manganese peroxide.

Oxide of cobalt possesses the remarkable property of decomposing solutions of hypochlorites at moderate temperatures with evolution of oxygen. For some time I have been endeavouring to find the explanation of the change, but hitherto without complete success. At first it seemed probable that an unstable cobaltate, analogous to a ferrate, was formed and decomposed at the temperature of the experiment. In fact oxygen is evolved when chlorine is passed through a boiling solution of sodic hydrate containing ferric hydrate in suspension. But no evidence of the existence of a cobaltate could be found. When a cobaltous salt is added to an alkaline solution of a hypochlorite, a black precipitate is formed which is usually stated to be cobaltic hydrate, $Co_2Ho_6$, but Vortmann has shown that, when a cobaltous salt is mixed with a solution of iodine in potassic iodide, and the liquid rendered alkaline by sodic hydrate, the precipitate formed at a temperature between 50° and 60° approaches in composition the dioxide of cobalt, $CoO_2$. He also found that the precipitate lost oxygen at the temperature of boiling water. I have repeated some of his experiments and can quite confirm them, although I have not obtained an oxide containing quite as much oxygen as his richest oxide. The oxides I prepared rapidly effected the decomposition of a solution of sodic hypochlorite, and that without undergoing any loss of oxygen themselves; in fact, in the two experiments made, the cobalt compound contained a little more oxygen after boiling with the hypochlorite.

We have now many instances of the influence which small quantities of substances have upon chemical reactions. These influences may be more common than is generally supposed. The presence of a third body is frequently helpful in the combination of elements with one another: thus dry chlorine will not attack melted sodium or finely divided copper; an electric spark will not cause a dry mixture of carbonic oxide and oxygen to explode; carbon, phosphorus, and sulphur will not unite with dry oxygen, and as chemical science progresses we may find that many well-known actions are conditioned by the presence of minute traces of other matter which have hitherto escaped detection. We all know the profound alterations of the properties of substances by minute traces of impurities, less than one-tenth per cent. of phosphorus will render steel unfit for certain purposes. The sapphire and ruby only differ from colourless alumina by the presence of traces of impurities hardly recognisable by chemical analysis. During this meeting we hope to have a contribution to the section on the influence of minute traces of what may be called impurities on the properties of different substances and their influence on chemical changes.

In this city, where the first public chemical laboratory was started in 1823 by Dr. Anderson, the assistant of Professor Hope, it is hardly necessary to insist on the extreme importance of teaching chemistry by practical work, but unfortunately, even at the present time, endeavours are made to teach the subject by means of lectures (sometimes without experiments) or by reading. Those who are acquainted with chemistry well know the impossibility (this is hardly too strong a word) of learning the science, especially in the first stages, without actual experiment, by which a practical acquaintance with chemical phenomena is obtained. The attempt to learn chemistry without practical experience reminds one of the well-known story (for the truth of which I will not vouch) of a mathematician who lectured on natural philosophy; he was visiting a foreign laboratory, and stopped before a piece of apparatus and asked what it was: on being told it was an air-pump he exclaimed: 'Dear me! I have lectured on the air-pump for twenty-five years, and this is the first time I have seen one.' It is problematical if his students can have derived much advantage from his lectures. Teaching of the kind to which reference has just been made is generally given to candidates for examinations who do not intend to take up chemistry as their chief subject. At the present time chemistry is required for entrance and preliminary examinations

from many classes of students. There is no doubt that it is an excellent means of education, teaching a boy to observe and draw conclusions from his observations; but if he makes no observations it is little more than useless cram, the memory might as well be exercised by learning a novel by heart.

This imperfect mode of teaching chemistry arises principally from the difficulty of obtaining properly appointed laboratories in schools, in addition to which the very strong fumes are sometimes disagreeable, making it inconvenient to have them in or near a house, to say nothing of the possible dangers to the clothes and their contents; but there is no help for it, the teaching must be accompanied by experimental demonstration, as was indicated in the Reports on the teaching of chemistry which have been presented to this Association in former years. It must be admitted that examinations do not always discover the best student; many are capable of preparing for examinations with a small knowledge of their subject, others, with a good knowledge, fail from nervousness or other causes, but at the present time, examination, although far from perfect, is almost the only means we have of judging the fitness of the candidate. By properly selecting questions the examiner may, to a considerable extent, discourage cram; he should endeavour to find out what the pupils have actually seen, and to make them draw conclusions from facts which they have either themselves observed, or which have been described to them; it is only in this manner that chemistry can be used as a means of mental training.

These remarks do not apply to the education of students intending to make chemistry their profession, who have many opportunities, in the large laboratories of Great Britain and the Continent, of obtaining all the necessary instruction. The Institute of Chemistry, which was founded to improve the status and also the education of professional chemists, requires that its members should have a thoroughly scientific training. Before a candidate for the associateship is admitted to examination, he must bring evidence that he has passed satisfactorily through a systematic course of at least three years' study in the subjects of theoretical and practical chemistry, physics, and elementary mathematics, in some recognised college or school; and before admission to the fellowship he must have passed through three additional years of work in chemistry. It is to be hoped that an example of this kind will ultimately have a good effect in improving the modes of teaching the science in its elementary stages.

There is another class of workers in chemistry who must not be forgotten at the present time, as they have much influence on the life of the world and have been working for ages, but have only recently been recognised. I mean those organisms which are included under the name of microbes. These organisms are capable of producing chemical changes which entirely surpass all the results hitherto obtained by the chemist in his laboratory. That the transformation of sugar into alcohol and carbonic anhydride in the ordinary process of fermentation is due to a living organism, has been known for some years; the important transformation of ammonia into nitrous and nitric acids in the soil has been shown to be due to organisms which have recently been investigated by many chemists; it is possible to transform ammonia into these acids in the laboratory by oxidation under certain conditions and at a high temperature, whereas the organism does the work quite as efficaciously at the common temperature. Other organisms have the power of producing complex organic poisons by the alteration of some of the constituents of the animal body, and the relation of these products to the study of diseases is of the highest possible importance. As we hope to have a discussion on this interesting subject by many eminent authorities, both from the chemical and biological points of view, it will be unnecessary to pursue the subject further, unless it be to urge some of the younger chemists to work at the chemical aspect of bacteriology. They must be prepared for hard work and many disappointments, for the subject is undoubtedly a difficult one.

I cannot conclude this address without reference to the great loss which chemistry has sustained by the death of Professor A. W. von Hofmann. I had the good fortune to be under him as student and assistant from 1856 until he left this country in 1865; all who worked with him must have been deeply impressed

by his capacity for work and his power of inducing work in others. Although perhaps some of us did not appreciate this at the time, yet we feel we owe him a debt of gratitude for his having started us in the right way. The list of papers under his name in the Royal Society Catalogue up to the year 1883 is 299, written by himself alone, besides twenty-two joint papers. One of his characteristics which impressed me was his investigation for the purpose of furthering chemical knowledge without any view to practical applications, and I well remember his lecture at the Royal Institution, in 1862, on Mauve and Magenta (which owed so much of their success to his work), in which he produced the original specimen of benzene which had been obtained by Faraday from oil-gas in 1825. He pointed out that Faraday had prepared this substance and investigated its properties without ever supposing that it could have any practical application. The following is the concluding paragraph of the lecture :—

'Need I say any more ? The moral of Mauve and Magenta is transparent enough; I read it in your eyes. We understand each other. Whenever in future one of your chemical friends, full of enthusiasm, exhibits and explains to you his newly-discovered compounds, you will not cool his noble ardour by asking him that most terrible of all questions, "What is its use ? Will your compound bleach or dye ? Will it shave ? May it be used as a substitute for leather ?" Let him quietly go on with his work. The dye, the lather, the leather will make their appearance in due time. Let him, I repeat it, perform his task. Let him indulge in the pursuit of truth—of truth pure and simple—of truth not for the sake of Mauve, not for the sake of Magenta, let him pursue truth for the sake of truth.'

This seems to me the true spirit of the scientific investigator, and in many cases the reward consists solely in the consciousness that the investigator has done his duty; in some cases the reward may take a more substantial form, and since the above paragraphs were written I have been informed that Professor von Hofmann has left a large fortune, the result of the applications of his discoveries in technical chemistry.

EDINBURGH, 1892.

# ADDRESS

### TO THE

# GEOLOGICAL SECTION

### OF THE

# BRITISH ASSOCIATION,

### BY

### PROFESSOR C. LAPWORTH, LL.D., F.R.S., F.G.S.,

#### PRESIDENT OF THE SECTION.

IT has, I believe, been the rule for the man who has been honoured by election to the Chair of President of this Geological Section of the British Association to address its members upon the recent advances made in that branch of geology in which he has himself been most immediately interested. It is not my intention upon the present occasion to depart from this time-honoured custom; for it has both the merit of simplicity and the advantage of utility to recommend it. In this way each branch of our science, as it becomes in turn represented, not only submits to the workers in other departments a report of its own progress, but presents by implication a broad sketch of the entire geological landscape, seen through the coloured glasses, it may be, of divisional prejudice, but at any rate instructive and corrective to the workers in other departments, as being taken from what is to them a novel and an unfamiliar point of view.

Now every tyro in geology is well aware of the fact that the very backbone of geological science is constituted by what is known as stratigraphical geology, or the study of the geological formations. These formations, stratified and un-stratified, build up all that part of the visible earth-crust which is accessible to the investigator. Their outcropping edges constitute the solid framework of the globe, the surface of which forms the physical geography of the lands of the present day; and their internal characters and inter-relationships afford us our only clues to the physical geographies of bygone ages. Within them lies enshrined all that we may ever hope to discover of the history and the development of the habitable world of the past.

These formations are to the stratigraphical geologist what species are to the biologist, or what the heavenly bodies are to the astronomer. It was the discovery of these formations which first elevated geology to the rank of a science. In the working out of their characters, their relationships, their development, and their origin, geology finds its means, its aims, and its justification. Whatever fresh material our science may yield to man's full conception of nature, organic and inorganic, must of necessity be grouped around these special and peculiar objects of its contemplation.

When the great Werner first taught that our earth-crust was made up of superimposed rock-sheets, or formations, arranged in determinable order, the value of his conclusions from an economic point of view soon led to their enthusiastic and careful study; and his crude theory of their successive precipitation from a universal chaotic ocean disarmed the suspicions of the many until the facts themselves had gained such a wide acceptance that denial was no longer possible. But when the greater Scotchman, Hutton, asserted that each of these rock-formations was in reality nothing more nor less than the recemented ruins of an

C

earlier world, the prejudices of mankind at large were loosed at a single stroke. Like Galileo's assertion of the movement of the globe, this demanded such an apparently undignified and improbable mode of creation that there is no wonder that, even down to the present day, there still exist some to whom this is a hard saying, to be taken, if taken at all, in homœopathic doses and with undisguised reluctance.

Hutton as regards his philosophy was, as we know, far in advance of his time. With all the boldness of conviction he unflinchingly followed out his ideas to their legitimate results. He claimed that as the stratified formations were composed of similar materials—sands, clays, limestones, and muds—to those now being laid down in the seas around our present coasts, they must, like them, have been the products of the ordinary natural agencies of rain, rivers, and sea waters, internal heat and external cold, acting precisely as they act now. And, further, that as these formations lie one below the other, in apparently endless downward succession, and all formed more or less of these fragmentary materials, so the present order of natural phenomena must have existed for untold ages. Indeed, to the commencement of this order, he frankly admits ' I see no trace of a beginning or sign of an end.'

The history of the slow acceptance of Hutton's doctrines, even among geologists, is, of course, perfectly familiar to us all. William Smith reduced the disputed formations to order, and showed that not only was each composed of the ruins of a vanished land, but that each contained in its fossils the proof that it was deposited in a vanished sea inhabited by a special life creation. Cuvier followed, and placed it beyond question that the fossilised relics of these departed beings were such as made it absolutely unquestionable that these creatures might well have inhabited the earth at the present day. Lyell completed the cycle by demonstrating stage by stage the efficiency of present natural agencies to do all the work required for the degradation and rebuilding of the formations. Since his day the students of stratigraphical geology have universally acknowledged that in the study of present geographical causes lies the key to the geological formations and to the inorganic world of the past.

In this way the road was paved for Darwin and the doctrine of descent. The aid which had been so ungrudgingly afforded by biology to geology was repaid by one of the noblest gifts ever made by one science to another. For the purposes of geology, the science of biology had practically completed a double demonstration: first, that the extinct life discernible in the geological formations was linked inseparably with the organic life of the present; and, second, that every fossil recognised by the geologist was the relic of a creature that might well have existed upon the surface of the earth at the present time. Geology repaid its obligation to biology by the still greater twofold demonstration: first, that in the economy of nature the most insignificant causes are competent to the grandest effects, if only a sufficiency of time be granted them; and, second, that in the geological formations we have the evidences of the actual existence of those mighty eons in which such work might be done.

The doctrine of organic evolution would always have remained a metaphysical dream had geology not given the time in which the evolution could be accomplished. The ability of present causes to bring about slow and cumulative changes in the species is, to all intents and purposes, a biological application of Hutton's ideas with respect to the origin of the geological formations. Darwin was a biological evolutionist, because he was first an uniformitarian geologist. Biology is pre-eminent to-day among the natural sciences, because its younger sister, Geology, gave it the means.

But the inevitable consequence of the work of Darwin and his colleagues was that the centre of gravity, so to speak, of popular regard and public controversy, was suddenly shifted from stratigraphical geology to biology. Since that day stratigraphical geology, to its great comfort and advantage, has gone quietly on its way unchallenged, and all its more recent results have, at least by the majority of the wonder-loving public, been practically ignored.

Indeed, to the outside observer it would seem as if stratigraphical geology for

the last thirty years had been practically at a standstill. The startling discoveries and speculations of the brilliant stratigraphists of the end of the last century and the first half of the present, forced the geology of their day into the very front rank of the natural sciences, and made it perhaps the most conspicuous of them all in the eyes of the world at large. Since that time, however, their successors have been mainly occupied in completing the work of the great pioneers. The stratigraphical geologists themselves have been almost wholly occupied in laying down upon our maps the superficial outlines of the great formations, and working out their inter-relationships and subdivisions. At the present day the young stratigraphical student soon learns that all the limits of our great formations have been laid down with accuracy and clearness, and finds but little to add to the accepted nomenclature of the time.

Our palæontologists also, have equally busied themselves in working out the rich store of the organic remains of the geological formations; and the youthful investigator soon discovers that almost every fossil he is able to detect in the field, has already been named, figured, and described, and its place in the geological record more or less accurately fixed.

In France, in Germany, in Norway, Sweden, and elsewhere, in Canada and in the United States, work as thorough, and as satisfactory has been accomplished, and the local development of the great stratified formations and their fossils has been laid down with detail and clearness.

Many a young unfledged but aspiring geologist, alive to these facts, and contrasting the well-mapped ground of the present time with the virgin lands of the days of the great pioneers, finds it hard to stifle a feeling of keen regret that there are nowadays no new geological worlds to conquer, no new systems to discover and name, and no strange and unexpected faunas to unearth and bring forth to the astonished light of day. The youth of stratigraphical geology, with all its wonder and freshness, seems to have departed, and all that remains is to accept, to commemorate, and to round off the glorious victories of the dead heroes of our science.

But to the patient stratigraphical veteran, who has kept his eyes open to discoveries new and old, this lull in the war of geological controversy presents itself rather as a grateful breathing time; the more grateful as he sees looming rapidly up in front the vague outlines of those oncoming problems which it will be the duty and the joy of the rising race of young geologists to grapple with, and to conquer, as their fathers met and vanquished the problems of the past. He knows perfectly well that Geology is yet in her merest youth, and that to justify even her very existence, there can be no rest until the whole earth-crust and all its phenomena, past, present, and to come, have been subjected to the domain of human thought and comprehension. There can be no more finality in Geology than in any other science; the discovery of to-day is merely the stepping-stone to the discovery of to-morrow; the living theory of to-morrow is nourished by the relics of its parent theory of to-day.

Now if we ask what are these formations which constitute the objects of study of the stratigraphical geologist, I am afraid that, as in the case of the species of the biologist, no two authorities would agree in framing precisely the same definition. The original use of the term formation was of necessity lithological, and even now the name is most naturally applied to any great sheet of rock which forms a component member of the earth-crust; whether the term be used specifically for a thin homogeneous sheet of rock like the Stonesfield slate, ranging over a few square miles; or generically, for a compound sheet of rock, like the Old Red Sandstone, many thousands of feet in thickness, but whose collective lithological characteristics give it an individuality recognisable over the breadth of an entire continent.

When Werner originally discovered that the 'formations' of Saxony followed each other in a certain recognisable order, a second characteristic of a formation became superposed upon the original lithological conception—namely, that of determinate 'relative position.' And when William Smith proved that each of the formations of the English Midlands was distinguished by an assemblage of organic remains peculiar to itself, there became added yet a third criterion—that of the possession of 'characteristic fossils.'

But these later superposed conceptions of time-succession, and life-type, are far better expressed by dividing the geological formations, on the one hand, into zoological *zones*, and grouping them together, on the other hand, into chronological *systems*. For in the experience of every geologist he finds his mind instinctively harking back to the bare lithological application of the word 'formation,' and I do not see that any real advantage is gained by departing from the primitive use of the term.

A 'zone,' which may be regarded as the *unit* of *zoological succession*, is marked by the presence of a special fossil, and may include one or many subordinate formations. A *system*, which is, broadly speaking, the *unit of geological time* or *succession*, includes many 'zones,' and often, but not always, many 'formations.' A *formation*, which is the *unit of geological stratigraphy*, is a rock sheet composed of many strata possessing common lithological characters. The formation may be simple, like the Chalk, or compound, like the New Red Sandstone, but, simple or compound, local or regional, it must be always recognisable, geographically and geologically, as a lithological individual.

As regards the natural grouping of these lithological individuals as such, fair progress has been made of late years and our information is growing apace. We know that there are at any rate three main groups : First, the stratified formations due to the action of moving water above the earth-crust. Second, the igneous formations which are derived from below the earth-crust. Third, the metamorphic formations which have undergone change within the earth-crust itself. We know also that of these three the only group which has hitherto proved itself available for the purpose of reading the past history of the globe is that of the stratified formations.

Studying these stratified formations therefore in greater detail, we find that they fall naturally in their turn into two sets, viz.: a mechanical set of pebble beds, sandstones and clays, formed of rock fragments, washed off the land into the waters, and an organic set of limestones, chalk, &c., formed of the shells and exuviæ of marine organisms.

But when we attempt a further division of these two sets, our classification soon begins to lose its definiteness. We infer that some formations, such as the Old Red and the Triassic, were the comparatively rapid deposits of lakes and inland seas ; that others, like the Coal Measures, London Clay, &c., were the less rapid deposits of lagoons, river valleys, deltas, and the like ; that others, like our finely laminated shales and clays of the Silurian and Jurassic, were the slower deposits of the broader seas ; and finally, that others, like our Chalk and Greensand, were possibly the extremely slow deposits of the oceanic deeps.

Nevertheless, after looking at the formations collectively, there remains no doubt whatever in the mind of the geologist that their mechanical members are the results of the aqueous degradation of vanished lands, and that their organic members are the accumulated relics of the stony secretions of what once were living beings. Neither is there any possibility of escape from the conclusion that they have all been deposited by water in the superficial hollows of the sea-bottoms and ocean floors of the earth-crust of their time.

In the life of every individual stratified formation of the mechanical type we can always distinguish three stages : first, the stage of erosion and transportation, in which the rock fragments were worn off the rocks of the higher ground and washed down by rain and rivers to the sea ; second, a stage of deposition and consolidation below the surface of the quiet waters ; and third, a final stage in which the completed rock-formation was bent and upheaved, in part at least, into solid land. In the formations of the organic type three corresponding stages are equally discernible : first, the period of mineral secretion by organised beings ; second, the period of deposition and consolidation ; and third, the final period of local elevation in mass. But one and all, mechanical and organic alike, the formations bear in their composition, in their arrangement, and in their fossils, abundant and irresistible evidences that they *were* the products, and that they *are* the memorials of the physical geography of their time.

Guided by the principles of Hutton and Lyell, geologists have worked out with great care and completeness the effects of those agencies which rule in the first of

these three life-stages in the history of a mechanical formation. No present geological processes are more familiar to the young geologist than those of denudation, erosion, and transportation. They form together the subject-matter of that most wonderful fascinating chapter in geology, which, from its modest opening among the quiet Norfolk sandhills, sweeps upwards and onwards without a break to its magnificent close on the brink of the gorge of the Colorado. But our knowledge of the detailed processes of deposition and consolidation which rule in the second stage is still exceedingly imperfect, although a flood of light has been thrown upon the subject by the brilliant results of the *Challenger* Expedition. And we are compelled to admit that our knowledge of the operations of those agencies which rule in the processes of upheaval and depression is as yet almost nil, and what little we have already learnt of the effects of these agencies is the prey of hosts of conflicting theories that merely serve to annoy and bewilder the working student of the science.

But not one of the formative triad of detrition, deposition, and re-elevation can exist without the others. No detrition is possible without the previous upheaval of the rock-sheet from which material can be removed; no deposition is possible without the previous depression of the rock-sheet which forms the basin in which the fragmentary material can be laid down.

Our knowledge, therefore, of the origin and meaning of any geological formation whatever, can at most be only fragmentary until this third chapter in the life-history of the geological formation has been attacked in earnest.

Now all the rich store of knowledge we possess respecting the first stage in the life of a geological formation has been derived from a comparison of the phenomena which the stratigraphical geologist finds in the rock formations of the past, with correspondent phenomena which the physical geographer discovers on the surface of the earth at the present. And all that we know of the second stage again has been obtained in precisely the same way. Surely analogy and common sense both teach us, that all which is likely to be of permanent value to us as regards the final stage of elevation and depression must be sought for in the same direction.

Within the last twenty years or so many interesting and vital discoveries have been made in the stratigraphy of the rock formations, that bear largely upon this obscure chapter of elevation and depression. And I propose on this occasion that we try to summarise a few of these new facts; and reading them in conjunction with what we actually know of the physical geography of the present day, try to ascertain how such mutual agreement as we can discover may serve to aid the stratigraphical geologist in his interpretation of the true meaning of the geological formations themselves. We may not hope for many years to come to read the whole of this geological chapter, but we may perhaps modestly essay an interpretation of one or two of the opening paragraphs.

In the physical geography of the present day, we find the exterior of our terraqueous globe divided between the two elements land and water. We know that the solid geological formations exist everywhere beneath the visible surface of the lands, but of their existence under the present ocean floor we have as yet no absolute certainty. We know both the form of the surface and the composition of the continental parts of the lithosphere; we only know as yet even in outline the form of its oceanic portions. The surface of each of our great continental masses of land resembles that of a long and broad arch-like form, of which we see the simplest type in the New World. The surface of this American arch is sagged downwards in the middle into a central depression which lies between two long marginal plateaux; and these plateaux are finally crowned by the wrinkled crests which form its two modern mountain systems. The surface of each of our ocean floors exactly resembles that of a continent turned upside down. Taking the Atlantic as our simplest type, we may say that the surface of each ocean basin resembles that of a mighty trough or syncline, buckled up more or less centrally into a medial ridge, which is bounded by two long and deep marginal hollows, in the cores of which still deeper grooves sink to the profoundest depths. This complementary relationship descends even to the minor features of the two.

Where the great continental sag sinks below the ocean level, we have our gulfs and our Mediterraneans, seen in our type continent as the Mexican Gulf and Hudson Bay. Where the central oceanic buckle attains the water-line we have our oceanic islands, seen in our type ocean as St. Helena and the Azores. Although these apparent crust-waves are neither equal in size nor symmetrical in form, this complementary relationship between them is always discernible. The broad Pacific depression seems to answer to the broad elevation of the Old World—the narrow trough of the Atlantic to the narrow continent of America.

Every primary wave of the earth's surface is broken up into minor waves, in each of which the ridge and its complementary trough are always recognisable. The compound ridge of the Alps answers to the compound Mediterranean trough; the continuous western mountain chain of the Americas to the continuous hollow of the Eastern Pacific which bounds them; the sweep of the crest of the Himalaya to the curve of the Indo-Gangetic depression. Even where the surface waves of the lithosphere lie more or less buried beneath the waters of the ocean and the seas, the same rule always obtains. The island chains of the Antilles answer to the several Caribbean abysses, those of the Ægean Archipelago answer to the Levantine deeps.

Draw a section of the surface of the lithosphere along a great circle in any direction, the rule remains the same : crest and trough, height and hollow succeed each other in endless sequence, of every gradation of size, of every degree of complexity. Sometimes the ridges are continental, like those of the Americas, sometimes orographic, like those of the Himalaya; sometimes they are local, like those of the English Weald. But so long as we do not descend to minor details we find that every line drawn across the earth's surface at the present day rises and falls like the imaginary line drawn across the surface of the waves of the ocean. No rise of that line occurs without its complementary depression; the two always go together, and must of necessity be considered together. Each pair constitutes one of those *geographical units of form* of which every continuous direct line carried over the surface of the lithosphere of our globe is made up. This unit is always made up of an arch-like rise and a trough-like depression which shade into each other along a middle line of contrary curvature. It resembles the letter S or Hogarth's line of beauty, and is clearly similar in form to the typical wave of the physicist. Here, then, we reach a very simple and natural conclusion. The surface of the earth-crust of the present day resembles that of a series of crust-waves of different lengths and different amplitudes, more or less irregular and complex, it is true, but everywhere alternately rising and falling in symmetrical pairs like the waves of the sea.

Now this rolling wave-like earth-surface is formed of the outcropping edges of the rock formations which are the special objects of study of the stratigraphical geologist. If, therefore, the physiognomy of the face of our globe is any real index of the character of the personality of the earth-crust beneath, these collective geographical features should be precisely those which answer to the collective structural characters of the geological formations.

In the earlier days of geology one of the first points recognised by our stratigraphists was the fact that the formations were successive lithological sheets, whose truncated outcropping edges formed the present surface of the land, and that these sheets lay inclined at an angle one over the other, as William Smith quaintly expressed it, like a tilted 'pile of slices of bread and butter.' But as discovery progressed, the explanation of this arrangement soon became evident. The formations revealed themselves as a series of what had originally been deposited as horizontal sheets, lying in regular order one over the other, but which had been subsequently bent up into alternating arches and troughs (the anticlines and synclines of the geologist); while their visible parts, which now constitute the surface of our habitable lands, were simply those parts of the formations which are cut at present by the irregular plane of the present earth's surface. All those parts of the great arches and troughs formerly occurring above that plane have been removed by denudation ; all those parts below that plane lie buried still out of sight within the solid earth-crust, although in every geological section of sufficient extent it was

seen that the anticline or arch never occurred without the syncline or trough—in other words, that there was never a rise without a corresponding fall of the stratum; yet it is only of late years that the stratigraphical geologist has come clearly to recognise the fact that the anticline and syncline must be considered together, and must be united as a single crust-wave, for the arch is never present without its complementary trough. The two together constitute the tectonic or orographic unit, *The Fold*, the study of which, so brilliantly inaugurated by Heim in his 'Mechanismus der Gebirgsbildung,' is destined, I believe, in time, to give us the clue to the laws which rule in the local elevation and depression of the earth-crust, and to furnish us with the means of discovery of those occult causes which lie at the source of those superficial irregularities which give to the face of our globe its variety, its beauty, and its habitability.

We have said already that this wave or fold of the geologist resembles that of the wave of the physicist. Now we may regard such a wave as formed of two parts, the arch-like part above and the trough-like part below. The length of the wave is naturally the length of that line joining the outer extremities of the arch and trough, and passing through that centre node or point of origin of the wave itself which bisects the line of contrary curvature. The amplitude of the wave is the height of the arch added to the depth of the trough. The arch part of such a wave, if perfectly symmetrical, may clearly be regarded as belonging either to a wave travelling to the right, in which case the complementary trough is the one in that direction; or it may be regarded as belonging to a wave travelling to the left, in which case its trough must be the one in that direction. But as in the case of the sea wave, the advancing slope of the wave is always the steeper, and the real centre of the wave must lie half-way down this steeper slope; so also, in the case of the geological fold, there is no difficulty in recognising the centre and the real direction of movement.

The fold of the geologist differs from the ordinary wave of the physicist, essentially in the fact, that even in its most elementary conception, as that of a plate bent by a pressure applied from opposite sides, it necessarily includes the element of thickness. And this being the case, the rock sheet which is being folded and curved has different layers of its thickness affected differently. In the arch of fold the upper layers of rock sheet are extended, while its lower layers are compressed. On the contrary in the trough of the fold the upper layers are compressed and the lower layers are extended. But in both arch and trough alike there exists a central layer, which, beyond taking up the common wave-like form, remains practically unaffected.

But the geological fold has in addition to length and thickness the further element of breadth, and this fact greatly complicates the phenomena.

But many of the movements which take place in a rock sheet which is being folded, or in other words those produced by the bending of a compound sheet composed of many leaves, can be fairly well studied in a very simple experiment. Take an ordinary large note-book, say an inch in thickness, with flexible covers. Rule carefully a series of parallel lines across the leaves at the top of the book, about ⅛ of an inch apart, and exactly at right angles to the plane of the cover. Then, holding the front edges loosely, press the book slowly from back and front into an S-like form until it can be pressed no further. As the wave grows, it will be noticed that the cross lines which have been drawn on the upper edge of the book remain fairly parallel throughout the whole of the folding process, except in the central third of the book, where they arrange themselves into a beautiful sheaf-like form, showing how much the leaves of the book have sheared or slidden over each other in this central portion. It will also be seen when the S is complete that the book has been forced into a third of its former breadth. It is clear that the wave the book now forms must be regarded as made up of three sections: viz. a section forming the outside of the trough on the one side, a section forming the outside of the arch on the other, and a central or common section, which may be regarded either as uniting or dividing the other two

As this experiment gives us a fair representation of what takes place in a geological fold, we see at a glance that the geologist is forced to divide his fold into

three parts—an arch limb, a trough limb, and a middle limb—which latter we may call the *copula* or the *septum*, according as we regard it as connecting or dividing the other two. Our note-book experiment, therefore, shows us also that in the trough limb and the arch limb the leaves or layers undergo scarcely any change of relative position beyond taking on the growing curvature of the wave. But the layers in the central part, or *septum*, undergo sliding and shearing. It will be found also, by gripping the unbound parts of the book firmly and practising the folding in different ways, that this *septum* is also a region of warping and twisting. This simple experiment should be practised again and again until these points are clear, and the various stages of the folding process become apparent ; the surface of the book being forced first into a gentle arch-like rise with a corresponding trough-like fall, then stage by stage the arch should be pushed over on to the trough until the surfaces of the two are in contact and the book can be folded no further.

In the structure of our modern mountain ranges we discover the most beautiful illustrations of the bending and folding of the rocky formations of the earth-crust. The early results of Rogers among the Alleghanies, of Lory and Favre in the Western Alps, have been greatly extended of late years by the discoveries of Heim Baltzer in the Central Alps, of Bertrand in Provence, of Margarie in Languedoc, of Dutton and his colleagues in the western ranges of America, and of Peach and Horne and others in the older rocks of Britain. The light these researches throw upon the phenomena of mountain structure will be found admirably summarised and discussed in the works of Leconte, of Dana, of Heim, and finally in the magnificent work of Suess, the ' Antlitz der Erde,' of which only the first two volumes have yet appeared.

Looking first at the mountain fold in its simplest form as that of a bent rock-plate, composed of many layers which have been forced into two similar arc-like forms, the convexities of which are turned the one upwards and the other downwards, we find in the present mountain ranges of the globe every kind represented. We commence with one in which the arch is represented merely by a gentle swell of the rock sheet, and the trough by an answering shallow depression, the two shading into each other in an area of contrary flexure. From this type we pass insensibly to others in which we see that the sides of the common limb or septum are practically perpendicular. From these we pass to folds in which the twisted common limb or septum overhangs the vertical, and so on, to that final extreme where the arch limb has been pushed completely over on to the trough limb, and all three members, as in our note-book experiment, are practically welded into one conformable solid mass.

In many cases, due partly to the action of transverse pressures, the septum becomes reduced to a plane of contrary motion or thrust-plane ; and the arch limb and trough limb slide over each other as two solid masses. But here we have no longer a fold, but a fault.

Although the movements of these folds are slow and insensible, and only effected in the course of ages, so that little or no evidence of the actual movement of any single one of them has been detected since they were first studied, yet it is perfectly plain that when we regard them collectively, we have here crust folds in every stage of their existence. Each example in itself represents some one single stage in the lifetime of a single fold. They are simply crust folds of different ages. Some are, as it were, just born ; others are in their earliest youth. Some have attained their majority, some are in the prime of life, and some are in the decrepit stages of old age. Finally, those in which all three members—arch limb, trough limb, and septum—are crushed together into a single mass, are dead. Their life of individual movement is over. If the earth pressure increases the material which they have packed together may of course form a passive part of a later fold, but they themselves can move no more.

We see that every mountain fold commences first as a gentle alternate elevation and depression of one or more of the component sheets of the geological formations which make up the earth-crust. This movement is due apparently to the tangential thrusts set up by the creeping together, as it were, of those neighbouring and more resistant parts of the earth-crust which lie in front of and behind the

moving wave. Yielding slowly to these lateral thrusts the crest of the fold rises higher and higher, the trough sinks lower and lower, the central common limb grows more and more vertical, and becomes more and more strained, sheared, and twisted. As this middle limb yields, the rising arch part of the fold is forced gradually over on to the sinking trough, until at last all three members come into conformable contact and further folding as such is impossible. Movement ceases; the fold is dead. We see also from our note-book experiment that the final result of the completion of the fold is clearly to strengthen up and consolidate that part of the crust plate to the local weakness of which it actually owed its origin and position. The fold has by its life-action theoretically trebled the thickness of that part of the earth-plate in which its dead remains now lie. If the lateral pressure goes on increasing, and the layers of the earth-crust again begin to fold in the same region, the inert remains of the first fold can only move as a passive part of a newer fold, either as a part of the new arch-limb, the new trough-limb, or the new septum. As each younger and younger fold formed in this way necessarily includes a more resistant, and therefore a thicker, broader, and deeper sheet of the earth-crust, we have here the phylogenetic evolution of a whole family of crust folds, each successive member of which is of a higher grade than its immediate predecessor.

But it very rarely happens that the continuous plate in which any fold is imbedded is able to resist the crust creep until the death of the first fold. Usually, long before the first simple fold is completed, a new and a parallel one rises in front of it on the side of the trough limb; and the two grow, as it were, henceforward, side by side. But the younger fold, being due to a greater pressure than the older, must of necessity be of a higher specific grade, and the two together form a generic fold in common.

Our present mountain systems are all constituted of several families of folds, all formed in this way, of different gradations of size, of different dates of origin, and of different stages of life evolution; and in each family group the members are related to each other by this natural genetic affinity.

Sometimes the new folds are formed in successive order only on one side of the first fold, and then we have our unilateral (or so-called unsymmetrical) mountain groups, like those of the Jura and the Bavarian Alps. Sometimes they are formed on both sides of the original fold, and then we have our bilateral (or so-called symmetrical) ranges, like the Central Alps. In both cases the septa of the aged or dead folds are of necessity all directed inwards towards the primary fold. If, therefore, they originate only on one side of the fold, our mountain group looks unsymmetrical, with a very steep side opposed to a gently sloping side. If they grow on both sides of the original fold, we have the well-known 'fan structure' of mountain ranges. In this case the whole complex range is seen at a glance to be a vast compound arch of the upper layers of the earth-crust, keyed up by the material of the dead or dying folds, which, by the necessities of the case, constitute mighty wedges, whose apices are directed inwards towards the centres of the system. But a complete arch of this kind is in reality not a single fold, but a double one, with a septum on both sides of it, and it requires two troughs, one on each side of it, as its natural complement. The so-called unsymmetrical ranges, therefore, which are constituted merely of arch limb, trough limb, and septum, are the more natural and the more common.

It is clear that in the lifetime of any single fold, its period of greatest energy and most rapid movement must be that of middle life. In early youth the lateral pressure is applied at a very small angle, and the tangential forces act therefore under the most disadvantageous circumstances. In the middle life of the fold the arch limb and the trough limb stand at right angles to the septum, and the work of deformation is then accomplished under the most favourable mechanical conditions and with the greatest rapidity. That is to say, the activity of the fold and the rate of movement of the septum, like the speed of the storm wind, varies directly as the gradient.

In our note-book experiment we observed that little or no change took place in the arch limb and trough limb, while the septum became remarkably sheared

and twisted. The same is the case in nature ; but here we have to recollect that these moving mountain folds are of enormous size, indeed actual mountains in themselves. These great arches, scores of miles in length, thousands of feet in height and thickness, must of necessity be of enormous weight, capable of crushing to powder the hardest rocks over which they move, while the thrust which drives them forward is practically irresistible. It is plain, therefore, that while the great arch limb and the trough limb of one of these mighty folds move towards each other from opposite directions, they form in conjunction an enormous machine, composed of two mighty rollers or millstones, which mangle, roll, tear, squeeze, and twist the rocky material of the middle limb or septum, which lies jammed in between them, into a laminated mass. This deformed material, which is the characteristic product of the mountain-making forces, is, of course, made up of the stuff or the original middle limb of the fold ; and whether we call it breccia, mylonite, phyllite, or schist, although it may be composed of sedimentary stuff, it is certainly no longer a *stratified* rock ; and though it may have been originally purely igneous material, it is certainly no longer *volcanic*. It is now a manufactured article made in the great earth mill.

These mountain folds, however, are merely the types of folds and wrinkles of all dimensions which affect the rock formations of the earth-crust. Within the mountain chains themselves we can follow them fold within fold, first down to formations, then to strata, then to laminæ, till they disappear at last in microscopic minuteness beyond the limits of ordinary vision. Leaving these, however, for the moment, let us travel rather in the opposite direction, for these mountain folds are by no means the largest known to the stratigraphical geologist. Look at any geological section crossing our type continent of North America, and it will be found that the whole of the Rocky Mountain range on its western side, and the Alleghany ranges on the eastern are really two mighty compound geological anticlines, while the broad sag of the Mississippi Basin is a compound geological syncline made up of the whole pile of the geological formations. That is to say, the continent of North America is composed of a pair of geological folds, the two arches of which are represented by the Rockies on the one side and the Alleghanies on the other ; while the intermediate Mississippi syncline is the common property of both. Here, then, we reach a much higher grade of fold than the orographic or mountain-making fold, viz. the plateau-making fold or the semi-continental fold, which, because of its enormous breadth, must include a very much thicker portion of the earth-crust than the ordinary orographic fold itself.

But which must be the actual middle limbs of these two American folds, the septal areas where most work is being done and the motion is greatest?

Taught by what we have already learnt of the mountain wave the answer is immediate and certain. It must be the steeper sides of each of the two folds, namely, those which face the ocean. How perfectly this agrees with the geological facts goes without saying. It is on the steep Pacific side of the western fold that the crushing and crumpling of its rocks is the greatest. It is on the Atlantic side of the eastern fold that the contortion and metamorphism of its rocks are at their maximum, while in the common and gently sloping trough of both folds, namely, in the intermediate Mississippi Valley, the entire geological sequence remains practically unmodified throughout.

Again, which of these two American folds should be the more active at the present day ? Taught by our study of the mountain wave, the answer again is immediate and conclusive. It must be that fold whose septum has the steeper gradient. Geology and geography flash at once into combination. The steeper Pacific septum of the western fold, from Cape Horn to Alaska, is ablaze with volcanoes, while the gently inclined Atlantic septum of the eastern fold, from Greenland to Magellan Straits shows none, except on the outer edge of the Antilles, at the very point where the slope of the earth surface is the steepest. We see at a glance that the vigour of these two great continental folds, like those of our mountain waves, varies directly as the surface gradient of the septum.

But the geographical surface of North America, considered as a whole, is in reality that of a double arch with a sag or common trough in the middle. We

have seen already that this double arch must be regarded as the natural comple-ment of the equally double Atlantic trough. Here, then, if the path of analogy we have hitherto so triumphantly followed up to this point is still to guide us, the basin of the Atlantic must be, not only in appearance but in actuality, formed of two long minor folds of the same grade as the two that form the framework of America, but with their members arranged in reverse order. If so, their sub-marine septa ought also to be lines of movement and of volcanic action. And this is again the case. The volcanic islands of the Azores and St. Helena lie not exactly on the longitudinal crests of the mid-oceanic *Challenger* ridge, but upon its bounding flanks.

But we have not yet, however, finished with our simple folds. If we draw a line completely round the globe, crossing the Atlantic basin at its shallowest, between Cape Verde to Cape St. Roque, and onwards in the direction of Japan, where the Pacific is at its deepest, as the trace of a great circle, we find that we have before us a crust fold of the very highest and grandest order. We have one mighty continental arch stretching from Japan to Chili, broken medially by the sag of the Atlantic trough, and this great terrestrial arch stands directly opposed to its natural complement, the great trough of the Pacific, which is bent up in the middle by the mightiest of all the submarine buckles of the earth-crust, on whose crest stand the oceanic islands of the central Pacific.

But if this be true, then the septum of all septa on our present earth-crust must cross our grandest earth fold where the very steepest gradient occurs along this line, and it must constitute the centre-point of the moving earth fold, and of greatest present volcanic activity. And where is this most sudden of all de-pression ? Taught once more by our geological fold, the answer is instantaneous and incontrovertible. It is on the shores of Japan, the region of the mightiest and most active of all the living and moving volcanic localities on the face of our globe.

But the course of the line which we indicated as forming our grandest terres-trial fold returns upon itself. It is an endless fold, an endless band, the common possession of two sciences. It is geological in origin, geographical in effect. It is the wedding ring of Geology and Geography, uniting them at once and for ever in indissoluble union.

Such an endless fold again must have an endless septum, which in the nature of things must cross it twice. Need I point out to the merest tyro in these wedded sciences, that if we unite the Old and New worlds, and Australia, with their inter-mediate sags of the Atlantic and the Indian Oceans, as one imperial earth-arch, and the unbroken watery expanse of the Pacific as its complementary depression, the circular coastal band of contrary surface-flexure which lies between them should constitute the moving master septum of the present earth-crust ? This is the ' volcanic girdle of the Pacific,' our 'Terrestrial Ring of Fire.'

Or, finally, if we rather regard the compact arch of the Old World itself as the natural complement of the broken Indo-Pacific depression, then the most active and continuous septal band of the present day should divide them. Again, our law asserts itself triumphantly. It is the great volcanic and earthquake band on which are strung the Festoon Islands of Western Asia; the band of Mt. St. Elias, the Alentians, Kamtchatka, and the Kuriles; the band of Fusijama, Krakatoa, and Sangir. The rate of movement of the earth's surface doubtless everywhere varies directly as the gradient.

We find, therefore, that even if we restrict our observations to the most simple and elementary conceptions of the rock fold, as being made up of arch-limb, trough-limb, and twisting but still continuous septum, we are able to connect in one un-broken chain of causation the minutest wrinkle on the finest lamina of a geological formation, with the grandest geographical phenomena of the face of our globe.

We find, precisely as we anticipated, that the wave-like surface of the earth of the present day reflects, in its entirety, the wave-like arrangement of the geological formations below. On the lands we find that the surface arches and troughs answer precisely to the grander regional anticlines and synclines of the subterra-nean sedimentary sequence ; and it may, I believe, be regarded as certain that the

submarine undulations have a similar relationship. We find in the New Geology, as Hutton found in the Old, that Geography and Geology are one. We discover, as we suspected, that the physiognomy of the face of our globe is an unerring index of the solid personality beneath. It bears in its lineaments the peculiar family features and the common traits of its long line of geological ancestors.

Such, it seems to me, is an imperfect account of the introductory paragraphs of that great chapter in the New Geology, now in course of interpretation by geologists of the present day, and we have translated them exactly in the old way, by means of the only living geological language, the language of present natural phenomena, and I doubt not that sooner or later the rest of the great chapter will be read by the same simple means.

I have strictly confined myself to-day to the discussion of the characteristics of the simple geological fold as reduced to its most elementary terms of arch, trough, and unbroken septum; for this being clearly understood, the rest naturally follows. But this twisted plate is really the key which opens the entire treasure-house of the New Geology, in which lie spread around in bewildering confusion, facts, problems, and conclusions, enough to keep going the young stratigraphists and other scientists busily at work for the next half-century.

INCOMPLETE

# ADDRESS

## TO THE

# BIOLOGICAL SECTION

### OF THE

# BRITISH ASSOCIATION,

### BY

## Professor WILLIAM RUTHERFORD, M.D., F.R.S.,

### PRESIDENT OF THE SECTION.

AT the meeting of this Association held at Birmingham in 1886 I had the honour of delivering a lecture on the Sense of Hearing, in which I criticised the current theory of tone-sensation, and I propose on this occasion to discuss the current theories regarding our sense of colour.

I may premise that our conceptions of the outer world are entirely founded on the experience gathered from our sensory impressions. Through our organs of sensation, mechanical, chemical, and radiant energies impress our consciousness. The manner in which the physical agents stimulate the peripheral sense-organs, the nature of the movement transmitted through our nerves to the centres for sensation in the brain, the manner in which different qualities of sensation are there produced—all these are problems of endless interest to the physiologist and psychologist.

Every psychologist has acknowledged the profound significance of Johannes Muller's law of the specific energies—or, as we should rather say, the specific activities of the sense-organs  To those unfamiliar with it, I may explain it by saying, that if a motor nerve be stimulated, the obvious result is muscular movement; it matters not 'by what form of energy the nerve is stimulated—it may be by electricity or heat, by a mechanical pinch or a chemical stimulus, the specific result is muscular contraction.  In like manner, when the nerve of sight is stimulated—it may be by light falling on the retina, or by electricity, or mechanical pressure, or by cutting the nerve—the invariable result is a luminous sensation, because the impression is transmitted to cells in the centre for vision in the brain, whose specific function is to produce a sense of light.

The same principle applies to the other sensory centres; when thrown into activity, they each produce a special kind of sensation.  The sun's rays falling on the skin induce a sense of heat, but falling on the eye, they induce a sense of sight. In both cases, the physical agent is the same ; the difference of result arises from specific differences of function in the brain centres concerned in thermal and visual sense.  We have no conception how it is that different kinds of sensation arise from molecular movements in the different groups of sensory cells ; we are as ignorant of that as we are of the nature of consciousness itself.

The subject I propose to discuss on this occasion is not the cause of the different *kinds* of sensation proper to the different sense-organs, but the causes of some *qualities* of sensation producible through one and the same sense-organ.

The theory of tone-sensation proposed by Helmholtz is, that the ear contains an elaborate series of nerve terminals capable of responding to tones varying in pitch from 16 vibrations to upwards of 40,000 vibrations per second, and that at least one different fibre in the auditory nerve, and at least one different cell in the centre

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for hearing, is affected by every tone of perceptibly different pitch. Although the physical difference between high and low tones is simply a difference in frequency of the sound waves, that is not supposed by Helmholtz to be the cause of the different sensations of pitch. According to his theory, the function of frequency of vibration is simply to excite by sympathy different nerve terminals in the ear. The molecular movement in all the nerve fibres is supposed to be identical, and the different sensations of pitch are ascribed to a highly specialised condition of cells in the hearing centre, whereby each cell, so to speak, produces the sensation of a tone of definite pitch, which in no way depends on the frequency of incoming nerve impulses, but simply on the specific activity of the cell concerned.

In my lecture on the Sense of Hearing I pointed out in detail the great anatomical difficulties attending the theory in question. I endeavoured to show the physical defect of a theory which does not suppose that our sensations of harmony and discord must immediately depend upon the numerical ratios of nerve vibrations transmitted from the ear to the central organ, and I offered a new theory of hearing based upon the analogy of the telephone. According to that theory, there is probably no analysis of sound in the ear; the hair-cells at the peripheral ends of the auditory nerve are probably affected by every audible sound of whatever pitch. When stimulated by sound they probably produce nerve vibration, simple or compound, whose frequency, amplitude, and wave-form correspond to those of the sound received. The nerve vibrations arriving in the cells of the auditory centre probably induce simple sensations of tones of different pitch, or compound sensations of harmonies or discords strictly dependent on the relative frequencies of the nerve vibrations coming in through the nerve

I cannot now recapitulate the evidence derived from anatomical, experimental, and pathological observations that give support to my theory of hearing, but I may briefly say that it is opposed to the theory of specific activities, in so far as it has been applied to explain the different qualities of sound sensation. It is, however, in strict accord with the fundamental proposition stated by Fechner [1] in his great work on Psychophysics in these words · 'The first, the fundamental hypothesis is, that the activities in our nervous system on which the sensations of light and sound functionally depend are, not less than the light and sound themselves, to be regarded as dependent on vibratory movements.' It is evident that, if we could only comprehend the nature of the molecular movement in the nerve that links the vibration of the physical agent to that in the sensory cell, we could advance towards a true theory of the physiological basis of different qualities of sensation in the different sense-organs. As yet no definite answer can be given to the question, what sort of molecular movement constitutes a nerve impulse, but in recent years our knowledge of the subject has been extended in a direction that opens up a vista of new possibilities.

A nerve impulse travels at a rate not much more than 100 feet per second—an extremely slow speed compared with that of electricity in a wire. It has been thought to be of the nature of a chemical change sweeping along the nerve, but that hypothesis is opposed by the fact that the most delicate thermo-pile shows no production of heat, even when an impulse is caused to sweep repeatedly along the same nerve. Again, it is far easier to fatigue a muscle than a nerve. A living frog's nerve removed from the animal, and therefore deprived of all nutrition, can retain its excitability for nearly an hour, although subjected all the while to thirty or forty stimulations per second. An excised muscle, when similarly stimulated, is exhausted far sooner, because the mechanical energy entirely springs from chemical change in the muscular substance, and therefore the muscle is more easily fatigued than the nerve. The molecular commotion in the excited nerve produces a momentary electrical current; but that result is not peculiar to nerve. The same occurs in muscle when stimulated. Possibly the molecular movement is of the nature of a mechanical vibration; at all events, we now know that a nerve can transmit hundreds, even thousands, of impulses, or let us simply say vibrations, per second. That fact is so important and significant in relation to the physiology

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[1] *Elemente der Psychophysik*, 1860. 2nd edition, 1889, part ii. p. 282.

of the sense organs, that I show you an experiment to render it more intelligible. A frog's muscle has been hooked to a light lever to record its movement on a smoked cylinder. The nerve of the muscle has been laid on two electrodes connected with the secondary coil of an induction machine. In the primary circuit a vibrating reed has been introduced to serve as a key for making and breaking the circuit, and so stimulating the nerve with periodic induction shocks. If we make the reed long enough to vibrate ten times per second, ten impulses are sent through the nerve to the muscle and ten distinct contractions produced, as shown by the wavy line upon the cylinder. If we shorten the reed so that it will vibrate, say, fifty times per second, the muscle is thrown into a continuous contraction and traces a smooth line on the cylinder; but if we listen to the muscle we can hear a tone having a pitch of fifty vibrations per second, from which we know that fifty nerve impulses are entering the muscle and inducing fifty shocks of chemical discharge in the muscular substance. If we take a reed that vibrates, say, 500 times per second, we hear, on listening to the muscle, a tone having the pitch of 500 vibrations. Observe, that we are not dealing with the transmission of electrical shocks along the nerve, but with the transmission of nerve impulses. By stimulating the nerve with wires of a telephone it has been shown by D'Arsonval that a nerve can transmit upwards of 5,000 vibrations per second, and that the waveforms may be so perfect that the complex electrical waves produced in the telephone by the vowel sounds can be reproduced in the sound of a muscle after having been translated into nerve vibrations and transmitted along a nerve. Such experiments go far in helping us towards a comprehension of the capabilities of nerves in transmitting nerve vibrations of great frequency and complicated wave form; but although they enable us reasonably to suppose that all the fibres of the auditory nerve can transmit nerve vibrations, simple or complex, and with a frequency similar to that of all audible tones, we encounter superlative difficulty in applying such a theory to the sense of sight. In objective sound we have to deal with a comparatively simple wave motion, whose frequency of vibration is not difficult to grasp even at the highest limit of audible sound—about 40,000 vibrations per second. But in objective light the frequency of vibration is so enormous— amounting to hundreds of billions per second—that everyone feels the difficulty of forming any conception of the manner in which different frequencies of ether waves induce differences in colour sensation.

But before passing to colour sense, I wish to allude for a moment to the sense of smell. The terminals of the olfactory nerve in the nose are epithelial cells. It has been recently shown by Von Brunn[1] that in man and other mammals the cells have at their free ends very delicate short hairs, resembling those long known in lower vertebrates. These hairs must be the terminal structures affected by substances that induce smell, and are therefore analogous to the hairs on the terminal cells in our organ of hearing. No one ever suggested that the hairs of the auditory cells can analyse sounds by responding to particular vibrations, and I think it quite as improbable that the hairs on any particular olfactory cell respond to the molecular vibrations of any particular substance. If we follow those who have had recourse to the doctrine of specific activities to explain the production of different smells, we must suppose that at least one special epithelial cell and nerve fibre are affected by each different smelling substance. Considering how great is the variety of smells, and that their number increases with the production of new substances, it would be a somewhat serious stretch of imagination to suppose that for each new smell of a substance yet to emerge from the retort of the chemist there is in waiting a special nerve terminal in the nose. It seems to me far simpler to suppose that all the hairs of the olfactory cells are affected by every smelling substance, and that the different qualities of smell result from differences in the frequency and form of the vibrations initiated by the action of the chemical molecules on the olfactory cells and transmitted to the brain. That hypothesis was, I believe, first suggested by Professor Ramsay,[2] of Bristol, in 1882, and it

[1] Von Brunn, *Archiv für mikroskopische Anatomie*, 1892, Band 39.
[2] Ramsay, *Nature*, 1882, vol. xxvi. p 189.

seems to me the only intelligible theory of smell yet offered. But it must be admitted that a theory of smell such as that advanced by Ramsay involves a more subtle conception of the molecular vibrations in nerve fibrils than is required in the case of hearing. It involves the conception that musk, camphor, and similar substances produce their characteristic qualities of smell by setting up nerve vibrations of characteristic form, and probably of different frequencies. We shall see what bearing this may have on the theory of colour sense, to which I now pass.

No impressions derived from external Nature yield so much calm joy to the mind as our sensations of colour. Pure tones and perfect harmonies produce delightful sensations, but they are outrivalled by the colour effects of a glorious sunset. Without our sense of colour all Nature would appear dressed in bold black and white, or indifferent grey. We would recognise, as now, the beauty of shapely forms, but they would be as the cold engraving contrasted with the brilliant canvas of Titian. The beautiful tints we so readily associate with natural objects are all of them sensations produced in our brain. Paradox though it appear, all Nature is really in darkness. The radiant energy that streams from a sun is but a subtle wave-motion, which produces the common effects of heat on all bodies, dead or living. It does not dispel the darkness of Nature until it falls on a living eye, and produces the sense of light. Objective light is only a wave motion in an etherial medium ; subjective light is a sensation produced by molecular vibration in our nerve apparatus.

The sensory mechanism concerned in sight consists of the retina, the optic nerve, and the centre for visual sensation in the occipital lobe of the brain. In the vertebrate eye the fibres of the optic nerve spread out in the inner part of the retina, and are connected with several layers of ganglionic cells placed external to them. The light has to stream through the fibres and ganglionic layers to reach the visual cells—that is, the nerve terminals placed in the outer part of the retina. They may be regarded as epithelial cells, whose peripheral ends are developed into peculiar rod and cone-shaped bodies, while their central ends are in physiological continuity with nerve fibrils. Each rod and cone consists of an inner and an outer segment. The outer segment is a pile of exceedingly thin, transparent, doubly refractive discs, colourless in the cone, but coloured pink or purple in the rod. In man, the inner segment of both rod and cone is colourless and transparent. Its outer part appears to be a compact mass of fine fibrils that pass imperceptibly into the homogeneous-looking protoplasm in the shaft of the cell. Owing to the position of the rods and cones, the light first traverses their inner, then their outer segments, and its unabsorbed portion passes on to the adjacent layer of dark-brown pigment cells by which it is absorbed. It is not necessary for me to discuss the possible difference of function between the rods and cones. I may simply say that in the central part of the yellow spot of the retina, where vision is most acute, and from which we derive most of our impressions of form and colour, the only sensory terminals are the cones. A single cone can enable us to obtain a distinct visual impression. If two small pencils of light fall on the same cone the resulting sensory impression is single. To produce a double impression the luminous pencils must fall on at least two cones. That shows how distinct must be the path pursued by the nerve impulse from a visual cell in the eye to a sensory cell in the brain. The impulses from adjacent terminals must pursue their own discrete paths through the apparent labyrinth of nerve fibrils and ganglion cells in the retina to the fibres of the optic nerve. How these facts bear on the theory of colour sense will presently be apparent. Meantime I pass to the physical agent that stimulates the retina.

When a beam of white light is dispersed by a prism or diffraction grating, the ether-waves are spread out in the order of their frequency of undulation. The undulations of radiant energy extend through a range of many octaves, but those able to stimulate the retina are comprised within a range of rather less than one octave, extending from a frequency of about 395 billions per second at the extreme red to about 757 billions at the extreme violet end of the visible spectrum. The ultra-violet waves in the spectrum of sunlight extend through rather more than

half an octave. Although mainly revealed, by their chemical effects, they are not altogether invisible : their colour is bluish-grey. The only *optical*—that is, strictly *physical*—difference between the several ether-waves in the visible or invisible spectrum is frequency of undulation, or, otherwise expressed, a difference in wave-length. The *chromatic*—that is, the colour-producing—effects of the ether-waves depend on their power of exciting sensations of colour, which vary with their frequency of undulation

Although the retina is extremely sensitive to differences in the frequency of ether-waves, it is not equally so for all parts of the spectrum. In the red and blue portions, the frequency varies considerably without producing marked difference of colour effect, but in the region of yellow and green, comparatively slight variations in frequency produce appreciable differences of colour sensation. One striking difference between the effect of ether-waves on the eye and sound-waves on the ear is the absence of anything corresponding to the octave of tone sensation. The ether-waves in the ultra-violet, which have twice the frequency of those of the red end of the spectrum, give rise to no sense of redness, but merely that of a bluish-grey. Even within the octave there are no harmonies or discords of colour sense corresponding to those of tone sensation.

Colours are commonly defined by three qualities or constants,—hue, purity, and brightness. Their hue depends upon the chromatic effect of frequency of undulation or wave length. Their purity or saturation depends on freedom from admixture with sensations produced by other colours or by white light. Their brightness or luminosity depends on the degree to which the sensory mechanism is stimulated. The loudness of sound depends on the amount of excitement produced in the auditory mechanism by the amplitude of sound waves, but a sound with small amplitude of undulation may seem loud when the nerve apparatus is unduly sensitive. The brightest colour of the spectrum is orange-yellow, but it does not follow that the amplitude or energy of the ether-waves is greater than in the region of dull red. There is no physical evidence of greater amplitude in the orange-yellow, and its greater luminosity is no doubt purely subjective, and arises from the greater commotion induced in the sensory mechanism.

The theory of colour sense long ago proposed by Sir Isaac Newton [1] is now commonly treated with what seems to me very undeserved neglect. Newton supposed that the rays of light induce vibrations in the retina which are transmitted by its nerve to the sensorium, and there induce different colour sensations according to the length of the incoming vibrations—the longest producing sensations of red and yellow, the shortest blue and violet, those of medium length a sense of green, and a mixture of them all giving a sense of whiteness. At the beginning of this century Thomas Young proposed a theory which seems to have been intended as a modification of that suggested by Newton rather than as a substitute for it. Young supposed that the ether-waves induce vibrations in the retina ' whose frequency must depend on the constitution of its substance; but as it is almost impossible to conceive that each sensitive point of the retina contains an infinite number of particles, each capable of vibrating in unison with every possible undulation, it becomes necessary to suppose the number limited to three primary colours, red, yellow, and blue, and that each sensitive filament of the nerve may consist of three portions, one for each principal colour.' [2] Soon afterwards he substituted green for yellow, and violet for blue, so that he came to regard red, green, and violet as the three fundamental colour sensations, by mixture of which in varying proportions all other colours, including white, are produced. Young believed that his suggestion ' simplified the theory of colours, and might therefore be adopted with advantage until found inconsistent with any of the phenomena.'

Young's trichromatic theory of colour sense was adopted by Clerk-Maxwell and Von Helmholtz, and underwent important amplification. Helmholtz suggested

[1] Thomas Young, ' On the Theory of Light and Colours,' *Phil Trans Lond*, 1802, p. 12.
[2] *Ibid.*

that the three sets of fibres supposed by Young to exist in the optic nerve are connected with three sets of terminals in the retina; that each terminal contains a different visual substance capable of being decomposed by light; that when the substance in the red nerve terminal undergoes chemical change its nerve fibre is stimulated, and the excitement travels to a cell in the brain by whose specific activity the sensation of red arises. In like manner, when the visual substances in the green and violet terminals are decomposed, nerve impulses travel through different fibres to different cells in the vision centre, by whose specific activities the sensations of green and violet arise. With Helmholtz there was no question as to difference in quality of sensation depending on difference in frequency of nerve vibration arriving in the sensorium; no such hypothesis was entertained by him either for tone or for colour sensation. With sight, as with hearing, he supposed that the function of frequency of undulation virtually stops at the nerve terminals in the eye and ear, and that the frequency of undulation of the physical agent has no correlative in the quality of motion passing from the receiving terminal to the sensory cell. He believes that the different frequencies of ether-waves simply excite chemical changes in different nerve terminals. He expressly states [1] that the molecular commotion in the nerve fibres for red, green, and violet is identical in kind, and that its different effects depend on the specific activities of the different cells to which it passes in the sensorium. It is evident that Helmholtz entirely dismissed the Newtonian theory of the production of different qualities of colour sense, and substituted for it the doctrine of his own great teacher, Johannes Müller.

The theory of Young and Helmholtz offers an explanation of so many facts, and has at the same time provoked so much criticism, that I must enter more fully into some of its details. On this theory, the sense of white or grey is supposed to result from a simultaneous and duly balanced stimulation of the red, green, and violet terminals. The red terminals are supposed to be excited chiefly by the longer waves in the region of the red and orange, but also by the shorter undulations extending as far as Fraunhofer's line F at the beginning of the blue. In like manner, the green terminals are excited chiefly by the waves of medium length, and to a less extent by the waves extending to about C in the red, and by the shorter waves extending to G in the violet. The violet terminals are stimulated most powerfully by the shorter undulations between F and G, but also by the longer ones reaching as far as D in the yellow; therefore, optically homogeneous light from any part of the spectrum, except its extreme ends, does not usually give rise to a pure colour sensation; all three primary sensations are present, and consequently the colour inclines towards white—the more, the stronger the light.

The experimental facts in support of Young's theory are familiar to all who have studied physics. Compound colour sensations may be produced by causing light of different wave lengths to fall simultaneously or in rapid succession on the same part of the retina. The commonest experimental device is to rapidly whirl discs with sectors of different colours, and observe the results of the mixed sensations; or to cause the images of coloured wafers or papers to fall simultaneously on the retina by Lambert's method; or to transmit light through glass of different colours, and cause the different rays to fall on the same surface; or to mix pure homogeneous light from different parts of the spectrum. For obvious reasons, the last method yields the most trustworthy results. We cannot, by any mixture of homogeneous light from different parts of the spectrum, obtain a pure red or green sensation, and, according to Helmholtz, the same holds true of violet. On the other hand, a mixture of homogeneous rays from the red and green parts produces orange or yellow, according to the proportions employed. A mixture of rays from the green and violet gives rise to intermediate tints of blue, and a mixture of red and violet light produces purple. Therefore, Young regarded red, green, and violet as primary sensations, and orange, yellow, and blue—just as much as purple—he regarded as secondary or compound sensations. Helmholtz discovered that to obtain a sense of white or grey it is not necessary to mingle rays from the

red, green, and violet portions of the spectrum. He found that he could obtain a white sensation by mixing only *two* optically homogeneous rays from several parts of the right and left halves of the spectrum. The pairs of spectral colours which he found complementary to each other are, red and greenish-blue, orange and cyan-blue, yellow and ultramarine-blue, greenish-yellow and violet ; the complement for pure green being found not in any homogeneous light, but in purple— a mixture of red and violet. The complementary colours may be arranged in a circle, with the complementaries in each pair placed opposite one another. Of course, the circle cannot be completed by the colours of the spectrum ; purple must be added to fill in the gap between the red and violet  Helmholtz found no constant ratios between the wave lengths of homogeneous complementaries ; and it is a striking fact that, while a mixture of the green and red, or of the green and violet, undulations gives rise to a sensation such as could be produced by rays of intermediate wave length, no such effect follows the mingling of rays from opposite halves of the spectrum. Pure green, with a wave length of 527 millionths of a millimetre, marks the division between the right and left halves. The mixture of blue from the right and yellow from the left side does not produce the intermediate green, but a sensation of white. A mixture of blue or violet and red produces not green, but its complementary—purple. On the trichromatic theory, the sense of white produced by the mingling of any of these two colours is simply regarded as the result of a balanced stimulation of the red, green, and violet terminals.

But Young's theory is beset with serious difficulties. It implies the existence of three sets of terminals in the retina, and these must all be found in the central part of the yellow spot where cones alone are present. Three sets of cones there, would be necessary to respond to the red, green, and violet light, and a colourless pencil of light could not be seen uncoloured, unless it fall on three cones, which we know is not the case. Therefore, if there are three different terminals, they must, in the human retina at all events. be found in every single cone in the yellow spot. But I cannot believe it possible that within a single cone there can be three sets of fibrils capable of simultaneous stimulation in different degrees, and of transmitting impulses through three different fibres to three different cells in the brain ; the anatomical difficulty is therefore great, and I am unable to see how it can be surmounted.

The phenomena of colour-blindness also offer great difficulty  In several cases of apoplectic seizure it has happened that the centre for vision on both sides of the brain has been completely or partially paralysed by the extravasated blood. In such cases the sense of colour may be entirely lost either for a time or permanently, while the sense of light and form remain—although impaired. The loss of colour sense in some cases has been found complete in both eyes ; in most of the recorded cases the loss of colour sense was limited to the right or left halves of both eyes , that is, if the lesion affected the vision centre on the right side of the brain, the right halves of both eyes were blind to all colours  That illustrates the now well-known fact that a sense of light does not imply a sense of colour. The colour sense probably involves a more highly refined action of the sensory cell than the mere sense of light and form, and is on that account more liable to be lost when the nutrition of the sensory cell is interfered with. In the normal eye the peripheral zone of the retina is totally blind to colour. If you turn the right eye outwards, close the left, and then move a strip of coloured paper from the left to the right in front of the nose, the image of the paper will first fall on the peripheral zone of the retina, and its form will be seen, though indistinctly, but not its colour. It is difficult to say in that case whether the colour-blindness is due to the state of the retina or to that portion of the vision centre in the brain associated with it. The absence of cones from the peripheral part of the retina has been assigned as the cause, but it is much more probable that the portion of the vision centre associated with the periphery of the retina, being comparatively little used, is less highly developed for form sensation, and not at all for colour sense. It is evident that the production of a sense of white or grey in the absence of all colour sense is not to be explained on the theory that it results from a balanced stimulation of red, green, and violet nerve terminals.

I need scarcely say that colour-blindness has attracted a large share of attention, not only because of its scientific interest, but still more on account of its practical importance in relation to the correct observation of coloured signals. In 1855 the late Professor George Wilson,[1] of this city, called attention to the growing importance of the subject  Some years ago Professor Holmgren made an elaborate statistical inquiry regarding it at the instance of the Swedish Government, and lately it has been investigated by a committee of the Royal Society of London, who have quite recently published their report.[2]

Although colour-blindness occasionally results from disease of the brain, retina, or optic nerve, it is usually congenital.  Total colour-blindness is extremely rare, but partial colour-blindness is not uncommon.  It occurs in about 4 per cent of males, but in less than 1 per 1,000 of females.  Its most common form is termed red-green blindness, in which red and green sensations appear to be absent.  So far as I can find, the first full and reliable account of the state of vision in red-green blindness is that given in 1859 by Mr. Pole,[3] of London, from an examination of his own case, which appears to be a typical one.  The state of his vision is dichromic ; his two-colour sensations are yellow and blue.  The red, orange, and yellowish-green parts of the spectrum appear to him yellow of different shades.  Greenish-blue and violet appear blue, and between the yellow and blue portions of the spectrum, as it appears to him, there is a colourless grey band in the position of the full green of the ordinary spectrum.  This neutral band is seen in the spectrum in all cases of dichromatic vision.  It may appear white or grey according to the intensity of the light, and it apparently results from an equilibrium of the two sensations ; no such band is seen in the spectrum by a normal eye.  Mr. Pole, in the account of his case given now three and thirty years ago, considered it impossible to explain his dichromatic vision on the commonly received theory that his sense of red is alone defective, and that his sense of yellow is a compound of blue and green.  He believed his green quite as defective as his red sensation, and that yellow and blue are quite as much entitled to be considered fundamental sensations as red and green.  He suggested that in normal colour vision  there are at least four primary sensations—red and green, yellow and blue.  Professor Hering is commonly accredited with the four-colour theory, but it was previously suggested by Pole.[4]

A year after Pole's paper appeared, Clerk-Maxwell[5] published his 'celebrated paper on the theory of compound colours, to which he appended an account of his observations on a case of what he believed to be red-blindness, but which we now know must have been red-green blindness.  The spectrum appeared dichromatic, its only colours being yellow and blue.  His description of the case does not materially differ from that given by Pole ; but Clerk-Maxwell believed in the trichromatic theory of normal vision, and that red-green and blue are the three primary sensations ; consequently he supposed that the yellow sensation of a red blind person is not pure yellow, such as normally results from a mixture of red and green, but a yellow in some way composed of a mixture of blue and green. The copy we have made of his curves will enable you to understand his meaning, but I question if they will enable you to comprehend the yellow sensation of the red blind person, if Young's theory be true, that yellow is a sensation compounded of red and green.

It is evident that much depends on the question, Is the yellow sensation of a red-green blind person the same as that of normal vision ?  For many years it was impossible to give a definite answer to that question, but the answer can now be given, as we shall immediately see.  Colour-blindness is frequently hereditary, and two or three cases are known in which the defective colour sense was limited to one eye, while in the other eye colour vision was normal.  In such a case observed

[1] Wilson, *Researches on Colour-Blindness*, Edinburgh, 1855
[2] 'Report of the Committee on Colour Vision,' *Proc. Roy. Soc. Lond.*, July 1892.
[3] W. Pole, ' On Colour-Blindness,' *Phil. Trans.* 1859, vol. cxlix. p. 323.
[4] *Ibid.* p. 331.
[5] Clerk-Maxwell, ' On the Theory of Compound Colours,' &c., *Phil. Trans.* 1860, vol. cl p. 57.

by Professor Hippel, of Giessen, there was red-green blindness in one eye. Holmgren, who examined Hippel's case, has published an account of it.[1] With one eye all the colours of the spectrum were seen as others see them, but to the other eye the spectrum had only two colours with a narrow white band between them at the junction of the blue and green. The yellow seen by the eye with the red-green defect had a greenish tinge like that of a lemon, but in other respects the observations confirmed Pole's account of his own case.

Hippel's case seems to me important for another reason. By some it is believed that congenital colour defect is due to the brain. If there had been defective colour sense on one side of the brain, it would not have implicated the whole of one eye, but the half of each eye. Its limitation to one eye, therefore, seems to me to suggest that the fault was in the eye rather than in the brain.

Another interesting fact in this relation is that in every normal eye, just behind the peripheral zone of total colour-blindness, to which I have already referred, there is a narrow zone in which red and green sensations are entirely wanting, while blue and yellow sensations are normal. Possibly the red-green defect is due to an imperfectly developed colour sense in the portion of the vision centre connected with that zone of the retina, but Hippel's case seems to me to show that such defect might be on the retina.

It has probably already struck you that red-green blindness is really blindness to red, green, and violet, that Young's three primary sensations appear to be absent, and the two remaining colours are those which he regarded as secondary compounds of his primaries.

That, however, is not all that is revealed by colour-blindness. There is at least another well-known though rare form in which a sense of yellow, blue, and violet is absent, and the only colour sensations present are red and green. The defect is sometimes termed violet blindness, but the term is somewhat misleading. It is much more in accordance with the fact to term it yellow-blue blindness; indeed, we would define it precisely by terming it yellow-blue-violet blindness. Holmgren[2] has recorded a unilateral case of that defect analogous to Hippel's case of unilateral red-green defect; we therefore know definitely how the spectrum appears to such a person. In the case referred to all the colours of the spectrum were seen with the normal eye, but to the other eye the spectrum had only two colours, red and green. The red colour extended over the whole left side of the spectrum to a neutral band in the yellow-green, a little to the right of Fraunhofer's line D. All the right side of the spectrum was green as far as the beginning of the violet, where it 'ended with a sharp limit (about the line G.')

If you turn to the Report of the Royal Society's Committee[3] on Colour Vision, you will find the spectrum as it appears to yellow-blue-violet blind persons. The plate agrees with the description of Holmgren's case already given; but you will not find a representation of the spectrum as it appears to those who are red-green blind, and as described by Pole, Clerk-Maxwell, and others. In place of it you will find two dichromic spectra, one with a red and blue half said to be seen by a green blind, the other with a green and a blue half said to be seen by a red blind person. We have copied the spectra for your inspection, and you will observe that yellow does not appear in either of them. I do not for a moment pretend to criticise these spectra from any observations of my own; I am aware Holmgren maintains that red-and-green blindness may occur separately; but, on the other hand, Dr. George Berry, an eminent opthalmologist, has assured me that he has always found them associated. The same statement is made by Stilling, by Hering, and others, who will no doubt have their own criticisms to offer.

Of the various methods of testing colour vision, that suggested by Holmgren is most commonly employed. The individual is mainly tested with regard to his sense of green and red. He is shown skeins of wool, one pale green, another pink or purple, and a third bright red, and he is asked to select from a heap of coloured

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[1] F. Holmgren, 'How do the Colour-Blind see the Different Colours?' *Proc. Roy. Soc. Lond.* 1881, vol. xxxi p. 302.

[2] *Ibid.* p. 306.    [3] See Reference 8, Plate I., No. 4.

wools, laid on a white cloth, the colours that appear to him to match those of the several tests. We have arranged such test skeins for your inspection, and have placed beneath each of them the colours which a red-green blind person usually selects as having hues similar to those of the test. It is startling enough to find brown, orange, green, and grey confused with bright red; pale red, orange, yellow, and grey confused with green; blue, violet, and green confused with pink; but these confusions have all their explanation in the fact that the red-green blind have only two colour sensations—yellow and blue

We have now to show you another and far more beautiful method of ascertaining what fundamental colour sensations are absent in the colour-blind. It is the method of testing them by what Chevreul long ago termed *simultaneous contrast*. If in a semi-darkened room we throw a beam of coloured light on a white screen and interpose an opaque object in its path, the shadow shows the complementary colour. If the light be red, the shadow appears green-blue; if it be green, the shadow appears purple or red according to the nature of the green light employed. If the light is yellow, the shadow is blue; if it is blue, the shadow is yellow. We must remember that the part of the screen on which the shadow falls is not entirely dark; a little diffuse light falls on the retina from the shadowed part, so that the retina and vision centre are slightly stimulated, whereby the image of the shadow.

The experiment can be rendered still more striking, though at the same time a little more complicated, by using two oxyhydrogen lamps and throwing their light on the same portion of the screen. If a plate of coloured—say ruby—glass is held before one of the lamps, and an opaque object such as the head of a T-square is placed in the path of both lights, the shadow cast by the white light falls on a surface illuminated by a red light, and shows a deep red far more saturated than the surrounding surface of the screen where the red and white lights fall. The shadow cast by the red light shows the complementary bluish green; and the contrast of the two is exceedingly striking.

These experiments which we have shown you point to some subtle physiological relations between complementary colours. A colour sensation produced in one part of the vision apparatus forces, so to speak, the neighbouring part, which is relatively quiescent, to produce the complementary colour subjectively, but not necessarily on the whole vision centre; for if the inducing light be a spot, say the opening of the lantern covered with red tissue paper and focussed with a lens between it and the screen, there is a broad halo of complementary green seen around the red spot but not filling the whole field of vision; therefore, the subjective complementary sensation appears to be induced by an influence extending from the stimulated spot in the retina, or—as is much more probable—in the vision centre.

Now I imagine many of you have already anticipated the question, What information can simultaneous contrast give regarding the fundamental sensations of the colour-blind? From an extended series of observations Dr. Stilling,[1] of Cassel, has ascertained that if a person cannot distinguish between red and green, no complementary colour appears in the shadow when the inducing light is red or green, but if the inducing light is yellow or blue the proper complementary appears in the shadow. If a person was blind to red, he never found the complementary green appear; if he was blind to green, he never found the complementary red appear. When the inducing light appeared colourless, the shadow was also colourless. Stilling therefore concluded that either the sensations of red and green or of blue and yellow were wanting at the same time or all colour sense was absent. It is difficult to see how these results are to be harmonised with the conclusions arrived at by the Committee of the Royal Society.

Facts such as these are regarded by some as lending support to the theory of colour sense proposed by Professor Hering, of Prague.[2] He supposes that the

[1] J. Stilling, 'The Present Aspect of the Colour Question,' *Archives of Ophthalmology*, 1879, viii p. 164.

[2] E. Hering, *Zur Lehre vom Lichtsinne*, 2nd ed. Vienna, 1878.

diversity of our visual perceptions arises from six fundamental sensations constituting three pairs—white and black, red and green, yellow and blue. The three pairs of sensations are supposed to arise from chemical changes in three visual substances not confined to the retina, but contained also in the optic nerve and in the vision centre.[1]   He imagines that a sense of white results from *decomposition* induced in a special visual substance by all visible rays, and that the *restitution* of the same substance produces a sense of black. The sensations of the red and green pair are supposed to arise, the one from decomposition, the other from restitution of a second substance; while yellow and blue are supposed to result from decomposition and restitution of a third substance. From our knowledge of photo-chemical processes we can readily suppose that light induces chemical change in the visual apparatus; but that the wave-lengths in the red and yellow parts of the spectrum induce *decomposition*, while the wave-lengths in the green and blue induce *restitution* of substances, it is difficult to believe. How such a visual mechanism could work it would be difficult to comprehend, for example, if we look at a bright red light for a few moments and then close our eyes, the sensation remains for a time, but changes from red to green and then slowly fades away. According to Hering's theory, the green after-sensation results from the restitution of a substance decomposed by the red light. But if we reverse the experiment by looking at a bright *green* light and then closing our eyes, the after-sensation changes to *red*   The theory in question would require us to suppose that the green light builds up a visual substance which spontaneously decomposes when the eyes are closed, and so produces the red after-image   I confess that such a hypothesis seems to me incredible. Another remarkable feature of Hering's theory is that colours termed *complementary* ought to be termed *antagonistic*,[2] because they are capable of producing a colourless sensation when mingled in due proportions. If the complementary colours yellow and blue could, when mixed, produce black, they might well be named 'antagonistic,' but since their combined effect is a sense of whiteness, and since the addition of them to white light increases its luminosity, it seems very difficult to comprehend on what ground the term *antagonistic* should be substituted for *complementary*. I confess I am quite unable to follow Hering when he supposes that three pairs of mutually antagonistic chemical processes are produced in the retina when white light falls on it, that these processes are all continued on through the optic nerve into the vision centre, and there give rise to our different light and colour sensations.

It must be admitted that the production of nerve impulses within the terminals in the retina is almost as obscure as ever. It is still the old question, Does light stimulate the optic terminals by inducing vibration, or by setting up chemical change?  Whichever view we adopt, it seems to me necessary to suppose that all the processes for the production of nerve impulses can take place in one and the same visual cell, and are transmitted to the brain through the same nerve fibre

I referred to the sense of smell because it seems to me that we cannot in that case escape from the conclusion that the different sensations arise from different molecular stimulations of the olfactory terminals, transmitting different frequencies and forms of nerve undulations.

From Lippmann's recent researches on the photography of colour[3] it appears that all parts of the spectrum can now be photographed on films of albumino-bromide of silver to which two aniline substances, azaline and cyanine, have been added. It seems, therefore, reasonable to suppose that a relatively small number of substances could enable all the rays of the visible spectrum to affect the retina. Helmholtz believes that three visual substances would suffice; but if the primary sensations are to be regarded as four—red, green, yellow, and blue—at least four visual substances appear to be necessary; and I think we must assume that all of them are to be found in the same visual cell in the retina, and that the nerve impulses which their decompositions give rise to are all transmitted through the

[1]  Hering, *ibid.* p. 75.
[2]  Hering, *ibid* p 121.
[3]  G. Lippmann, ' On the Photography of Colour,' *Comptes Rendus*, 1892, tome 111, p. 961.

same optic fibres to the brain cells, there to produce a sense of uncoloured or coloured light. Evidently there is nothing novel in such a hypothesis; it is essentially a return to that long ago suggested by Newton. The only difference is that light is supposed to induce photo-chemical changes in the retina, as Von Helmholtz suggested, instead of mere mechanical vibration, as Newton supposed. But if in the sense of smell nerve undulations are induced by mechanical vibrations of molecules acting on delicate hairs at the ends of cells, would it, after all, be unreasonable to suppose that within each visual cell there are different kinds of molecules that vibrate in different modes when excited by ether waves? Four or five sets of such molecules in each terminal element in the retina would probably be sufficient to project successively or simultaneously special forms of undulations through the optic nerve, to induce colour sensations differing according to the wave form of the incoming nerve undulation. It seems to me that the question becomes narrowed down to this: Do the nerve impulses arise from mere vibration or from chemical change in the molecules of the nerve terminal? The photo-chemical hypothesis has much in its favour. We know how rapidly light can induce chemical change in photographic films, and we know that light induces chemical change in the vision-purple in the outer segments of the rod cells in the retina. The fact that the cones contain no vision-purple is no argument against the theory, for the inner segment of both rod and cone is by many regarded as the true nerve terminal, and there is no vision-purple in either of them. The visual substances in the cones, at all events, are colourless, and the existence of them as substances capable of producing nerve impulses by chemical decomposition is as yet only a speculation awaiting proof. The fatigue of the retina produced by bright light is best explained on a chemical theory, but it could also be explained on a mechanical theory, for we must remember that, even if the nerve impulses produced in the visual cells were merely a translation of the energy of light into vibration of nerve molecules, the nerve impulse has to pass through layers of ganglionic cells before reaching the fibres of the optic nerve, and in these cells it probably always induces chemical change. The phenomena of partial colour-blindness could be explained on a photo-chemical theory by supposing that it arises from the absence of the substances required to produce the wave forms necessary for the colour sensation which is defective, but the total colour-blindness at the anterior part of the retina is evidently a difficulty. How could we have a sense of light from that portion of the retina if all the visual substances are absent? That is one of the reasons why Hering supposed that a special visual substance is present everywhere in the retina, which by decomposition gives rise to a sense of light as distinguished from colour. But even on the hypothesis I am pursuing, it is not necessary to suppose that all visual substance is absent, for the colour-blindness in the front of the retina could be explained by supposing that colour perception has not been developed in the corresponding portion of the vision centre, and consequently all nerve impulses coming from that part of the retina produce a sense of light.

If the photo-chemical theory is entertained, it seems necessary to suppose that there is some singular relation between the pairs of substances which respectively give rise to red and green, and yellow and blue, seeing that both members of a pair frequently, if not always, fail together.

It seems to me that the great difficulty arises when we consider the puzzling phenomena of contrast. If light of a particular wave length decomposes a special substance, and gives rise to say a sense of red, why does the complementary bluish-green sensation appear in the vision centre around the spot in which the red sensation arises? If the induced colour were a pure green, one might attempt to explain it by supposing that a sympathetic change had been induced in a substance closely related to that suffering decomposition by the objective light, but no such simple explanation is admissible; the complementary contrast of red is not green, but a mixture of green and blue. The inadmissibility of such an explanation becomes still more apparent if we take pure green as the inducing colour—the complementary contrast that appears is purple, which involves a blue or violet, as well as a red sensation. It matters not what inducing colour sensation we adopt, the induced contrast is always the complementary required to make a sense of white.

George Wilson[1] long ago suggested that the simultaneous contrast probably arises from a 'polar manifestation of force;' indeed, he regarded it as a 'true, though unrecognised, manifestation of polarity.' It is enough to mention that interesting suggestion, but I must not pursue it, for we are dealing with a problem that has as yet baffled the wit of man.

I have endeavoured to place before you a subject that involves physical and physiological considerations of extreme difficulty. I have not attempted to solve the difficulties, but rather to show their nature. I have not found it an agreeable task to point out the shortcomings of theories advanced by those for whom I have the deepest regard; but in the progress of scientific thought it is especially necessary to keep our minds free from the thraldom of established theory, for theories are but the leaves of the tree of science; they bud and expand, and in time they fade and fall, but they enable the tree to breathe and live. If this address has been full of speculation, I trust you will allow that the scientific use of the imagination is more stimulating to thought than a mere statement of facts. I could have wished that time had allowed me to have alluded to colour sense in animals, and the question of its evolution, but I have already detained you too long, and can now only thank you for your patience

[1] Wilson, *Researches on Colour-Blindness*, Edinburgh, 1855, p. 179.

# ADDRESS

## TO THE

# GEOGRAPHICAL SECTION

### OF THE

# BRITISH ASSOCIATION.

### BY

### Professor JAMES GEIKIE, LL.D., D.C.L., F.R.SS.L. & E., F.G.S

#### PRESIDENT OF THE SECTION.

AMONGST the many questions upon which of late years light has been thrown by deep-sea exploration and geological research, not the least interesting is that of the geographical development of coast-lines. How is the existing distribution of land and water to be accounted for? Are the revolutions in the relative position of land and sea, to which the geological record bears witness, due to movements of the earth's crust or of the hydrosphere? Why are coast-lines in some regions extremely regular, while elsewhere they are much indented? About 150 years ago the prevalent belief was that ancient sea-margins indicated a formerly higher ocean-level. Such was the view held by Celsius, who, from an examination of the coast-lands of Sweden, attributed the retreat of the sea to a gradual drying up of the latter. But this desiccation hypothesis was not accepted by Playfair, who thought it much more likely that the land had risen. It was not, however, until after Von Buch had visited Sweden (1806–1808), and published the results of his observations, that Playfair's suggestion received much consideration. Von Buch concluded that the apparent retreat of the sea was not due to a general depression of the ocean-level, but to elevation of the land—a conclusion which subsequently obtained the strong support of Lyell. The authority of these celebrated men gained for the elevation theory more or less complete assent, and for many years it has been the orthodox belief of geologists that the ancient sea-margins of Sweden and other lands have resulted from vertical movements of the crust. It has long been admitted, however, that highly flexed and disturbed strata require some other explanation. Obviously such structures are the result of lateral compression and crumpling. Hence geologists have maintained that the mysterious subterranean forces have affected the crust in different ways. Mountain-ranges, they conceive, are ridged up by tangential thrusts and compression, while vast continental areas slowly rise and fall, with little or no disturbance of the strata. From this point of view it is the lithosphere that is unstable, all changes in the relative level of land and sea being due to crustal movements. Of late years, however, Traut-schold and others have begun to doubt whether this theory is wholly true, and to maintain that the sea-level may have changed without reference to movements of the lithosphere. Thus Hilber has suggested that sinking of the sea-level may be due, in part at least, to absorption, while Schmick believes that the apparent elevation and depression of continental areas are really the results of grand secular movements of the ocean. The sea, according to him, periodically attains a high level in each hemisphere alternately, the waters being at present heaped up in the southern hemisphere. Professor Suess, again, believing that in equatorial regions the sea is, upon the whole, gaining on the land, while

in other latitudes the reverse would appear to be the case, points out that this is in harmony with his view of a periodical flux and reflux of the ocean between the equator and the poles.   He thinks that we have no evidence of any vertical elevation affecting wide areas, and that the only movements of elevation that take place are those by which mountains are upheaved.   The broad invasions and transgressions of the continental areas by the sea, which we know have occurred again and again, are attributed by him to secular movements of the hydrosphere itself.

Apart from all hypothesis and theory, we learn that the surface of the sea is not exactly spheroidal.   It reaches a higher level on the borders of the continents than in mid-ocean, and it varies likewise in height at different places on the same coast.   The attraction of the Himalaya, for example, suffices to cause a difference of 300 feet between the level of the sea at the delta of the Indus and on the coast of Ceylon.   The recognition of such facts has led Penck to suggest that the submergence of the maritime regions of North-west Europe and the opposite coasts of North America, which took place at a recent geological date, and from which the lands in question have only partially recovered, may have been brought about by the attraction exerted by the vast ice-sheets of the Glacial Period.   But, as Drygalski, Woodward, and others have shown, the heights at which recent marine deposits occur in the regions referred to are much too great to be accounted for by any possible distortion of the hydrosphere.   The late James Croll had previously endeavoured to show that the accumulation of ice over northern lands during glacial times would suffice to displace the earth's centre of gravity, and thus cause the sea to rise upon the glaciated tracts.   More recently other views have been advanced to explain the apparently causal connection between glaciation and submergence, but these need not be considered here.

Whatever degree of importance may attach to the various hypotheses of secular movements of the sea, it is obvious that the general trends of the world's coast-lines are determined in the first place by the position of the dominant wrinkles of the lithosphere.   Even if we concede that all ' raised beaches,' so called, are not necessarily the result of earth-movements, and that the frequent transgressions of the continental areas by oceanic waters in geological times may possibly have been due to independent movements of the sea, still we must admit that the solid crust of the globe has always been subject to distortion.   And this being so, we cannot doubt that the general trends of the world's coast-lines must have been modified from time to time by movements of the lithosphere.

As geographers we are not immediately concerned with the mode of origin of those vast wrinkles, nor need we speculate on the causes which may have determined their direction.   It seems, however, to be the general opinion that the configuration of the lithosphere is due simply to the sinking in and crumpling up of the crust on the cooling and contracting nucleus.   But it must be admitted that neither physicists nor geologists are prepared with a satisfactory hypothesis to account for the prominent trends of the great world-ridges and troughs.   According to the late Professor Alexander Winchell, these trends may have been the result of primitive tidal action.   He was of opinion that the transmeridional progress of the tidal swell in early incrustive times on our planet would give the forming crust structural characteristics and aptitudes trending from north to south. The earliest wrinkles to come into existence, therefore, would be meridional or submeridional, and such, certainly, is the prevalent direction of the most conspicuous earth-features.   There are many terrestrial trends, however, as Professor Winchell knew, which do not conform to the requirements of his hypothesis; but such transmeridional features, he thought, could generally be shown to be of later origin than the others.   This is the only speculation, so far as I know, which attempts, perhaps not altogether unsuccessfully, to explain the origin of the main trends of terrestrial features.   According to other authorities, however, the area of the earth's crust occupied by the ocean is denser than that over which the continental regions are spread.   The depressed denser part balances the lighter elevated portion.   But why these regions of different densities should be so distributed no one has yet told us.   Neither does Le Conte's view, that the continental areas and

the oceanic depressions owe their origin to unequal radial contraction of the earth in its secular cooling, help us to understand why the larger features of the globe should be disposed as they are.

Geographers must for the present be content to take the world as they find it. What we do know is that our lands are distributed over the surface of a great continental plateau of irregular form, the bounding slopes of which plunge down more or less steeply into a vast oceanic depression. So far as geological research has gone, there is reason to believe that these elevated and depressed areas are of primeval antiquity—that they antedate the very oldest of the sedimentary formations. There is abundant evidence, however, to show that the relatively elevated or continental area has been again and again irregularly submerged under tolerably deep and wide seas. But all historical geology assures us that the continental plateau and the oceanic hollows have never changed places, although from time to time portions of the latter have been ridged up and added to the margins of the former, while ever and anon marginal portions of the plateau have sunk down to very considerable depths. We may thus speak of the great world-ridges as regions of dominant elevation, and of the profound oceanic troughs as areas of more or less persistent depression. From one point of view, it is true, no part of the earth's surface can be looked upon as a region of dominant elevation. Our globe is a cooling and contracting body, and depression must always be the prevailing movement of the lithosphere. The elevation of the continental plateau is thus only relative. Could we conceive the crust throughout the deeper portions of the oceanic depression to subside to still greater depths, while at the same time the continental plateau remained stationary, or subsided more slowly, the sea would necessarily retreat from the land, and the latter would then appear to rise. It is improbable, however, that any extensive subsidence of the crust under the ocean could take place without accompanying disturbance of the continental plateau; and in this case the latter might experience in places not only negative but positive elevation. During the evolution of our continent, crustal movements have again and again disturbed the relative level of land and sea, but since the general result has been to increase the land surface and to contract the area occupied by the sea, it is convenient to speak of the former as the region of dominant elevation, and of the latter as that of prevalent depression. Properly speaking, both are sinking regions, the rate of subsidence within the oceanic trough being in excess of that experienced over the continental plateau. The question of the geographical development of coast-lines is therefore only that of the dry lands themselves.

The greater land masses are all situated upon, but are nowhere co-extensive with, the area of dominant elevation, for very considerable portions of the continental plateau are still covered by the sea. Opinions may differ as to which fathoms-line we should take as marking approximately the boundary between that region and the oceanic depression; and it is obvious, indeed, that any line selected must be arbitrary and more or less misleading, for it is quite certain that the true boundary of the continental plateau cannot lie parallel to the surface of the ocean. In some regions it approaches within a few hundreds of fathoms of the sea-level; in other places it sinks for considerably more than 1,000 fathoms below that level. Thus, while a very moderate elevation would in certain latitudes cause the land to extend to the edge of the plateau, an elevation of at least 10,000 feet would be required in some other places to bring about a similar result.

Although it is true that the land surface is nowhere co-extensive with the great plateau, yet the existing coast-lines may be said to trend in the same general direction as its margins. So abruptly does the continental plateau rise from the oceanic trough, that a depression of the sea-level, or an elevation of the plateau, for 10,000 feet, would add only a narrow belt to the Pacific Coast between Alaska and Cape Horn, while the gain of land on the Atlantic slope of America between 30° N.L. and 40° S.L. would not be much greater. In the higher latitudes of the Northern Hemisphere, however, very considerable geographical changes would be accomplished by a much less amount of elevation of the plateau. Were the continental plateau to be upheaved for 3,000 feet, the major portion of the Arctic Sea would become land. Thus, in general terms, we may say that the coast-lines

of Arctic and temperate North America and Eurasia are further withdrawn from the edge of the continental plateau than those of lower latitudes.

In regions where existing coast-lines approach the margin of the plateau, they are apt to run for long distances in one determinate direction, and whether the coastal area be high or not, to show a gentle sinuosity. Their course is seldom interrupted by bold projecting headlands or peninsulas, or by intruding inlets, while fringing or marginal islands rarely occur. To these appearances the northern regions, as everyone knows, offer the strongest contrast. Not only do they trend irregularly, but their continuity is constantly interrupted by promontories and peninsulas, by inlets and fiords, while fringing islands abound. But an elevation of some 400 or 500 fathoms only would revolutionise the geography of those regions, and confer upon the northern coast-lines of the world the regularity which at present characterises those of Western Africa.

It is obvious, therefore, that the coast-lines of such lands as Africa owe their regularity primarily to their approximate coincidence with the steep boundary slopes of the continental plateau, while the irregularities characteristic of the coast-line of North-western Europe and the corresponding latitudes of North America are determined by the superficial configuration of the same plateau, which in those regions is relatively more depressed. I have spoken of the general contrast between high and low northern latitudes, but it is needless to say that in southern regions the coast-lines exhibit similar contrasts. The regular coast-lines of Africa and South America have already been referred to, but we cannot fail to recognise in the much indented sea-board and the numerous coastal islands of Southern Chili a complete analogy to the fiord regions of high northern latitudes. Both are areas of comparatively recent depression. Again, the manifold irregularities of the coasts of South-eastern Asia, and the multitudes of islands that serve to link that continent to Australia and New Zealand, are all evidence that the surface of the continental plateau in those regions is extensively invaded by the sea.

A word or two now as to the configuration of the oceanic trough. There can be no doubt that this differs very considerably from that of the land surface. It is, upon the whole, flat or gently undulating. Here and there it swells gently upwards into broad elevated banks, some of which have been traced for great distances. In other places narrower ridges and abrupt mountain-like elevations diversify its surface, and project again and again above the level of the sea, to form the numerous islets of Oceania. Once more, the sounding-line has made us acquainted with the notable fact that numerous deep depressions—some long and narrow, others relatively short and broad—stud the floor of the great trough. I shall have occasion to refer again to these remarkable depressions, and need at present only call attention to the fact that they are especially well-developed in the region of the Western Pacific, where the floor of the sea, at the base of the bounding slopes of the continental plateau, sinks in places to depths of three and even of five miles below the existing coast-lines. One may further note the fact that the deepest areas of the Atlantic are met with in like manner close to the walls of the plateau—a long ridge, which rises midway between the continents and runs in the same general direction as their coast-lines, serving to divide the trough of the Atlantic into two parallel hollows.

But, to return to our coast-lines and the question of their development, it is obvious that their general trends have been determined by crustal movements. Their regularity is in direct proportion to the closeness of their approach to the margin of the continental plateau. The more nearly they coincide with the edge of that plateau, the fewer irregularities do they present; the further they recede from it, the more highly are they indented. Various other factors, it is true, have played a more or less important part in their development, but their dominant trends were undoubtedly determined at a very early period in the world's history—their determination necessarily dates back, in short, to the time when the great world-ridges and oceanic troughs came into existence. So far as we can read the story told by the rocks, however, it would seem that in the earliest ages of which geology can speak with any confidence, the coast-lines of the world must have been infinitely more irregular than now. In Palæozoic times, relatively small areas of

the continental plateau appeared above the level of the sea. Insular conditions everywhere prevailed. But as ages rolled on, wider and wider tracts of the plateau were exposed, and this notwithstanding many oscillations of level. So that one may say there has been upon the whole a general advance from insular to continental conditions. In other words, the sea has continued to retreat from the surface of the continental plateau. To account for this change, we must suppose that depression of the crust has been in excess within the oceanic area, and that now and again positive elevation of the continental plateau has taken place, more especially along its margins. That movements of elevation, positive or negative, have again and again affected our land areas can be demonstrated, and it seems highly probable, therefore, that similar movements may have been experienced within the oceanic trough.

Two kinds of crustal movement, as we have seen, are recognised by geologists. Sometimes the crust appears to rise, or, as the case may be, to sink over wide regions, without much disturbance or tilting of strata, although these are now and again more or less extensively fractured and displaced. It may conduce to clearness if we speak of these movements as regional. The other kind of crustal disturbance takes place more markedly in linear directions, and is always accompanied by abrupt folding and mashing together of strata, along with more or less fracturing and displacement. The plateau of the Colorado has often been cited as a good example of regional elevation, where we have a wide area of approximately horizontal strata apparently uplifted without much rock-disturbance, while the Alps or any other chain of highly flexed and convoluted strata will serve as an example of what we may term axial or linear uplifts It must be understood that both regional and axial movements result from the same cause—the adjustment of the solid crust to the contracting nucleus—and that the term *elevation*, therefore, is only relative. Sometimes the sinking crust gets relief from the enormous lateral pressure to which it is subjected by ridging up along lines of weakness, and then mountains of elevation are formed , at other times, the pressure is relieved by the formation of broader swellings, when wide areas become uplifted relatively to surrounding regions. Geologists, however, are beginning to doubt whether upheaval of the latter kind can affect a broad continental area. Probably, in most cases, the apparent elevation of continental regions is only negative. The land appears to have risen because the floor of the oceanic basin has become depressed. Even the smaller plateau-like elevations which occur within some continental regions may in a similar way owe their dominance to the sinking of contiguous regions.

In the geographical development of our land, movements of elevation and depression have played an important part. But we cannot ignore the work done by other agents of change. If the orographical features of the land everywhere attest the potency of plutonic agents, they no less forcibly assure us that the inequalities of surface resulting from such movement are universally modified by denudation and sedimentation. Elevated plains and mountains are gradually demolished, and the hollows and depressions of the great continental plateau become slowly filled with their detritus. Thus inland seas tend to vanish, inlets and estuaries are silted up, and the land in places advances seaward. The energies of the sea, again, come in to aid those of rain and rivers, so that under the combined action of all the superficial agents of change, the irregularities of coastlines become reduced, and, were no crustal movement to intervene, would eventually disappear. The work accomplished by those agents upon a coast-line is most conspicuous in regions where the surface of the continental plateau is occupied by comparatively shallow seas. Here full play is given to sedimentation and marine erosion, while the latter alone comes into prominence upon shores that are washed by deeper waters When the coast-lines advance to the edge of the continental plateau, they naturally trend, as we have seen, for great distances in some particular direction Should they preserve that position, undisturbed by crustal oscillation, for a prolonged period of time, they will eventually be cut back by the sea. In this way a shelf or terrace will be formed, narrow in some places, broader in others, according to the resistance offered by the varying character of

the rocks. But no long inlets or fiords can result from such action. At most the harder and less readily demolished rocks will form headlands, while shallow bays will be scooped out of the more yielding masses. In short, between the narrower and broader parts of the eroded shelf or terrace a certain proportion will tend to be preserved. As the shelf is widened, sedimentation will become more and more effective, and in places may come to protect the land from further marine erosion. This action is especially conspicuous in tropical and subtropical regions, which are characterised by well-marked rainy seasons. In such regions immense quantities of sediment are washed down from the land to the sea, and tend to accumulate along shore, forming low alluvial flats. All long-established coast-lines thus acquire a characteristically sinuous form, and perhaps no better examples could be cited than those of Western Africa.

To sum up, then, we may say that the chief agents concerned in the development of coast-lines are crustal movements, sedimentation, and marine erosion. All the main trends are the result of elevation and depression. Considerable geographical changes, however, have been brought about by the silting up of those shallow and sheltered seas which, in certain regions, overflow wide areas of the continental plateau. Throughout all the ages, indeed, epigene agents have striven to reduce the superficial inequalities of that plateau, by levelling heights and filling up depressions, and thus, as it were, flattening out the land surface and causing it to extend. The erosive action of the sea, from our present point of view, is of comparatively little importance. It merely adds a few finishing touches to the work performed by the other agents of change.

A glance at the geographical evolution of our own continent will render this sufficiently evident. Viewed in detail, the structure of Europe is exceedingly complicated, but there are certain leading features in its architecture which no profound analysis is required to detect. We note, in the first place, that highly disturbed rocks of Archæan and Palæozoic age reach their greatest development along the north-western and western borders of our continent, as in Scandinavia, the British Islands, North-west France, and the Iberian peninsula. Another belt of similarly disturbed strata of like age traverses Central Europe from west to east, and is seen in the South of Ireland, Cornwall, North-west France, the Ardennes, the Thuringerwald, the Erzgebirge, the Riesengebirge, the Bohmerwald, and other heights of Middle and Southern Germany. Strata of Mesozoic and Cainozoic age rest upon the older systems in such a way as to show that the latter had been much folded, fractured, and denuded before they came to be covered with younger formations. North and north-east of the central belt of ancient rocks just referred to, the sedimentary strata that extend to the shores of the Baltic and over a vast region in Russia, range in age from Palæozoic down to Cainozoic times, and are disposed for the most part in gentle undulations—they are either approximately horizontal or slightly inclined. Unlike the disturbed rocks of the maritime regions and of Central Europe, they have obviously been subjected to comparatively little folding since the time of their deposition. To the south of the primitive backbone of Central Europe succeeds a region composed superficially of Mesozoic and Cainozoic strata for the most part, which, along with underlying Palæozoic and Archæan rocks, are often highly flexed and ridged up, as in the chains of the Jura, the Alps, the Carpathians, &c. One may say, in general terms, that throughout the whole Mediterranean area Archæan and Palæozoic rocks appear at the surface only when they form the nuclei of mountains of elevation into the composition of which rocks of younger age largely enter.

From this bald and meagre outline of the general geological structure of Europe, we may gather that the leading orographical features of our continent began to be developed at a very early period. Unquestionably the oldest land areas are represented by the disturbed Archæan and Palæozoic rocks of the Atlantic sea-board and Central Europe. Examination of those tracts shows that they have experienced excessive denudation. The Archæan and Palæozoic masses, distributed along the margin of the Atlantic, are the mere wrecks of what, in earlier ages, must have been lofty regions, the mountain chains of which may well have rivalled or even exceeded in height the Alps of to-day. They, together with

the old disturbed rocks of Central Europe, formed for a long time the only land in our area. Between the ancient Scandinavian tract in the North and a narrow interrupted belt in Central Europe, stretched a shallow sea, which covered all the regions that now form our Great Plain; while immediately south of the central belt lay the wide depression of the Mediterranean—for as yet the Pyrenees, the Alps, and the Carpathians were not. Both the Mediterranean and the Russo-Germanic sea communicated with the Atlantic. As time went on land continued to be developed along the same lines, a result due partly to crustal movements, partly to sedimentation. Thus by-and-by the relatively shallow Russo-Germanic sea became silted up, while the Mediterranean shore-line advanced southwards. It is interesting to note that the latter sea, down to the close of Tertiary times, seems always to have communicated freely with the Atlantic, and to have been relatively deep. The Russo-Germanic sea, on the contrary, while now and again opening widely into the Atlantic, and attaining considerable depths in its western reaches, remained on the whole shallow, and ever and anon vanished from wide areas to contract into a series of inland seas and large salt lakes.

Reduced to its simplest elements, therefore, the structure of Europe shows two primitive ridges—one extending with some interruptions along the Atlantic sea-board, the other traversing Central Europe from west to east, and separating the area of the Great Plain from the Mediterranean basin. The excessive denudation which the more ancient lands have undergone, and the great uplifts of Mesozoic and of Cainozoic times, together with the comparatively recent submergence of broad tracts in the north and north-west, have not succeeded in obscuring the dominant features in the architecture of our continent.

I now proceed to trace, as rapidly as I can, the geographical development of the coast-lines of the Atlantic as a whole, and to point out the chief contrasts between them and those of the Pacific. The extreme irregularity of the Arctic and Atlantic shores of Europe at once suggests to a geologist a partially drowned land, the superficial inequalities of which are accountable for the vagaries of the coast-lines. The fiords of Norway and Scotland occupy what were at no distant date land valleys, and the numerous marginal islands of those regions are merely the projecting portions of a recently sunken area. The continental plateau extends up to and a little beyond the one hundred fathoms line, and there are many indications that the land formerly reached as far. Thus the sunken area is traversed by valley-like depressions, which widen as they pass outwards to the edge of the plateau, and have all the appearance of being hollows of subaerial erosion. I have already mentioned the fact that the Scandinavian uplands and the Scottish Highlands are the relics of what were at one time true mountains of elevation, corresponding in the mode of their formation to those of Switzerland, and, like these, attaining a great elevation. During subsequent stages of Palæozoic time, that highly elevated region was subjected to long-continued and profound erosion —the mountain country was planed down over wide regions to sea-level, and broad stretches of the reduced land surface became submerged. Younger Palæozoic formations now accumulated upon the drowned land, until eventually renewed crustal disturbance supervened, and the marginal areas of the continental plateau again appeared as dry land, but not, as before, in the form of mountains of elevation. Lofty table-lands now took the place of abrupt and serrated ranges and chains—table-lands which, in their turn, were destined in the course of long ages to be deeply sculptured and furrowed by subaerial agents. During this process the European coast-line would seem to have coincided more or less closely with the edge of the continental plateau. Finally, after many subsequent movements of the crust in these latitudes, the land became partially submerged—a condition from which North-western and Northern Europe would appear in recent times to be slowly recovering. Thus the highly indented coast-line of those regions does not coincide with the edge of the plateau, but with those irregularities of its upper surface which are the result of antecedent subaerial erosion.

Mention has been made of the Russo-Germanic plain and the Mediterranean as representing original depressions in the continental plateau, and of the high grounds that extend between them as regions of dominant elevation, which,

throughout all the manifold revolutions of the past, would appear to have persisted as a more or less well-marked boundary, separating the northern from the southern basin.   During certain periods it was no doubt in some degree submerged, but never apparently to the same extent as the depressed areas it served to separate.   From time to time uplifts continued to take place along this central belt, which thus increased in breadth, the younger formations, which were accumulated along the margins of the two basins, being successively ridged up against nuclei of older rocks    The latest great crustal movements in our continent, resulting in the uplift of the Alps and other east and west ranges of similar age, have still further widened that ancient belt of dominant elevation which in our day forms the most marked orographical feature of Europe.

The Russo-Germanic basin is now for the most part land, the Baltic and the North Sea representing its still submerged portions.   This basin, as already remarked, was probably never so deep as that of the Mediterranean.   We gather as much from the fact, that while mechanical sediments of comparatively shallow-water origin predominate in the former area, limestones are the characteristic features of the southern region.   Its relative shallowness helps us to understand why the northern depression should have been silted up more completely than the Mediterranean.   We must remember also that for long ages it received the drainage of a much more extensive land surface than the latter—the land that sloped towards the Mediterranean in Palæozoic and Mesozoic times being of relatively little importance.   Thus the crustal movements which ever and anon depressed the Russo-Germanic area were, in the long run, counterbalanced by sedimentation. The uplift of the Alps, the Atlas, and other east and west ranges, has greatly contracted the area of the Mediterranean, and sedimentation has also acted in the same direction, but it is highly probable that that sea is now as deep as, or even deeper than, it has ever been.   It occupies a primitive depression in which the rate of subsidence has exceeded that of sedimentation.   In many respects, indeed, this remarkable transmeridional hollow—continued eastward in the Red Sea, the Black Sea, and the Aralo-Caspian depression—is analogous, as we shall see, to the great oceanic trough itself.

In the earlier geological periods linear or axial uplifts and volcanic action again and again marked the growth of the land on the Atlantic sea-board.   But after Palæozoic times, no great mountains of elevation came into existence in that region, while volcanic action almost ceased.   In Tertiary times, it is true, there was a remarkable recrudescence of volcanic activity, but the massive eruptions of Antrim and Western Scotland, of the Færoe Islands and Iceland, must be considered apart from the general geology of our continent    From Mesozoic times onwards it was along the borders of the Mediterranean depression that great mountain uplifts and volcanoes chiefly presented themselves.   And as the land surface extended southwards from Central Europe, and the area of the Mediterranean was contracted, volcanic action followed the advancing shore-lines.   The occurrence of numerous extinct and of still existing volcanoes along the borders of this inland sea, the evidence of recent crustal movements so commonly met with upon its margins, the great irregularities of its depths, the proximity of vast axial uplifts of late geological age, and the frequency of earthquake phenomena, all indicate instability, and remind us strongly of similarly constructed and disturbed regions within the area of the vast Pacific.

Let us now look at the Arctic and Antarctic coast-lines of North America. From the extreme north down to the latitude of New York the shores are obviously those of a partially submerged region.   They are of the same type as the coasts of North-western Europe.   We have every reason to believe also that the depression of Greenland and North-east America, from which these lands have only partially recovered, dates back to a comparatively recent period.   The fiords and inlets, like those of Europe, are merely half-drowned land valleys, and the continental shelf is crossed by deep hollows which are evidently only the seaward continuations of well-marked terrestrial features.   Such, for example, is the case with the valleys of the Hudson and the St. Lawrence, the submerged portions of which can be followed out to the edge of the continental plateau, which is notched

by them at depths of 474 and 622 fathoms respectively. There is, in short, a broad resemblance between the coasts of the entire Arctic and North Atlantic regions down to the latitudes already mentioned. Everywhere they are irregular and fringed with islands in less or greater abundance—highly denuded and deeply incised plateaus being penetrated by fiords, while low-lying and undulating lands that shelve gently seaward are invaded by shallow bays and inlets. Comparing the American with the opposite European coasts one cannot help being struck with certain other resemblances. Thus Hudson Bay at once suggests the Baltic, and the Gulf of Mexico, with the Caribbean Sea, recall the Mediterranean. But the geological structure of the coast-lands of Greenland and North America betrays a much closer resemblance between these and the opposite shores of Europe than appears on a glance at the map. There is something more than a mere superficial similarity. In eastern North America and Greenland, just as in Western Europe, no grand mountain uplifts have taken place for a prodigious time. The latest great upheavals, which were accompanied by much folding and flexing of strata, are those of the Apallachian chain and of the coastal ranges extending through New England, Nova Scotia, and Newfoundland, all of which are of Palæozoic age. Considerable crustal movements affected the American coast-lands in Mesozoic times, and during these uplifts the strata suffered fracture and displacement, but were subjected to comparatively little folding. Again, along the maritime borders of North-east America, as in the corresponding coast-lands of Europe, igneous action, more or less abundant in Palæozoic and early Mesozoic times, has since been quiescent From the mouth of the Hudson to the Straits of Florida the coast-lands are composed of Tertiary and Quaternary deposits. This shows that the land has continued down to recent times to gain upon the sea—a result brought about partly by quiet crustal movements, but to a large extent by sedimentation, aided, on the coasts of Florida, by the action of reef-building corals

Although volcanic action has long ceased on the American sea-board, we note that in Greenland, as in the West of Scotland and North of Ireland, there is abundant evidence of volcanic activity at so late a period as the Tertiary. It would appear that the great plateau-basalts of those regions, and of Iceland and the Færoe Islands, were contemporaneous, and possibly connected with an important crustal movement. It has long been suggested that at a very early geological period Europe and North America may have been united. The great thickness attained by the Palæozoic rocks in the eastern areas of the latter implies the existence of a wide land surface from which ancient sediments were derived. That old land must have extended beyond the existing coast-line, but how far we cannot tell. Similarly in North-west Europe, during early Palæozoic times, the land probably stretched further into the Atlantic than at present. But whether, as some think, an actual land connection subsisted between the two continents it is impossible to say. Some such connection was formerly supposed necessary to account for the emigration and immigration of certain marine forms of life which are common to the Palæozoic strata of both continents, and which, as they were probably denizens of comparatively shallow water, could only have crossed from one area to another along a shore-line. It is obvious, indeed, that if the oceanic troughs in those early days were of an abysmal character, a land bridge would be required to explain the geographical distribution of cosmopolitan life-forms. But if it be true that subsidence of the crust has been going on through all geological time, and that the land areas have notwithstanding continued to extend over the continental plateau, then it follows that the oceanic trough must be deeper now than it was in Palæozoic times. There are, moreover, certain geological facts which seem hardly explicable on the assumption that the seas of past ages attained abysmal depths over any extensive areas. The Palæozoic strata which enter so largely into the framework of our lands have much the same appearance all the world over, and were accumulated for the most part in comparatively shallow water. A petrographical description of the Palæozoic mechanical sediments of Europe would serve almost equally well for those of America, of Asia, or of Australia. Take in connection with this the fact that Palæozoic faunas had a

E 3

very much wider range than those of Mesozoic and later ages, and were characterised above all by the presence of many cosmopolitan species, and we can hardly resist the conclusion that it was the comparative shallowness of the ancient seas that favoured that wide dispersal of species, and enabled currents to distribute sediments the same in kind over such vast regions. As the oceanic area deepened and contracted, and the land surface increased, marine faunas were gradually restricted in their range, and cosmopolitan marine forms diminished in numbers, while sediments, gathering in separate regions, became more and more differentiated. For these and other reasons, which need not be entered upon here. I see no necessity for supposing that a Palæozoic Atlantis connected Europe with North America. The broad ridge upon which the Fœroe Islands and Iceland are founded seems to pertain as truly to the oceanic depression as the long Dolphin Ridge of the South Atlantic. The trend of the continental plateau in high latitudes is shown, as I think, by the general direction of the coast-lines of North-western Europe and East Greenland, the continental shelf being submerged in those regions for a few hundred fathoms only. How the Icelandic ridge came into existence, and what its age may be, we can only conjecture. It may be a wrinkle as old as the oceanic trough which it traverses, or its origin may date back to a much more recent period. We may conceive it to be an area which has subsided more slowly than the floor of the ocean to the north and south , or, on the other hand, it may be a belt of positive elevation. Perhaps the latter is the more probable supposition, for it seems very unlikely that crustal disturbances, resulting in axial and regional uplifts, should have been confined to the continental plateau only. Be that as it may, there seems little doubt that land connection did obtain between Greenland and Europe in Cainozoic times, along this Icelandic ridge, for relics of the same Tertiary flora are found in Scotland, the Fœroe Islands, Iceland, and Greenland. The deposits in which these plant-remains occur are associated with great sheets of volcanic rocks, which in the Fœroe Islands and Iceland reach a thickness of many thousand feet. Of the same age are the massive basalts of Jan Mayen, Spitzbergen, Franz Joseph Land, and Greenland  These lavas seem seldom to have issued from isolated foci in the manner of modern eruptions, but rather to have welled up along the lines of rectilineal fissures. From the analogy of similar phenomena in other parts of the world it might be inferred that the volcanic action of these northern regions may have been connected with a movement of elevation, and that the Icelandic ridge, if it did not come into existence during the Tertiary period, was at all events greatly upheaved at that time  It would seem most likely, in short, that the volcanic action in question was connected mainly with crustal movements in the oceanic trough  Similar phenomena, as is well known, are met with further south in the trough of the Atlantic. Thus the volcanic Azores rise like Iceland from the surface of a broad ridge which is separated from the continental plateau by wide and deep depressions.  And so again, from the back of the great Dolphin Ridge, spring the volcanic islets of St. Paul's, Ascension, and Tristan d'Acunha.

I have treated of the Icelandic bank at some length for the purpose of showing that its volcanic phenomena do not really form an exception to the rule that such eruptions ceased after Palæozoic or early Mesozoic times to disturb the Atlantic coast-lines of Europe and North America. As the bank in question extends between Greenland and the British Islands, it was only natural that both those regions should be affected by its movements.  But its history pertains essentially to that of the Atlantic trough; and it seems to show us how transmeridional movements of the crust, accompanied by vast discharges of igneous rock, may come in time to form land connections between what are now widely separated areas.

Let us next turn our attention to the coast-lines of the Gulf of Mexico and the Caribbean Sea. These enclosed seas have frequently been compared to the Mediterranean, and the resemblance is self-evident. Indeed, it is so close that one may say the Mexican-Caribbean Sea and the Mediterranean are rather homologous than simply analogous. The latter, as we have seen, occupies a primitive depression, and formerly covered a much wider area. It extended at one time over much of Southern Europe and Northern Africa, and appears to have had full communica-

tion across Asia Minor with the Indian Ocean, and with the Arctic Ocean athwart the low-lying tracts of North-western Asia. Similarly, it would seem, the Mexican-Caribbean Sea is the remaining portion of an ancient inland sea which formerly stretched north through the heart of North America to the Arctic Ocean. Like its European parallel, it has been diminished by sedimentation and crustal movements. It resembles the latter also in the greatness and irregularity of its depths, and in the evidence which its islands supply of volcanic action as well as of very considerable crustal movements within recent geological times. Along the whole northern borders of the Gulf of Mexico the coast-lands, like those on the Atlantic sea-board of the Southern States, are composed of Tertiary and recent accumulations, and the same is the case with Yucatan; while similar young formations are met with on the borders of the Carribbean Sea and in the Antilles. The Bahamas and the Windward Islands mark out for us the margin of the continental plateau, which here falls away abruptly to profound depths. One feels assured that this portion of the plateau has been ridged up to its present level at no distant geological date But notwithstanding all the evidence of recent extensive crustal movements in this region, it is obvious that the Mexican-Caribbean depression, however much it may have been subsequently modified, is of primitive origin.[1]

Before we leave the coast-lands of North America, I would again point out their leading geological features. In a word, then, they are composed for the most part of Archæan and Palæozoic rocks; no great linear or axial uplifts marked by much flexure of strata have taken place in those regions since Palæozoic times; while igneous action virtually ceased about the close of the Palæozoic or the commencement of the Mesozoic period. It is not before we reach the shores of the Southern States and the coast-lands of the Mexican-Caribbean Sea that we encounter notable accumulations of Mesozoic, Tertiary, and younger age These occur in approximately horizontal positions round the Gulf of Mexico, but in the Sierra Nevada of Northern Colombia and the Cordilleras of Venezuela Tertiary strata are ridged up into true mountains of elevation. Thus the Mexican-Caribbean depression, like that of the Mediterranean, is characterised not only by its irregular depths and its volcanic phenomena, but by the propinquity of recent mountains of upheaval, which bear the same relation to the Caribbean Sea that the mountains of North Africa do to the Mediterranean.

We may now compare the Atlantic coasts of South America with those of Africa. The former coincide in general direction with the edge of the continental plateau, to which they closely approach between Cape St. Roque and Cape Frio. In the north-east, between Cape Paria, opposite Trinidad, and Cape St. Roque, the continental shelf attains a considerably greater breadth, while south of Cape Frio it gradually widens until, in the extreme south, it runs out towards the east in the form of a narrow ridge, upon the top of which rise the Falkland Islands and South Georgia. Excluding from consideration for the present all recent alluvial and Tertiary deposits, we may say that the coast-lands from Venezuela down to the South of Brazil are composed principally of Archæan rocks; the eastern borders of the continent further south being formed of Quaternary and Tertiary accumulations. So far as we know, igneous rocks are of rare occurrence on the Atlantic sea-board. Palæozoic strata approach the coast-lands at various points between the mouths of the Amazons and La Plata, and these, with the underlying and surrounding Archæan rocks, are more or less folded and disturbed, while the younger strata of Mesozoic and Cainozoic age (occupying wide regions in the basin of the Amazons, and here and there fringing the sea-coast), occur in approximately horizontal positions It would appear, therefore, that no great axial uplifts have taken place in those regions since Palæozoic times. The crustal movements of later ages were regional rather than axial; the younger rocks are not flexed and mashed together, and their elevation (negative or positive) does not seem to have been accompanied by conspicuous volcanic action.

[1] Professor Suess thinks it is probable that the Caribbean Sea and the Mediterránean are portions of one and the same primitive depression which traversed the Atlantic area in early Cretaceous times He further suggests that it may have been through the gradual widening of this central Mediterranean that the Atlantic in later times came into existence.

The varying width of the continental shelf is due to several causes. The Orinoco, the Amazons, and other rivers descending to the north-west coast, carry enormous quantities of sediment, much of which comes to rest on the submerged slopes of the continental plateau, so that the continental shelf tends to extend seawards  The same process takes place on the south-east coast, where the River Plate discharges its muddy waters.  South of latitude 40° S , however, another cause has come into play.  From the mouth of the Rio Negro to the terminal point of the continent the whole character of the coast betokens a geologically recent emergence, accompanied and followed by considerable marine erosion.  So that in this region the continental shelf increases in width by the retreat of the coast-line, while in the north-east it gains by advancing seawards  It is to be noted, however, that even there, in places where the shores are formed of alluvia, the sea tends to encroach upon the land.

The Atlantic coast of Africa resembles that of South America in certain respects, but it also offers some important contrasts.  As the northern coasts of Venezuela and Colombia must be considered in relation rather to the Caribbean depression than to the Atlantic, so the African sea-board between Cape Spartel and Cape Nun pertains structurally to the Mediterranean region.  From the southern limits of Morocco to Cape Colony the coastal heights are composed chiefly of Archæan and Palæozoic rocks, the low shore-lands showing here and there strata of Mesozoic and Tertiary age together with still more recent deposits.  The existing coast-lines everywhere advance close to the edge of the continental plateau, so that the submarine shelf is relatively narrower than that of Eastern South America. The African coast is still further distinguished from that of South America by the presence of several groups of volcanic islands—Fernando Po and others in the Gulf of Guinea, and Cape Verde and Canary Islands.  The last-named group, however, notwithstanding its geographical position, is probably related rather to the Mediterranean depression than to the Atlantic trough.

The geological structure of the African coast-lands shows that the earliest to come into existence were those that extend between Cape Nun and the Cape of Good Hope.  The coastal ranges of that section are much denuded, for they are of very great antiquity, having been ridged up in Palæozoic times  The later uplifts (negative or positive) of the same region were not attended by tilting and folding of strata, for the Mesozoic and Tertiary deposits, like those of South America, lie in comparatively horizontal positions.  Between Cape Nun and Cape Spartel the rocks of the maritime tracts range in age from Palæozoic to Cainozoic, and have been traced across Morocco into Algeria and Tunis.  They all belong to the Mediterranean region, and were deposited at a time when the southern shores of that inland sea extended from a point opposite the Canary Islands along the southern margin of Morocco, Algeria, and Tunis.  Towards the close of the Tertiary period the final upheaval of the Atlas took place, and the Mediterranean, retreating northwards, became an almost land-locked sea.

I need hardly stop to point out how the African coast-lines have been modified by marine erosion and the accumulation of sediment upon the continental shelf. The extreme regularity of the coasts is due partly to the fact that the land is nearly co-extensive with the continental plateau, but it also results in large measure from the extreme antiquity of the land itself  This has allowed of the cutting-back of headlands and the filling up of bays and inlets, a process which has been going on between Morocco and Cape Colony with probably little interruption for a very prolonged period of time.  We may note also the effect of the heavy rains of the equatorial region in washing down detritus to the shores, and in this way protecting the land to some extent from the erosive action of the sea.

What now, let us ask, are the outstanding features of the coast-lines of the Atlantic Ocean ?  We have seen that along the margins of each of the bordering continents the last series of great mountain-uplifts took place in Palæozoic times. This is true alike for North and South America, for Europe and Africa.  Later movements which have added to the extent of land were not marked by the extreme folding of strata which attended the early upheavals.  The Mesozoic and Cainozoic rocks, which now and again form the shore-lands, occur in more or less undisturbed

condition. The only great linear uplifts or true mountains of elevation which have come into existence in Western Europe and North Africa since the Palæozoic period trend approximately at right angles to the direction of the Atlantic trough, and are obviously related to the primitive depression of the Mediterranean. The Pyrenees and the Atlas, therefore, although their latest elevation took place in Tertiary times, form no exceptions to the rule that the extreme flexing and folding of strata which is so conspicuous a feature in the geological structure of the Atlantic sea-board dates back to the Palæozoic era. And the same holds true of North and South America. There all the coastal ranges of highly flexed and folded strata are of Palæozoic age. The Cordilleras of Venezuela are no doubt a Tertiary uplift, but they are as obviously related to the Caribbean depression as the Atlas ranges are to that of the Mediterranean. Again, we note that volcanic activity along the borders of the Atlantic was much less pronounced during the Mesozoic period than it appears to have been in earlier ages. Indeed, if we except the great Tertiary basalt-flows of the Icelandic ridge and the Arctic regions, we may say that volcanic action almost ceased after the Palæozoic era to manifest itself upon the Atlantic coast-lands of North America and Europe. But while volcanic action has died out upon the Atlantic margins of both continents, it has continued during a prolonged geological period within the area of the Mediterranean depression And in like manner the corresponding depression between North and South America has been the scene of volcanic disturbances from Mesozoic down to recent times. Along the African coasts the only displays of recent volcanic action that appertain to the continental margin are those of the Gulf of Guinea and the Cape de Verde Islands. The Canary Islands and Madeira may come under the same category, but, as we have seen, they appear to stand in relationship to the Mediterranean depression and the Tertiary uplift of North Africa. Of Iceland and the Azores I have already spoken, and of Ascension and the other volcanic islets of the South Atlantic it is needless to say that they are related to wrinkles in the trough of the ocean, and therefore have no immediate connection with the continental plateau.

Thus in the geographical development of the Atlantic coast-lines we may note the following stages:—*First*, during Palæozoic times a series of great mountain-uplifts, which were frequently accompanied by volcanic action. *Second*, a prolonged stage of comparative coastal tranquillity, during which the maritime ranges referred to were subject to such excessive erosion that they were planed down to low levels, and in certain areas even submerged. *Third*, renewed elevation (negative or positive) whereby considerable portions of the much denuded Archæan and Palæozoic rocks, now largely covered by younger deposits, were converted into high lands. During this stage not much rock-folding took place, nor were any true mountains of elevation formed parallel to the Atlantic margins. It was otherwise, however, in the Mediterranean and Caribbean depressions, where coastal movements resulted in the formation of enormous linear uplifts. Moreover, volcanic action is now and has for a long time been more characteristic of these depressions than of the Atlantic coast-lands.

I must now ask you to take a comprehensive glance at the coast-lines of the Pacific Ocean. In some important respects these offer a striking contrast to those we have been considering. Time will not allow me to enter into detailed description, and I must therefore confine attention to certain salient features. Examining first the shores of the Americas, we find that there are two well-marked regions of fiords and fringing islands—namely, the coasts of Alaska and British Columbia, and of South America from 40° S.L. to Cape Horn. Although these regions may be now extending seawards in places, it is obvious that they have recently been subject to submergence. When the fiords of Alaska and British Columbia existed as land valleys it is probable that a broad land connection obtained between North America and Asia. The whole Pacific coast is margined by mountain ranges, which in elevation and boldness far exceed those of the Atlantic sea-board. The rocks entering into their formation range in age from Archæan and Palæozoic down to Cainozoic, and they are almost everywhere highly disturbed and flexed. It is not necessary, even if it were possible, to consider the geological history of all those uplifted masses. It is enough for my

purpose to note the fact that the coastal ranges of North America and the principal chain of the Andes were all elevated in Tertiary times. It may be remarked further, that from the Mesozoic period down to the present the Pacific borders of America have been the scene of volcanic activity far in excess of what has been experienced on the Atlantic sea-board

Geographically the Asiatic coasts of the Pacific offer a strong contrast to those of the American borders. The latter, as we have seen, are for the most part not far removed from the edge of the continental plateau. The coasts of the mainland of Asia, on the other hand, retire to a great distance, the true margin of the plateau being marked out by that great chain of islands which extends from Kamchatka south to the Philippines and New Guinea. The seas lying between those islands and the mainland occupy depressions in the continental plateau. Were that plateau to be lifted up for 6,000 or 7,000 feet the seas referred to would be enclosed by continuous land, and all the principal islands of the East Indian Archipelago—Sumatra, Java, Celebes, and New Guinea, would become united to themselves as well as to Australia and New Zealand. In short, it is the relatively depressed condition of the continental plateau along the western borders of the Pacific basin that causes the Asiatic coast-lines to differ so strikingly from those of America.

From a geological point of view the differences are less striking than the resemblances. It is true that we have as yet a very imperfect knowledge of the geological structure of Eastern Asia, but we know enough to justify the conclusion that in its main features that region does not differ essentially from Western North America. During Mesozoic and Cainozoic times the sea appears to have over-flowed vast tracts of Manchooria and China, and even to have penetrated into what is now the great Desert of Gobi. Subsequent crustal movements revolutionised the geography of all those regions. Great ranges of linear uplifts came into existence, and in these the younger formations, together with the foundations on which they rested, were squeezed into folds and ridged up against the nuclei of Palæozoic and Archæan rocks which had hitherto formed the only dry land. The latest of these grand upheavals are of Tertiary age, and, like those of the Pacific slope of America, they were accompanied by excessive volcanic action. The long chains of islands that flank the shores of Asia we must look upon as a series of partially submerged or partially emerged mountain-ranges, analogous geographically to the coast ranges of North and Central America, and to the youngest Cordilleras of South America. The presence of numerous active and recently extinct volcanoes, taken in connection with the occurrence of many great depressions which furrow the floor of the sea in the East Indian Archipelago, and the profound depths attained by the Pacific trough along the borders of Japan and the Kurile and Aleutian Islands—all indicate conditions of very considerable instability of the lithosphere. We are not surprised, therefore, to meet with much apparently conflicting evidence of elevation and depression in the coast-lands of Eastern Asia, where in some places the sea would seem to be encroaching, while in other regions it is retreating. In all earthquake-ridden and volcanic areas such irregular coastal changes may be looked for. So extreme are the irregularities of the sea-floor in the area lying between Australia, the Solomon Islands, the New Hebrides, and New Zealand, and so great are the depths attained by many of the depressions, that the margins of the continental plateau are harder to trace here than anywhere else in the world. The bottom of the oceanic trough throughout a large portion of the Southern and Western Pacific is, in fact, traversed by many great mountain ridges, the summits of which approach the surface again and again to form the numerous islets of Polynesia. But notwithstanding the considerable depths that separate Australia from New Zealand there is geological evidence to show that a land connection formerly linked both to Asia. The continental plateau, therefore, must be held to include New Caledonia and New Zealand. Hence the volcanic islets of the Solomon and New Hebrides groups are related to Australia in the same way as the Riu-kiu, Japanese, and Kurile Islands are to Asia.

Having rapidly sketched the more prominent features of the Pacific coast-lines, we are in a position to realise the remarkable contrast they present to the coast-

lines of the Atlantic. The highly folded strata of the Atlantic sea-board are the relics of great mountains of upheaval, the origin of which cannot be assigned to a more recent date than Palæozoic times. During subsequent crustal movements no mountains of corrugated strata were uplifted along the Atlantic margins, the Mesozoic and Cainozoic strata of the coastal regions showing little or no disturbance. It is quite in keeping with all this that volcanic action appears to have been most strongly manifested in Palæozoic times. So many long ages have passed since the upheaval of the Archæan and Palæzoic mountains of the Atlantic sea-board that these heights have everywhere lost the character of true mountains of elevation. Planed down to low levels, partially submerged and covered to some extent by newer formations, they have in many places been again converted into dry lands, forming plateaus—now sorely denuded and cut up into mountains and valleys of erosion. Why the later movements along the borders of the Atlantic basin should not have resulted in the wholesale plication of the younger sedimentary rocks is a question for geologists. It would seem as if the Atlantic margins had reached a stage of comparative stability long before the grand Tertiary uplifts of the Pacific borders had taken place; for, as we have seen, the Mesozoic and Cainozoic strata of the Atlantic coast-lands show little or no trace of having been subjected to tangential thrusting and crushing. Hence one cannot help suspecting that the retreat of the sea during Mesozoic and Cainozoic ages may have been due rather to subsidence of the oceanic trough and to sedimentation within the continental area than to positive elevation of the land.

Over the Pacific trough, likewise, depression has probably been in progress more or less continuously since Palæozoic times, and this movement alone must have tended to withdraw the sea from the surface of the continental plateau in Asia and America. But by far the most important coastal changes in those regions have been brought about by the crumpling up of the plateau, and the formation of gigantic mountains of upheaval along its margins From remotest geological periods down almost to the present, the land area has been increased from time to time by the doubling-up and consequent elevation of coastal accumulations and by the eruption of vast masses of volcanic materials. It is this long-continued activity of the plutonic forces within the Pacific area which has caused the coast-lands of that basin to contrast so strongly with those of the Atlantic. The latter are incomparably older than the former—the heights of the Atlantic borders being mountains of denudation of vast geological antiquity, while the coastal ranges of the Pacific slope are creations but of yesterday as it were. It may well be that those Cordilleras and mountain-chains reach a greater height than was ever attained by any Palæozoic uplifts of the Atlantic borders. But the marked disparity in elevation between the coast-lands of the Pacific and the Atlantic is due chiefly to a profound difference in age. Had the Pacific coast-lands existed for as long a period and suffered as much erosion as the ancient rocks of the Atlantic sea-board, they would now have little elevation to boast of.

The coast-lines of the Indian Ocean are not, upon the whole, far removed from the margin of the continental plateau. The elevation of East Africa for 6,000 feet would add only a very narrow belt to the land. This would still leave Madagascar an island, but there are geological reasons for concluding that this island was at a far distant period united to Africa, and it must therefore be considered as forming a portion of the continental plateau. The great depths which now separate it from the mainland are probably due to local subsidence, connected with volcanic action in Madagascar itself and in the Comoro Islands. The southern coasts of Asia, like those of East Africa, approach the edge of the continental plateau, so that an elevation of 6,000 feet would make little addition to the land area. With the same amount of upheaval, however, the Malay Peninsula, Sumatra, Java, and West Australia, would become united, but without extending much further seawards. Land connection, as we know, existed in Mesozoic times between Asia, Australia, and New Zealand, but the coast-lines of that distant period must have differed considerably from those that would appear were the regions in question to experience now a general elevation. The Archæan and Palæozoic rocks of the Malay Peninsula and Sumatra are flanked on the side of the Indian Ocean by great

volcanic ridges, and by uplifts of Tertiary strata, which continue along the line of the Nicobar and Andaman Islands into Burma. Thus the coast-lines of that section of the Indian Ocean exhibit a geographical development similar to that of the Pacific sea-board. Elsewhere, as in Hindustan, Arabia, and East Africa, the coast-lines appear to have been determined chiefly by regional elevations of the land or sub-sidence of the oceanic trough in Mesozoic and Cainozoic times, accompanied by the outwelling of enormous floods of lava. Seeing, then, that the Pacific and the Indian Oceans are pre-eminently regions which, down to a recent date, have been subject to great crustal movements and to excessive volcanic action, we may infer that in the development of their coast-lines the sea has played a very subordinate part. The shores, indeed, are largely protected from marine erosion by partially emerged volcanic ridges and by coral islands and reefs, and to a considerable extent also by the sediment which in tropical regions especially is swept down to the coast in great abundance by rains and rivers. Moreover, as the geological structure of these regions assures us, the land would appear seldom to have remained sufficiently long at one level to permit of much destruction by waves and tidal currents.

In fine, then, we arrive at the general conclusion that the coast-lines of the globe are of very unequal age. Those of the Atlantic were determined as far back as Palæozoic times by great mountain uplifts along the margin of the continental plateau. Since the close of that period many crustal oscillations have taken place, but no grand mountain ranges have again been ridged up on the Atlantic sea-board Meanwhile the Palæozoic mountain-chains, as we have seen, have suffered extensive denudation, have been planed down to the sea-level, and even submerged. Subsequently converted into land, wholly or partially as the case may have been, they now present the appearance of plains and plateaus of erosion, often deeply indented by the sea. No true mountains of elevation are met with any-where in the coast-lands of the Atlantic, while volcanic action has well-nigh ceased. In short, the Atlantic margins have reached a stage of comparative stability. The trough itself, however, is traversed by at least two well-marked banks of upheaval —the great meridional Dolphin Ridge, and the approximately transmeridional Færoe-Icelandic belt—both of them bearing volcanic islands.

But while the coast-lands of the Atlantic proper attained relative stability at an early period, those of the Mediterranean and Caribbean depressions have up to recent times been the scenes of great crustal disturbance. Gigantic mountain-chains were uplifted along their margins at so late a period as the Tertiary, and their shores still witness volcanic activity.

It is upon the margins and within the troughs of the Pacific Ocean, however, that subterranean action is now most remarkably developed. The coast-lines of that great basin are everywhere formed of grand uplifts and volcanic ranges, which, broadly speaking, are comparable in age to those of the Mediterranean and Carib-bean depressions. Along the north-east margin of the Indian Ocean the coast-lines resemble those of the Pacific, being of like recent age, and similarly marked by the presence of numerous volcanoes. The northern and western shores, however (as in Hindustan, Arabia, and East Africa), have been determined rather by regional elevation or by subsidence of the ocean-floor than by axial uplifts—the chief crustal disturbances dating back to an earlier period than those of the East Indian Archipelago. It is in keeping with this greater age of the western and northern coast-lands of the Indian Ocean that volcanic action is now less strongly manifested in their vicinity.

I have spoken of the comparative stability of the earth's crust within the Atlantic area as being evidenced by the greater age of its coastal ranges and the declining importance of its volcanic phenomena. This relative stability is further shown by the fact that the Atlantic sea-board is not much disturbed by earth-quakes. This, of course, is what might have been expected, for earthquakes are most characteristic of volcanic regions and of those areas in which mountain-uplifts of recent geological age occur. Hence the coast-lands of the Pacific and the East Indies, the borders of the Caribbean Sea, the volcanic ridges of the Atlantic basin, the lands of the Mediterranean, the Black Sea, and the Aralo-Caspian depressions,

the shores of the Red Sea, and vast tracts of Southern Asia, are the chief earth-quake regions of the globe. It may be noted, further, that shocks are not only most frequent but most intense in the neighbourhood of the sea. They appear to originate sometimes in the volcanic ridges and coastal ranges, sometimes under the floor of the sea itself. Now earthquakes, volcanoes, and uplifts are all expressions of the one great fundamental fact that the earth is a cooling and contracting body, and they indicate the lines of weakness along which the enormous pressures and strains induced by the subsidence of the crust upon its nucleus find relief. We cannot tell why the coast-lands of the Atlantic should have attained at so early a period a stage of relative stability—why no axial uplifts should have been developed along their margins since Palæozoic times. It may be that relief has been found in the wrinkling-up of the floor of the oceanic trough, and consequent formation of the Dolphin Ridge and other great submarine foldings of the crust. And it is possible that the growth of similar great ridges and wrinkles upon the bed of the Pacific may in like manner relieve the coast-lands of that vast ocean, and prevent the formation of younger uplifts along their borders

I have already remarked that two kinds of elevatory movements of the crust are recognised by geologists—namely, axial and regional uplifts. Some, however, are beginning to doubt, with Professor Suess, whether any vast regional uplifts are possible. Yet the view that would attribute all such apparent elevations of the land to subsidence of the crust under the great oceanic troughs is not without its difficulties. Former sea-margins of very recent geological age occur in all latitudes, and if we are to explain these by sub-oceanic depression, this will compel us to admit, as Suess has remarked, a general lowering of the sea-level of upwards of 1,000 feet. But it is difficult to believe that the sea-floor could have subsided to such an extent in recent times. Suess thinks it is much more probable that the high-level beaches of tropical regions are not contemporaneous with those of higher latitudes, and that the phenomena are best explained by his hypothesis of a secular movement of the ocean—the water being, as he contends, alternately heaped up at the equator and the poles The strand-lines in high latitudes, however, are certainly connected with glaciation in some way not yet understood. And if it cannot be confidently affirmed that they indicate regional movements of the land, the evidence, nevertheless, seems to point in that direction.

In concluding this imperfect outline-sketch of a large subject, I ought perhaps to apologise for having trespassed so much upon the domains of geology. But in doing so I have only followed the example of geologists themselves, whose divagations in territories adjoining their own are naturally not infrequent. From much that I have said, it will be gathered that with regard to the causes of many coastal changes we are still groping in the dark. It seems not unlikely, however, that as light increases we may be compelled to modify the view that all oscillations of the sea-level are due to movements of the lithosphere alone. That is a very heretical suggestion ; but that a great deal can be said for it anyone will admit after a candid perusal of Suess's monumental work, ' Das Antlitz der Erde.'

# ADDRESS

## TO THE

# ECONOMIC SCIENCE AND STATISTICS SECTION

### OF THE

# BRITISH ASSOCIATION,

### BY

## The Hon. Sir CHARLES W. FREMANTLE, K.C.B.

#### PRESIDENT OF THE SECTION.

I suppose that few Presidents of any Section of this Association begin the preparation of their Addresses without taking at least a mental retrospect of the work of their predecessors. I have turned with great interest to the Address delivered by the late Lord Neaves, who occupied this chair in 1871, when the Association last met in Edinburgh. Lord Neaves rightly held that the subject of statistics is ancillary to the main subject of the Section, Economic Science, and his immediate predecessor, the lamented Professor Stanley Jevons, pointed out at Liverpool in 1870 that even 'the name "statistics" in its true meaning denotes all knowledge relating to the condition of the State or people.' I propose to devote the main portion of my Address to a subject to which I have devoted much attention, and which is intimately connected with the welfare of an important section of our people, and I shall hope to point out the means which may be taken to promote their welfare without leading them, as Lord Neaves expressed it in his concluding words, 'to dispense with ordinary and necessary prudence.' It is impossible to exaggerate the change which has taken place since the date of Lord Neaves's Address in the ideas of the public as to its responsibilities in regard to what is called charity. While it recognises that much which was then held to be 'charity' is nothing more than justice to the poorer classes, its sense of the dangers of pauperisation has been greatly intensified, and it justly regards many of the charitable methods which would then have been unhesitatingly advocated as not conducive to their best interests. I venture to claim a considerable part of the change which has taken place as due to the efforts of the Charity Organisation Society, which had then been recently founded, and of which I have the honour this year to be Chairman. I claim that the Society has made men everywhere think, and think seriously, of the duty incumbent upon them not only of giving, but of giving with care and discrimination, and that it has enlisted in the service of their poorer brethren an army which, besides being always ready to be prudently generous, is in a thousand cases willing to ensure, by personal effort, that charitable help shall be wisely and kindly dispensed. Such personal effort realises what was well described centuries ago in the Talmud as 'the doing of kindness,' and is developing 'a system founded not on rights but on sympathy, dealing not in doles but in deeds of friendship and of fellowship, and demanding a giving of oneself rather than of one's stores.' It has naturally followed that collateral subjects, such as the promotion of thrift and the better regulation of benevolent and benefit societies, have during the last twenty years received a greatly increased amount of enlightened attention.

Before proceeding, however, to the main subject of my Address, let me briefly refer to two questions more directly connected with the special work to which the greater part of my official life has been devoted.

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    The first of these is the restoration of the gold coinage, a question which has
for many years past exercised the minds of successive Chancellors of the Exchequer
and has been a stumbling-block to bankers and the commercial world.  It had
long been felt that the machinery provided by the law, as laid down in the old
proclamations and embodied in the Coinage Act of 1870, was of necessity powerless
to maintain the gold currency in an efficient condition.  The law provided that
'where any gold coin of the realm is below the current weight . . . every person
shall, by himself or others, cut, break, or deface any such coin tendered to him in
payment, and the person tendering the same shall bear the loss'; but as there was
no penalty for the disregard of this obligation, it became practically inoperative.
Gold coins, however much below the least current weight, passed freely from hand
to hand, and bankers received them from their customers and paid them away again.
Only the Bank of England and a few other public departments obeyed the law,
with the result that the principal sufferers were the banking establishments, who
in the course of business pay large amounts of gold coin into the Bank of England
and were obliged to submit to the loss on all coins found to be light.  The banks,
in self-defence, naturally paid in as many full-weight coins as possible and put the
light again into circulation.  Not more than 1,500,000l. of light coin, therefore,
was annually withdrawn, and it was calculated that, at last, of the sovereigns in
circulation as many as 46 per cent., and of the half-sovereigns no fewer than 70
per cent., were below the least current weight.  A Bill was brought in in 1884
for the withdrawal of light coins by the State and for the substitution for the
half-sovereign of a ten-shilling piece of the intrinsic value of 9s., so that a fund
might be provided to cover the expense of the operation and of the future main-
tenance of the currency in a proper condition; but this Bill was not proceeded
with.  Of the subsequent Bills introduced none became law, until in 1889 an Act
was passed withdrawing light gold coins of former reigns, and these coins were
finally called in under a proclamation issued in November 1890.  The entire
operation was effected at a cost of about 50,000l.  It is curious to note that this
is the first instance in which gold coin has been decried in this country, for the
guinea and half-guinea had never been declared uncurrent, and doctors and others
might have contended that their fees were still represented by coins which were
legal tender.  The Act of 1889, with the subsequent proclamation, having served
its purpose by clearing the circulation of all the older gold coinages, there only
remained coins of the present reign to deal with.  The Coinage Act of 1891 pro-
vides for the withdrawal of light gold coin by the State at its full nominal value,
and will apply equally to coins which will hereafter become light as to those
which have already fallen below the legal weight.  No one can now or in the
future suffer for tendering a light sovereign or half-sovereign more than for making
a payment with a worn half-crown or shilling, and any Victorian gold coin
tendered at the Bank of England, provided that it has not been defaced and that
its weight has not been fraudulently reduced, is received and exchanged.  For the
present, coins must be sent in in parcels of 100l.  It is unnecessary to dwell upon
the advantage which these arrangements have conferred, and will confer, upon
the public.  In 1842–45, when the previous withdrawal of light gold took place,
the coin was only paid for by weight at the Mint price of 3l. 17s. 10½d. per ounce,
and many were the misunderstandings and bitter the complaints to which the
conditions of withdrawal gave rise.  No inconvenience or alarm, on the other
hand, is likely to attend the measures necessary under the Act of last year, which
make it possible to effect the gradual withdrawal of the light coin without friction.
To July 1 last the amounts withdrawn were · sovereigns 5,150,000l., and half-
sovereigns, 3,850,000l.  It had been estimated that the average deficiency of weight
in each sovereign would be 2·57d., and in each half-sovereign 2·65d., and the actual
deficiency found has been 2·65d. in the case of sovereigns and 2·93d. in the
case of half-sovereigns.  After the first withdrawals have been effected it is
probable that the deficiency will become less, as a certain amount of much worn
coin had no doubt been accumulated in banks in anticipation of the passing of the
Act.  As far as the work has as yet proceeded, however, the cost of withdrawing
1,000,000l. in sovereigns has been found to be 11,056l., and of withdrawing

1,000,000*l.* in half-sovereigns 24,418*l.* A sum of 400,000*l.* was set aside by the Act for the expenses of the withdrawal, which will be sufficient at this rate to meet the loss on 26,593,000*l.* Elaborate investigations were conducted by the late Professor Jevons in 1868, by Messrs. Inglis Palgrave and J. B. Martin in 1882, and by the Mint in 1888, with a view of ascertaining the total amount of light gold in circulation in the United Kingdom. Time does not admit of my analysing the results here, and indeed, so far as actual facts are concerned, the problem can never be solved, as a large number of coins become light each year, and the restoration of the currency, therefore, can never be complete. I might perhaps mention, as an interesting fact, that the Mint examination just referred to showed the gold coins circulating in Scotland to be less worn than those in circulation in England and Wales, owing no doubt to the general use in the north of 1*l.* notes.

The other question connected with the currency to which I wish to refer is one which since the last meeting of the Association has been much discussed, and which, though it has not as yet been the subject of legislation, is of primary importance.

In December last, the Chancellor of the Exchequer, in an address at the London Chamber of Commerce, described the changes which he thought it would be desirable to make in the currency system of this country for the purpose of increasing the central store of gold. The Baring crisis and difficulties which accompanied it, and in particular the necessity for obtaining 3,000,000*l* in gold from the Bank of France at very short notice, had drawn the attention of the business community to the fact that the existing metallic reserve was very small in relation to the enormous structure of credit founded upon it, and that it might be found to be wholly insufficient. Mr. Goschen's proposal was to allow the Bank to issue 1*l.* notes, requiring four-fifths of any additional amount of issue so created to be covered by gold, while only the remaining fifth would be allowed to be issued against securities. The effect of this scheme, if 1*l.* notes proved popular, would have been to increase the total amount of the central store of gold, and also to increase the proportion borne by the gold in the Issue Department of the Bank of England to the note-issue covered by it. At the same time the profits upon the fiduciary portion of the additional issue would have sufficed to defray the cost of that issue without additional charge to the public. The Chancellor of the Exchequer considered that if a substantial increase were by these means secured in the gold in the Issue Department, it would be safe to allow the Bank, in times of crisis, an elastic power of issuing further notes against securities, upon conditions stringent enough to secure this privilege from abuse. This elastic power of increased note-issue was intended to take the place of the illegal suspensions of the Bank Act which had on several occasions been found necessary in the past.

This scheme was the subject of much discussion both in the Press and in banking and business communities. There appeared to be a general consensus of opinion that an increase in the central store of gold was very desirable, but there was difference of opinion as to the manner in which that increase might best be brought about. Objection was also felt by many bankers, and by a large part of the general public in the South of England, to the issue of 1*l.* notes.

The conditions of the concluding session of the late Parliament were not favourable for dealing with a large scheme of currency reform, and, as it was evident that the scheme proposed would not receive such unanimous support as would make it possible to pass it without very full discussion and consideration, the Chancellor of the Exchequer did not bring his proposals before the House of Commons in the shape of a Bill.

I make no apology for devoting a large part of this Address to the subject of old-age pensions, although I am inclined to condole with my hearers and myself on the necessity of discussing a question which has now been for many months before the public, and which may by this time be considered to have been worn somewhat threadbare. But the question is surely a great and important one, on

the wise solution of which the welfare of a not inconsiderable part of our population may materially depend, and one, therefore, which should certainly find a place in the discussions of this section of the British Association.

All honour, let me say in the first place, to Canon Blackley, the pioneer of the movement so closely identified with his name and labours! Canon Blackley was, and is, in the opinion of many thoughtful people, only in advance of his age, and deserving of the credit of seeing that without compulsion no system of National Insurance worthy of the name can be carried into effect. His scheme, with others subsequently proposed, was considered by a committee of the House of Commons originally appointed in 1885, and reappointed in the Parliaments of 1885 and 1886, ' to inquire into the best system of national provident insurance against pauperism.' The report of the Committee, issued in August 1887, stated that their inquiry had ' practically narrowed itself into an examination of one particular scheme,' namely, Canon Blackley's, ' which had manifestly impressed itself, whether favourably or unfavourably, upon the minds of witnesses, to the exclusion of all other proposals.' It might ' be briefly described,' they reported, as a scheme ' for the compulsory insurance of all persons, of both sexes and of every class, by the prepayment between the ages of 18 and 21 years of the sum of 10*l.* or thereabouts into a National Friendly or Provident Society, thereby securing to the wage-earning classes 8*s.* per week sick-pay and 4*s.* per week superannuation pay after the age of 70 years.' In pronouncing their opinion on the scheme, the Committee first called attention to the evidence they had received from working-men and large employers of labour in favour of enforced contributions to a National Insurance Fund, the latter class of witnesses describing the benefits which had resulted from the establishment of such funds among persons in their own employment. They then proceeded to record the objections to the scheme laid before them from the administrative and actuarial points of view, to the difficulty of enforcing the payments to the fund, to the exclusion of all but wage-earners from benefit, to the discontent which would be felt by the upper and middle classes at being called upon to contribute, and, finally, to the proposal for compulsion, which they considered ' open to very strong objections.' It is clear, I think, that we are not prepared, at any rate at present, for the adoption of so sweeping a measure.

Canon Blackley has, indeed, since expressed his willingness to admit the idea of State aid towards pensions in accordance with the proposals of the National Provident League, with which he is connected; but he appears disposed to admit this and other deviations from his original plan only as stepping-stones towards a general system of compulsory contributions.

I next turn to Mr. Chamberlain's scheme. Mr. Chamberlain and the voluntary Committee of members of the House of Commons with whom he is associated propose to establish a State Pension Fund, to which Parliament should be asked to make an annual grant, to be supplemented by contributions from local rates  The scheme is applicable to both men and women, and contains provisions for the payment of certain sums into the Post Office Savings Bank before the age of twenty-five, and certain further sums during each of the succeeding forty years, which would entitle men to pensions of 13*l.*, and women to pensions of 7*l.* 16*s.* per annum at sixty-five. There are other provisions for the cases of widows of persons dying before sixty-five, and for other contingencies. There can be no doubt that this is a serious and businesslike attempt to grapple with the problem before us, but it seems open to the objection that it only touches the fringe of the question. By it only the willing fish would be swept into the net, while the too numerous small fry, anxious to elude the cast of the fisherman, whose especial object it nevertheless is to secure them, are allowed to swim away at their ease in the sea of thriftlessness and prospective pauperism. No one who knows the mental attitude and habits of thought prevalent among a large proportion of the working-classes can have failed to note the force of the resistance which they are too often inclined to oppose to any attempt, however gentle, to bring them into the disagreeable position of making definite arrangements even for the immediate future, and of practising anything like systematic self-denial. The inveterate dislike to looking forward, the hopefulness that in some cases seems actually to

grow as misfortunes thicken, the daily evidence that 'muddling on' often does not in fact lead to any decisive or irretrievable catastrophe—all these contribute to encourage a 'happy-go-lucky' existence, and to fortify the belief that without any special effort life may not improbably be lived without great distress, and in due time brought to a fairly satisfactory end. With these fatalistic views and ideas, can we wonder that there is so little thought of the morrow? It is to be feared that such a scheme as Mr Chamberlain's, notwithstanding the manifest advantages which it offers, would not be widely adopted except by the comparatively small number of prudent people who are already prepared to make the effort necessary to secure a provision for their old age.

Many other schemes of more or less importance and interest have been put forward. Some are ingenious ; some appear to contemplate the problem from one point of view only ; others are, I had almost said, fantastic. An able and useful work by Mr. J. A. Spender, published in February last, and entitled 'The State and Pensions in Old Age,' with a preface by Mr. Arthur Acland, M.P., discusses the merits of the more important proposals which had then been made, and contains much valuable information.

Among the contributions to the literature of the subject should be mentioned a pamphlet by the Rev. T. W. Fowle, rector of Islip, with the title 'The Poor Law, the Friendly Societies, and Old-Age Destitution—a proposed Solution.' Mr. Fowle advocates the gradual extinction of outdoor relief within a period not exceeding twenty-five years, and the allocation to the friendly societies of the sum thus saved, which he reckons, including cost of management, at 3,000,000*l.* per annum, on condition that they should in return guarantee a sufficient maintenance to all their members permanently disabled by sickness or old age. He further proposes that, in consideration of this subsidy, the societies should be required to be, or to become, efficient, and to subject their tables, investments, and rules to the sanction of a Government authority. I do not think the societies would consent to this arrangement. It would doubtless have the eventual effect of putting them all on a solvent basis, except those whose financial position is clearly hopeless, and whose extinction might be contemplated, as Mr. Fowle contends, with equanimity. But such an interference with the affairs of the societies generally would be resented, and their opposition to it, and to any general scheme of pensions which would affect their position and objects, could hardly be considered unnatural or altogether selfish, composed as they are in the main of the flower of the working-classes, keenly alive to the advantages of their independence, and to the evils which any infringement of it might entail.

A striking instance of the feeling in this matter is afforded by the speech of the Grand Master of the Manchester Unity of Oddfellows at the annual Congress of Delegates held at Derby in June last. Speaking of old-age pensions, Mr. Bytheway said :—' For the State to assume that a man in these days was not in a position to earn for himself sufficient to put by to keep himself in old age without assist-ance from the State would have a most demoralising effect, and would be impolitic on national grounds, and calculated to destroy that independence of character that had done so much in the past history of our country to raise and elevate the people, and encourage thrift upon the only true basis—industry, self-help, and self-denial, and, therefore, to create a strong self-reliance in its train of good re-sults. If the lazy or drunken were to fare alike with the temperate and industrious, this would not encourage thrift ; and by giving pensions all round it would certainly not be an encouragement to the better members of society, but would act in a contrary direction. The suggestion that the medium for granting State pensions should be through the agency of friendly societies perhaps more immediately concerned them. As they had built up for themselves a position, and accumulated large funds by the exercise of liberty in managing their own affairs, he would not advise running the risk of losing this liberty and selling their own birthright for a mess of pottage, and having the right of self-management curtailed by any intermeddling on the part of the State, which would be sure to follow if State aid were accepted by them in their aggregate capacity as Odd-fellows. Any friendly society accepting such aid would, no doubt, very soon be

astonished many of his admirers, and which, coming from any other quarter, would, I venture to say, have been generally characterised, if not as Utopian, at least as affording to our social and political intelligences, in their present imperfect state of development, no food for serious discussion. It is nothing less, as is well known, than a scheme for universal pensions, or general endowment of old age. With his usual straightforwardness Mr. Booth at the outset informs his readers [1] that, as there are at present 733,000 women and 590,000 men, or about 1,323,000 persons in all, above sixty-five years of age in England and Wales, a universal pension list for those parts of the United Kingdom alone would amount, at 13*l*. each, to 17,000,000*l* per annum. This sum is reduced by an anticipated contribution of 4,000,000*l*. from the local authorities in consideration of the reduction which would be effected in the rates, and the total amount to be provided by Imperial taxation for carrying the scheme into effect throughout the United Kingdom is estimated at 16,000,000*l*. per annum. Mr. Booth anticipates that such a sum could be raised without difficulty, by direct and indirect taxation, which latter might include increased duties on sugar and drink, ' provided there be any desire that the thing should be done.' I should fear that the means proposed would be quite sufficient to counteract any such desire, and Mr. Booth is unquestionably right in adding that ' if the project does not so far commend itself to the community as to make the necessary sacrifice welcome, no sensible statesman could be expected to take it up.'

But even if there should be any such widely expressed desire, let us see whether the scheme should commend itself in any degree to our matured ideas of self-government or to our long experience of the working of the Poor Law and charitable and other agencies. It is proposed that every man and woman in the United Kingdom should, after sixty-five, receive a pension—duke and dock-labourer, countess and costermonger. Every person, whatever his or her position or antecedents, whether good or bad, rich or poor, thrifty or reckless, is to be treated in precisely the same way. No man, however wealthy or neglectful of his plainest duties to society, however drunken or improvident, as soon as he has reached the magic age, is to be debarred from the right to receive his pension. Is there any merit, I would ask, in living to sixty-five? and cannot a man or woman who has attained that age be almost as great a discredit to society as at any preceding time of life? Surely the mere fact of attaining a certain age should not obliterate the equally certain fact, it may be, that a man's whole career has been a negation of his duty as a citizen and even as a decent human being? Nor can I pass over as futile some of the many objections to the scheme which Mr. Booth mentions, and with which he deals. Among these are, that the hard-working and thrifty would pay for the idle and worthless, and that it is unjust as well as impolitic that the undeserving and those who have done nothing to help themselves should benefit equally with the thrifty and deserving. Mr. Booth contends (I quote his own words) that as ' according to the present law every drunken, immoral, lazy, ill-tempered old man or woman now existing has a right to demand the shelter of the workhouse,' there can, therefore, be no harm in according to such people a weekly allowance of five shillings, which is in effect less than they would cost in the workhouse. I think the difference between the two cases is obvious. The financial results of each arrangement, to the payer of rates and taxes, may be nearly identical, but surely we ought to look further than this and see to it that the deserving citizen is not confronted with the spectacle of his undeserving brother living upon an allowance which he has done nothing to earn, in as perfect freedom as himself, and with every advantage, so far as the law goes, which he himself enjoys. I do not think this would be a very edifying state of things, nor one likely to promote thrift. To this second point Mr. Booth only answers that ' it is even more subtly dangerous to inquire into a man's character than into his means, if the benefit to be received is to be kept free from all taint of pauperism.' I confess that it disturbs me little, as I conceive it would disturb our disreputable friend still less, to add the taint of pauperism to the many worse taints with which

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[1] *Pauperism—a Picture; and Endowment of Old Age—an Argument* London (Macmillan & Co ), 1892, p. 64

he has been polluted, and to which he has become indifferent, during a long and ill-spent life. If he could look forward to his pension, as Mr. Booth proposes, would he feel a glow of moral superiority and of conscious pride in his manhood? Hardly. His motto would only vary the Epicurean 'Let us eat and drink, for to-morrow we die' to 'Let us eat and drink, for to-morrow' (that is, when we complete our sixty-fifth year) 'we shall get pensions of 5s a week.' But in regard to his prospective pensioners generally, however deserving, Mr. Booth admits 'that a provision for old age, obtained compulsorily under the law, and paid out of taxation, would carry with it none of the moral benefit which would attend the winning of a pension by direct personal sacrifice . . nor would it directly minister to independence of character,' though he contends, in a somewhat too sanguine spirit as it appears to me, 'that no one would make less voluntary effort to save because of it, and that many would increase their exertions in this direction.' I should have thought that, on the contrary, looking to the widespread inclination to prefer provision against sickness to insurance for old age, which is a well-known feature in the habits of the working classes, and to which the arrangements of the friendly societies bear such striking witness, the mere fact of having a pension of 5s a week to fall back upon would be sufficient to deter most of them from making further provision for the declining years which they may never live to see. Granted, however, that the advantages of a universal pension scheme from taxation were fully shown, there still remain several points touched upon by Mr. Booth which should make the cautious mind pause before consenting to its adoption. For instance, Mr. Booth says: 'It is not to be forgotten or disguised that year by year the sum needed' (for pensions) 'must steadily increase, faster very likely than the rate of increase of the whole population. . . . Happily, wealth is increasing faster than population.' When it is considered that the initial cost is estimated at 16,000,000*l* per annum, this is a very disquieting suggestion, and quite sufficient in itself to make the boldest hesitate before plunging into such a sea of uncertainty. Then the administrative details of carrying the scheme into effect would necessarily be somewhat complex; and Mr. Booth shows that, when the official army of registrars and superintendent-registrars has been set in motion, the arrangements for fixing the age of the applicant made, and the precautions against fraudulent claims in two places taken, a great deal of difficult and harassing work will have been done. He contends, indeed, that the system is simple as compared with any scheme of national insurance, but he says enough to show that, as might be expected, a very considerable amount of trouble both to officials and claimants will be inevitable. And, finally, he makes no provision for the expenses of the scheme, which, he thinks, need not exceed from 10*s*. to 20*s*. in each case, suggesting that this amount 'could be deducted from the first payments of pension, at the rate of 2*s* 6*d*. a week till paid.' It is to be feared that, looking to the necessity which there certainly would be, especially in towns, of keeping a constant watch over each case to prevent fraud, such as the drawing of a pension after the decease of the pensioner, the services of registrars would be in pretty continuous demand, and those services would have to be paid for.

But it may be said that national pension schemes have been set on foot in other countries, and that there is no reason why we should be behindhand in the good work It is true that in Germany three insurance laws have been passed, and according to Mr. Wilhelm Bode—whose article in the 'National Review' of March last should be read by all interested in the question—the latest, that for old age and sickness, is by far the most unpopular. It is generally called the 'Klebegesetz,' or 'Sticking Law,' from the immense number of stamps which it is necessary to use in carrying its provisions into effect, and its administration appears to have been found intolerable. A report on the working of the law during the first year (1891), made by Herr von Bötticher in the German Reichstag in February last, shows that there were 173,668 claims for old-age pensions under this law during the year, of which 132,917 were allowed, the average amount of pension being 125 marks, or 6*l*. a year. It is to be observed that these persons obtained pensions without having contributed anything to the insurance fund. Mr. Bode states that

Herr von Bötticher congratulated himself that a larger number of persons called upon to insure had not absconded, and that the discontent caused by it was not greater. adding that no wish was felt in the country for a continuation of social reform laws. This latest social law certainly does not appear to have been attended with encouraging results. Already a popular movement for its repeal has made some way in Bavaria, but there is of course but little prospect of getting rid of it at present Meanwhile the self-help societies have either ceased to exist or have been greatly crippled, and Mr Bode can only hope that they will one by one come back, and that the energy of German manhood, sapped by the compulsory system, will return, and the dishonesty which it fostered die out. His article concludes with an earnest appeal to England not to encumber herself ' with any big scheme of any impatient State socialist,' but to remain, ' what she has been so long, the chosen land of the free—of the men who help themselves.' I think such an appeal should touch us nearly.

In June 1891 a Bill was presented to the French Chamber of Deputies by M. Constans, then Minister of the Interior, and M Rouvier, Minister of Finance, for the creation of a ' Caisse Nationale des Retraites Ouvrières,' or pension fund for the benefit of workmen and others employed in trade, farm labourers, and domestic servants of both sexes, whose income does not exceed 3,000 francs (120l.) per annum. All persons in this position will be considered to be willing to take advantage of the benefits of the fund, unless they make a declaration of unwillingness before the mayor of their place of residence. It is proposed that the fund should be formed by equal contributions from the depositor and his employer, which are either to be paid into the newly established ' Caisse,' or into duly authorised provident societies already existing, and by an addition to be made by the State equal to two-thirds of those contributions. The latter are to consist of not less than five centimes nor more than ten centimes per working-day contributed both by the workman and his employer; and, taking the average number of actual working-days in the year at 280, so as to allow for holidays, slackness of work, and sickness, it is calculated that, after thirty years of continuous saving, five centimes per day put by from each source, and invested at 4 per cent., should amount to a pension of 180 francs, and ten centimes per day to a pension of 360 francs, per annum. These amounts not being considered sufficiently high to tempt the class whom it is desired to benefit, it is proposed, as mentioned above, that the State should materially add to them. The term of years over which the contributions are to spread is limited to thirty, as, owing to compulsory army service, it is considered that contributors will hardly have settled down to steady work before the age of twenty-five, and that but few persons would be willing to continue the necessary payments beyond the age of fifty-five or fifty-six. On arriving at the time for pension, the contributor must be able to prove that his income is not more than 600 francs per annum. The Bill also contains provisions for life insurance, the State contributing towards the payment of the annual premiums; for the payment of their pensions to contributors who have become permanently incapacitated through sickness, and for the relief of those who may be obliged on account of accidents to interrupt their payments into the fund. Several other Bills have been brought forward by independent deputies with analogous objects, into the details of which time will not permit me to enter. It is interesting, however, to note that some of the methods proposed for raising the funds necessary to enable the State to grant pensions are hardly such as would commend themselves to our ideas, as, for instance, the proposal in a Bill presented by several deputies that all collateral successions to property should be suppressed, and that a sliding scale of succession duty should be fixed, rising from 1 per cent. on sums below 10,000 francs to no less than 75 per cent. on sums above 1,000,000 francs. Hardly less interesting is the suggestion of another enthusiastic legislator that, at the central office of the pensions department, to be placed in the Louvre, there should be a museum in which a ' golden book ' should be kept for inscribing the names of donors of not less than 100 francs to the pension fund, while the generosity of donors of 10,000 francs should be recorded on a marble tablet, and that of princely subscribers of not less than 100,000 francs by a bust.

In Italy the question of establishing a National Pension Fund has also been widely discussed, and in past years several schemes have been proposed to the Legislature. A Bill is now before the Chamber providing for the establishment of a central governing body, whose duty it would be to administer funds partly subscribed by authorised savings banks or other self-help societies and by individuals, and partly by the State from various specified sources. Every Italian, man or woman, certified to belong to the working classes, may subscribe to the fund, but not more than 500 lire, or 20l. per annum, and every person who has subscribed for not less than twenty years is to be entitled at sixty to a pension, the amount of which is to be determined by the amount of the contributions made to the funds with the addition of compound interest. No pension may exceed 20l. per annum. Provision is also made, in the case of the subscriber's death, for the payment to his representatives of all contributions and interest   It is evident that this scheme does not go very far in the direction of establishing a general Old-Age Pension Fund.

In April 1891 the Danish Legislature passed a law giving every Danish subject, man and woman, the right to a pension at sixty years of age.   Exception is made of persons who have been convicted of crime, who have fraudulently made over their property to relations or others, who have brought themselves to distress by extravagance or evil-living, who have during the preceding ten years received relief from the poor law (assistance publique), or who have been convicted of mendicity.   Applications for pensions are to be addressed to the parish (commune), who will make all inquiries, and fix the amount of the relief to be granted, which may be in money or in kind.   The relief may be withdrawn if the pensioner should become ineligible through misconduct or spend his pension improperly, and, if he marries, his pension is *ipso facto* withdrawn, and he becomes chargeable to the poor law.   It will be seen, therefore, that there is in Denmark no sentimental objection to an inquiry into an applicant's moral character and pecuniary position such as Mr. Booth so strongly deprecates, and that the so-called pensions are but an extension of the system of what we should call outdoor relief.   The pension is to be derived from the parish, subject to certain conditions as to the applicant's place of birth, or, if the place of birth cannot be determined, from the poor-law, and the State contributes half the expenses of the parishes in distributing the relief, provided that those expenses do not exceed one million crowns (55,000l.) in each of the years 1891–95, and two million crowns (110,000l) in subsequent years. No appeal lies against the decision of the communal authorities.

It is evident that, as only one of the three schemes last mentioned is in actual operation, they cannot as yet be fully judged, but I venture to question whether there is anything which we could think of following here.   In this country there is, no doubt, a holy horror of the workhouse, but there is also a perhaps unreasonable prejudice in favour of 'going as you please,' and a scarcely less pronounced aversion, upon the whole reasonable and certainly characteristic, to being what the French would call 'administered.'   I can hardly imagine my countrymen, of any class or disposition, subjecting themselves to a regular system of Government interference in affairs of which the management, or mismanagement for the matter of that, they have always considered to be a Briton's birthright.   Let us ask ourselves, after looking at the question in all its bearings, whether we must not give up the idea of anything like compulsion in matters of thrift, if indeed we must not also give up the idea, when it came to the point, of anything in the nature of Government help and intervention.

But is there, then, no way in which help can be rendered to our deserving poor? Must the present state of things go on, and the public conscience continue to be shocked at the sight of thousands of old people lapsing hopelessly into pauperism? Let us examine the present state of things, and look a little into its causes.   How is old-age pauperism brought about ?   There is doubtless a not inconsiderable part of our population which might make at least some provision for old age, but which prefers the careless living from hand to mouth, and considers subscription to a burial club the only claim which the future has upon it.   As I have already said, even

where there is some thought of the morrow, inveterate habit leads many bread-winners to think more of the immediate than of the comparatively distant future, and to provide rather against the risk of accident or illness by joining a sick-club than against the remote prospect of destitution when the day of work is over. When all this is conceded there must remain, no doubt, many cases of unforeseen and undeserved misfortune, in which old age overtakes the toiler without his having had a chance of making provision for it—cases where wages have hardly ever been such as to allow of saving, where families have been large and sickly, where the struggling widow, work and pinch how she might, has had difficulty in keeping the wolf from the door. These are the hardships with which we must all sympathise—these are the sorrows we should all wish to relieve. Putting unavoidable misfortune aside, however, for the moment, let us consider whether our present system is such as to offer the maximum amount of encouragement to self-help and self-reliance, and the minimum amount of encouragement to an easy-going frame of mind which looks forward to pauperism with equanimity. What are the prospects, generally speaking, of the average worker who has made no provision for his old age ? He sees the system of outdoor relief in full operation ; he knows that unless and until he becomes utterly helpless and friendless, a dole will be made to him which will keep him from starvation, and he learns to look forward to that dole without repugnance and without dismay. The circumstances under which it is allotted to him make but little change in his family arrangements. His able-bodied children, if he has any, are seldom called upon by the Guardians to make any great sacrifice for him, and he sinks down into a more or less contented, but complete and hopeless, pauperism. I say that a community which tolerates and maintains such a system incurs a grave responsibility, and, so long as it makes no effort to improve it, has no right to wax impatient at the crying evil of old-age pauperism. And if a change in the system is possible, surely we ought to consider whether it cannot and ought not to be made before we seek by heroic measures to set aside arrangements susceptible of gradual improvement and substitute for them a state of things which would perpetuate many of the worst evils of dependence. If we had reason to believe that the Poor Law could only be administered in the manner indicated above, we should perhaps be justified in at once looking outside it for means to improve the condition of our aged poor. But the very reverse is the case. We have abundant evidence that by firm and patient administration the condition of whole districts in regard to pauperism may be radically changed, to the great benefit, material and moral, of the poorer inhabitants During the last twenty years experiments in this direction have been made both in urban and rural districts, not conceived in the spirit of empiricism or caprice, but undertaken as the result of ripe experience and with a single eye to the real interests of the poor, which have been attended with complete success. The tendency of the reforms effected has been, as is well known, towards a great reduction, and in some cases the total abolition, of outdoor relief. In the winter of 1869–70 the Guardians of Whitechapel, one of the poorest districts in London, had forced upon them the necessity of reviewing their position. Up to that time, in the words of Mr. Vallance, the Clerk to the Guardians :

'The system may be said to have been that of meeting apparent existing circumstances of need by small doles of outdoor relief, the indoor establishments . . . . being reserved for the destitute poor who voluntarily sought refuge in them. Able-bodied men who applied for relief on account of want of employment were set to work under the Outdoor Relief Regulation Order, and, in return for such work, were afforded outdoor relief in money and kind. Under this system, the administration was periodically subjected to great pressure, so much so that the aid of the police had not infrequently to be invoked to restrain disorder and afford necessary protection to officers and property. Police protection was even at times required for the Guardians during their administration of relief.'

In such circumstances, it is not to be wondered at that the Guardians should have earnestly endeavoured to reform 'a system which was felt to be fostering pauperism and encouraging idleness, improvidence, and imposture, while the "relief" in no true sense helped the poor' They gradually restricted outdoor

relief in 'out-of-work' cases, and subsequently in other cases also   Sick persons, widows, and the aged and infirm, were only relieved out of the workhouse on conditions strictly applicable to their individual cases.   The latter class were not so relieved unless it was proved that they had been thrifty and had no children or other relations legally or morally liable to support them and able to do so, and even they ceased to be a charge on the rates after the establishment of the Tower Hamlets Pension Fund, which was formed for the express purpose of saving the really deserving poor from the Poor Law   The result has been that outdoor relief has gradually ceased to exist in Whitechapel, and that no cases, other than those of sudden or urgent necessity relieved by the relieving officer in kind, have been added to the Outdoor Relief lists for more than twenty years.   Notwithstanding this, the number of indoor paupers has not increased   Inquiry was made into every case in which outdoor relief was withdrawn during the two years ended Lady Day 1875, from able-bodied widows and deserted women, the most helpless of all classes, with the result that out of 167 cases, comprising 600 individuals, 77 were found to be doing as well as or better than when in receipt of outdoor relief without further assistance, 52 were obtaining an independent living after having received assistance from charitable agencies or other sources, 10 had been admitted to the workhouse, 18 had apparently left the district, two had died, and eight only, owing to vicious habits or the refusal of the assistance offered, were believed to be not doing well.   It will thus be seen that of the 167 cases no less than 129 had been taken from the ranks of pauperism, with the best results to themselves, to say nothing of the ratepayers, and with moral results to the community at large which cannot be described in detail here, but which must be obvious. Mr. Loch, the Secretary of the Charity Organisation Society, in his work 'Old-Age Pensions and Pauperism,'[1] has given details showing what the results have been of a similar Poor Law policy in two other poor Metropolitan Unions, Stepney and St George's-in-the-East, the latter, taken as a whole, being the poorest of all the Unions in London, and comparing them with the Unions of the Strand and Bethnal Green.   He has also shown the results of careful administration in Unions the very opposite to the London districts above-mentioned, namely, the rural Unions of Brixworth in Northamptonshire and Bradfield in Berkshire, as compared with the two similar Unions of Linton in Cambridgeshire and Midhurst in Sussex. The facts brought out by these two sets of comparisons are striking and conclusive. In Unions in which there has been a careful administration of the Poor Law for a period, more or less, of twenty years, it has been proved that the proportion of paupers over sixty to population can be reduced in the country to about four per cent., and in London, judging by its poorest Union, by more than half the present number.   Can it be doubted that in these cases the bugbear of old-age pauperism has already been faced and in a great measure dispelled, and is there any reason, beyond the force of habit and a *vis inertiæ* which surely might be grappled with, why both town and country Guardians should not follow the example set them by the pioneers of this movement, and by a common effort subdue the common enemy?   Let us not, with the experience we have to guide us, be led astray in this matter, though some of our philanthropists and political economists seem inclined to countenance a large increase of public expenditure in connection with the relief of the poor.   Among the many nostrums for the cure of the disease under which the State is supposed to labour is a proposal by Mr. Bartley, M P., embodied in the 'Old Age Provident Pension Bill' which he brought in during the last session of Parliament.   Mr. Bartley proposes that every person (man or woman) of sixty-five, who is not a criminal or drunkard, and who is unable to earn the wages of his calling, shall be entitled to a pension of 7s. per week from the local authority, which is to be the County Council, provided that he has never received poor-law relief.   If he has purchased an annuity from the Post Office or some friendly society, or paid a lump sum for the purchase of a deferred annuity from the Post Office, or is prepared to pay a lump sum of not less than 10l. to the

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[1] *Old-Age Pensions and Pauperism*   An Inquiry as to the Bearing of the Statistics of Pauperism quoted by the Right Hon J Chamberlain, M P , and others, in support of a scheme for National Pensions (London, Swan Sonnenschein & C , 1892).

local authority, or has partially provided for himself in other ways, to the satisfaction of the local authority, he is to be entitled to a pension of 3s. 6d. per week, with the addition of an extra amount according to the payments which he has made. And further, there are to be pensions, if the local authority should see fit, of 3s. 6d. per week even for the persons declared ineligible, provided that they can show unavoidable illness or misfortune. The necessary funds are to be raised by a special rate to be called the 'Pension Rate.' I need not do more than call attention to these provisions to show how disastrous such a law would be. The burden which would be imposed on the County Councils of deciding upon the merits or demerits of each case; the wide discretion allowed in the award of pensions, even to the criminal and drunkard, if only what can be construed into 'unavoidable misfortune' can be proved; and the danger that absolutely different constructions of the law would prevail in different localities—all combine to make it next to impossible but that its operations should be fatal to the exercise of thrift and should bring back in redoubled force many of the evils of extensive outdoor relief, with greatly increased burdens on the ratepayer. Among the political economists I regret to say that an eminent Professor, and what is more (I speak with bated breath), a former President of this Section, has thrown his great weight into the scale of wide, if not lavish, distribution of outdoor relief. I am not quite sure that I understand Professor Marshall's position, but he propounds in the 'Economic Journal' sixteen questions which he thinks should be considered before any large scheme is undertaken for the relief of the aged, and which seem incidentally to show his antagonism to most of what I had thought to be the generally accepted maxims of poor-relief.

But let it not be supposed that advocates of a firm and careful administration of the Poor Law consider it the only thing required to prevent or deal with all cases of old-age pauperism. In the best-regulated Unions, especially in towns, there will always be cases—too many, alas!—of highly deserving old people who are unable to maintain themselves, and of whom no just person could bear to think as condemned to outdoor relief, and still less to incarceration in a workhouse. These are precisely the cases which are best brought out where outdoor relief has either been entirely abolished or is quite the exception. For these—and experience in well-managed Unions has shown how comparatively few they are—there surely remains the exercise of a well-ordained charity which will step in and prevent a consummation so much to be deprecated. Children and other relations, who under a loose system of poor-relief are too apt to consider that in one shape or other their parents and aged kinsfolk may naturally be left to the tender mercies of the Poor Law, are brought together and induced to contribute to their support; and pension societies, such as the Tower Hamlets Pension Committee, the Local Pension Committees of the Charity Organisation Society, and the like, are willing and anxious to come to the rescue. Nor is this organised assistance to those whom the late Duke of Albany called 'the aristocracy of the poor,' of use to the recipients only. In hundreds of cases which have come under my own knowledge in East London, for instance, it has been the means of inspiring in men and women a holy zeal for charity which, without any hateful feeling of patronage on the one side, or of cringing dependence on the other, gives a scope, such as none other can supply, for a true friendship between rich and poor, and blesses both the giver and the receiver. I have endeavoured to show, in these few and necessarily brief remarks about one of the great social questions which occupy men's minds to-day, that for the promotion of the best interests of our aged poor, there may be a 'more excellent way' than a vast organisation of State-aided pensions. May we work out this and other similar problems, as Englishmen do, calmly, wisely, and to good effect!

But, turning to our immediate duty as members of this Section, let us endeavour to ascertain what we can do to inculcate and foster sound views on these and similar subjects. In the proceedings of this British Association for the Advancement of Science we may see how intimately the work of Section F is

related to that of the Sections in which the physical and mechanical sciences are studied. The extraordinary advances which have of late years been made in the application of science to industry have materially added to the wealth of the working classes, and that wealth is more easily earned than in the past. But their knowledge of the great economic laws upon which true progress must depend has not kept pace with their increased resources; nor can this be the case until they are able to grasp the principles of our subject. It may fairly be urged that the advance in physical science is drawing its relations to economic science closer day by day. May we not be about to witness some of the enormous developments caused by the substitution of machinery for hand-labour which we have for some time past been led to expect? The rapid exhaustion of coalfields, to which Sir Robert Ball has recently again called attention, is leading to the utilisation of power from other sources, a question with which the President of Section G will doubtless deal. We are told that the falls of Niagara develop a force of $4\frac{1}{2}$ million horse-power, or the equivalent of all the steam-power used in the world, and that steps have been taken for the immediate utilisation from this source of 100,000 horse-power, or the equivalent of one forty-fifth part of the steam-power of the globe Advances such as these in the utilisation and transmission of energy must, by extending the means of production, profoundly affect the wage-earning capacity of the workman, and consequently the general relations between employers and employed; and it is the privilege of members of this Section to prepare their countrymen for the altered condition under which they may be called upon to live and work. They must never be weary in setting before all sections of the community the necessity of being ready to face such momentous changes as those which I have indicated, and, if I may be permitted to borrow an illustration from electrical science, I would say that their duty is analogous to that of the 'transformers,' of which so much has lately been heard. They deal less with energy itself than with its control, but their function is so to change forces of unwonted 'potential,' that those forces may cease to be dangerous and disruptive, and may be made to weld the various efforts of humanity into coherence and strength

# ADDRESS

TO THE

# MECHANICAL SCIENCE SECTION

OF THE

# BRITISH ASSOCIATION,

BY

## W. CAWTHORNE UNWIN, F.R.S., M.Inst.C.E.,

PRESIDENT OF THE SECTION.

By what process selection is made of a Sectional President of the British Association is to me unknown. I may confess that it was pleasant to receive the request of the Council to preside at the meetings of Section G, even though much of the pleasure was due to its unexpectedness. I ventured to believe I might accept the honour gratefully, trusting to your kindness to assist me in fulfilling its obligations. Amongst engineers there are many with greater claims than I have to such a position, and who could speak to you from a wider practical experience. Here in Section G, I think it may be claimed that the profession of engineering owes much to some who from circumstances or natural bias have concerned themselves more with those scientific studies and experimental researches which are useful to the engineer, than with the actual carrying on of engineering operations. Here, at so short a distance from the University where Rankine and James Thomson laboured, I may venture to feel proud of being amongst those whose business it has been rather to investigate problems than to execute works.

The year just passed is not one unmemorable in the annals of engineering. By an effort remarkable for its rapidity, and as an example of organisation of labour, the broad gauge system has been extinguished. It has disappeared like some prehistoric mammoth, a large-limbed organism, perfect for its purpose and created in a generous mood, but conquered in the struggle for existence by smaller but more active rivals. If we recognise that the great controversy of fifty years ago has at last been decided against Brunel, at least we ought to remember that the broad gauge system was one only of many original experiments, due to his genius and courage, experiments in every field of engineering, in bridge building, in locomotive design, in ship construction, the successes and failures of which have alike enlarged the knowledge of engineers and helped the progress of engineering.

The past year has seen the completion of the magnificent scheme of water supply for Liverpool, from the Vyrnwy, carried out from 1879 to 1885 by Mr. Hawksley and Mr. Deacon, and since then completed under the direction of the latter engineer. This is one of the largest and most striking of those works of municipal engineering, rendered necessary by the growth of great city communities and made possible by their wealth and public spirit. For the supply of water to Liverpool, the largest artificial lake in Europe has been created in mid-Wales, by the construction across a mountain valley of a dam of cyclopean masonry, itself one of the most remarkable masonry works in the world. The lake contains an available supply of over 12,000 million gallons, its size having been determined not only to supply forty million gallons daily for the increasing demand of Liverpool, but also to meet the necessity imposed by Parliament that an unpre-

cedentedly large compensation, amounting to ten million gallons daily and fifty million gallons additional on thirty-two days yearly, should be afforded to the Severn. The masonry dam, though a little less in height than some of the French dams, is of greater length It is nearly double the length of the great dam at Verviers.[1] Although masonry dams were an old expedient of engineers, it is in quite recent times, and chiefly in consequence of the scientific investigations of French engineers, that they have been revived in engineering practice. Since the construction of the Vyrnwy dam, another very large dam, the Tansa dam, has been completed in Bombay. This dam has a length of two miles and a height of 118 feet, and it is 100 feet thick at the base. The reservoir will supply 100 million gallons per day. In the United States a still greater work of the same kind has been commenced on the Croton river, in connection with the water supply of New York. This dam will have a length of 2,000 feet, and a height of 285 feet. Its greatest thickness will be 215 feet. It will be very much the boldest work of its kind.

Returning to the Liverpool supply, the water taken from the lake at the most suitable level into a straining tower provided with very complete hydraulic machinery, passes through the Hirnant tunnel, and thence by an aqueduct, partly consisting of rock tunnels, partly of pipes 39 in. to 42 in. in diameter, sixty-eight miles in length, being the longest aqueduct yet constructed. The crossing of the Mersey by an aqueduct tunnel has proved the greatest engineering difficulty to be surmounted. The tunnel has been carried through layers of running sand, gravel, and silt. At first slow progress was made, but later, by the adoption of the Greathead system of shield, with air locks and air-compressing machinery, as much as fifty-seven feet of tunnel were driven and lined in one week. The whole work is now complete, and Liverpool has available an extra supply of very pure water, amounting to forty million gallons daily.

A scheme of water supply for Manchester from Lake Thirlmere in Westmoreland, on an equally large scale, is approaching completion. Birmingham is likely to carry out another work of the same kind. And London, at a greater distance from pure water sources and under greater difficulties from the complexity of existing interests, has come to realise that, within fifty years, a population of $12\frac{1}{2}$ millions will probably have to be provided for. To supply such a population, a volume of water is required ten times as great as the whole available supply from Lake Vyrnwy.

Here in Edinburgh one remembers that the birthplace of the steam-engine is near at hand. A century and a quarter ago, James Watt made an invention which has profoundly influenced all the conditions of social, national, commercial, and industrial life. It is due to the steam-engine more than to any other single cause that the population in this country has tripled since the beginning of the century, and that we have become dependent on steam-power for fuel, for transport, for manufactures, in many cases for water supply, for sanitation, and for artificial light. From some German statistics it appears that there are probably now in the world, employed in industry, steam-engines exerting forty-nine million horse-power, besides locomotives exerting six million horse-power Engines in steamships are not included. The steam-engine has become a potent factor in civilisation, because it places at our disposal mechanical energy at a sufficiently low cost, and the efforts of engineers have been steadily directed to diminishing the cost at which steam-power is produced. Members of one great branch of our profession are much concerned in the production of mechanical energy at a sufficiently cheap rate. They require it in very large quantity for transformation into light and for re-transformation into mechanical energy under conditions more convenient than the direct use of steam-power. Perhaps it will not be inappropriate if in Section G I first discuss briefly some of the causes which have made the steam-engine inefficient and the extent to which we are getting to a scientific knowledge of the methods of evading them. I propose then to consider some of the methods of economising the cost and increasing the convenience of

[1] The length of the dam from rock to rock is 1,172 feet. Height from lowest part of foundation to parapet of carriage way, 161 feet. Height from bed of river to overflow sill, 84 feet. Thickness of masonry at base, 120 feet.

mechanical power by generating it at central stations and distributing it, and, lastly, how far means of transporting energy are likely to make available cheaper sources of energy than steam-power.

Let us go back for a moment to James Watt. The most distinct feature about the invention of the steam-engine is that it arose out of studies of such questions as the relation of pressure and temperature of steam, the heat absorbed in producing it, and its volume at different pressures.

Armed with this knowledge, Watt was able to determine that the quantity of steam used in a model atmospheric engine was enormously greater than that due to the volume described by the piston. There was waste or loss. To discover the loss was to get on the path of finding a remedy. The separate condenser, by diminishing cylinder condensation, annulled a great part of the loss. So great was Watt's insight into the action of the engine that he was able to leave it so perfect that, except in one respect, little remained for succeeding engine builders, except to perfect the machines for its manufacture, to improve its details, and to adapt it to new purposes Now it very early became clear that there were two directions of advance which ought to secure greater economy. Simple mechanical indications showed that increased expansion ought to insure increased economy. Thermodynamic considerations indicated that higher pressures, involving a greater temperature range of working, ought to secure greater economy. But in attempting to advance in either of these directions, engineers were more or less disappointed Some of Watt's engines worked with 5 lbs. of coal per indicated horse-power per hour. Many engines with greater pressures and longer expansions have done but little better. The history of steam-engine improvement for a quarter of a century has been an attempt to secure the advantages of high pressures and high ratios of expansion. The difficulty to be overcome has proved to be due to the same cause as the inefficiency of Watt's model engine. The separate condenser diminished but it did not annul the action of the cylinder wall The first experiments which really startled thoughtful steam engineers were those made by Mr. Isherwood, between 1860 and 1865. Mr. Isherwood showed that in engines such as those then in use in the United States Navy, with the large cylinders and low speeds then prevalent, any expansion of the steam beyond three times led, not to an increased economy, but to an increased consumption of steam. Very little later than this M. Hirn undertook, in 1871-5, his classical researches on the action of the steam in an engine of about 150 indicated horse-power. Experiments of greater accuracy or completeness, or of greater insight into the conditions which were important, have never since been made, and Hirn with his assistants, MM. Hallaner and Dwelshauvers Dery, has determined, once for all, the whole method of a perfect steam-engine trial. M. Hirn was the first to clearly realise that the indicator gives the means of determining the steam present in the cylinder during every period of the cycle of the engine. Consequently, superheating in ordinary cases being out of the question, we have the means of determining the heat present and the heat already converted into work. The heat delivered into the engine is known from boiler measurements, combined with calorimetric tests of the quality of the steam, tests which Hirn was the first to undertake. The balance or heat unaccounted for is, then, a waste or loss due to causes which have to be investigated. Hirn originated a complete method of analysis of an engine test, showing at every stage of the operation the heat accounted for and a balance of heat unaccounted for ; and the latter proved to be a very considerable quantity.

Meanwhile theoretical writers, especially Rankine and Clausius, had been perfecting a thermodynamic theory of the steam-engine, based primarily on the remarkable and irrefragable principle of Carnot. The result of Hirn's analysis was to show that these theories, applied to the actual steam-engine, were liable to lead to errors of 50 or 60 per cent, the single false assumption made being that the interaction between the walls of the cylinder and the steam was an action small enough to be negligeable

In this country Mr Mair Rumley, following Hirn's method, made a series of experiments on actual engines with great care and accuracy and completeness. All these experiments demonstrated the fact of a large initial condensation of steam on the walls of the cylinder, alike in jacketted and unjacketted engines. This con-

densed steam is re-evaporated partially during expansion, but mainly during exhaust, and serves as a mere carrier of heat from boiler to condenser, in conditions not permitting its utilisation in producing work.

It became clear from Hirn's experiments, if not from the earlier experiments of Isherwood, that for each engine there is a particular ratio of expansion for which the steam expenditure per horse-power is least. Professor Dery has since deduced from them that the practical condition of securing the greatest efficiency is that the steam at release should be nearly dry. In producing that dryness the jacket has an important influence. In spite of much controversy amongst practical engineers about the use of the jacket, it does not appear that any trustworthy experiment has yet been adduced in which there was an actual loss of efficiency due to the jacket. In the older type of comparatively slow engines it is a rule that the greater the jacket condensation the greater the economy of steam, even when the jacket condensation approaches 20 per cent. of all the steam used. It appears, however, that as the speed of the engine increases, the influence of the jacket diminishes, so that for any engine there is a limit of speed at which the value of the jacket becomes insignificant.

Among steam-engine experiments directed specifically to determine the action of the cylinder walls, those of the late Mr. Willans should be specially mentioned. Mr. Willans' death is to be deplored as a serious loss to the engineering profession. His steam-engine experiments, some of them not yet published, are models of what careful experiments should be. They are graduated experiments designed to indicate the effect of changes in each of the practically variable conditions of working. They showed a much greater variation of steam consumption (from 46 to 18 lbs. per indicated horse-power hour) in different conditions of working than, I think, most practical engineers suspected, and this has been made more significant in later experiments, on engines working with less than full load. The first series showed that in full load trials the compound was superior to the simple engine in practically all the conditions tried, but that the triple was superior to the compound only when certain limits of pressure and speed were passed.

As early as 1878 Prof. Cotterill had shown that the action of a cylinder wall was essentially equivalent to that of a very thin metallic plate, following the temperature of the steam. The exceedingly rapid dissipation of heat from the surface during exhaust especially being due to the evaporation of a film of water initially condensed on its surface. In permanent *régime* the heat received in admission must be equal to that lost after cut off In certain conditions it appeared that a tendency would arise to accumulate water on the cylinder surfaces, with the effect of increasing in certain cases the energy of heat dissipation. Recently Prof. Cotterill has been able to carry much further the analysis of the complex action of condensation and re-evaporation in the cylinder and to discriminate in some degree between the action of the metal and the more ambiguous action of the water film. By discarding the less important actions, Prof. Cotterill has found it possible to state a semi-empirical formula for cylinder condensation in certain restricted cases which very closely agrees with experiments on a wide variety of engines. It is to be hoped that, with the data now accumulating, a considerable practical advance may be made in the clearing up of this complex subject. There are, no doubt, some people who are in the habit of depreciating quantitative investigations of this kind. They are as wise as if they recommended a manufacturer to carry on his business without attending to his account books. Further, the attempt to obtain any clear guidance from experiments on steam-engines has proved a hopeless failure without help from the most careful scientific analysis. There is not a fundamental practical question about the thermal action of the steam-engine, neither the action of jackets or of expansion or of multiple cylinders, as to which contradictory results have not been arrived at, by persons attempting to deduce results from the mass of engine tests without any clear scientific knowledge of the conditions which have affected particular results. In complex questions fundamental principles are essential in disentangling the results. Interpreted by what is already known of thermodynamic actions, there are very few trustworthy engine tests which do not fall into a perfectly intelligible order. There is only one known

method, not now much used, by which the cylinder condensation can be directly combated. Thirty years ago superheating the steam was adopted with very considerable increase of economy. It is likely that it was thought by the inventor of superheating that an advantage would be gained by increasing the temperature range. If so, his theory was probably a mistaken one. For the cooling action of the cylinder is so great that the steam is reduced to saturation temperature before it has time to do work; but the economy due to superheating was unquestionable, and was very remarkable considering how small a quantity of heat is involved in superheating. The heat appears to diminish the cylinder wall action so much as almost to render a jacket unnecessary. The plan of superheating was abandoned from purely practical objections, the superheaters then constructed being dangerous. Recently superheating has been tried again at Mulhouse by M. Meunier, and his experiments are interesting because they are at higher pressures than in the older trials and with a compound engine. It appears that even when the superheater was heated by a separate fire there was an economy of steam of 25 to 30 per cent. and an economy of fuel of 20 to 25 per cent., and four boilers with superheating were as efficient as five without it

It may be pointed out as a point of some practical importance that if a trustworthy method of superheating could be found, the advantage of the triple over the compound engine would be much diminished. For marine purposes the triple engine is perfectly adapted. But for other purposes it is more costly than the compound engine, and it is less easily arranged to work efficiently with a varying load.

There does not seem much prospect of exceeding the efficiency attained already in the best engines, though but few engines are really as efficient as they might be, and there are still plenty of engines so designed that they are exceedingly uneconomical. The very best engines use only from 12 to 13 lbs. of steam per indicated horse-power hour, having an absolute efficiency reckoned on the indicated power of 16 per cent., or reckoned on the effective power, 13 per cent. The efficiency, including the loss in the boiler, is only about 9 per cent. But there are internal furnace engines of the gas-engine or oil-engine type in which the thermal efficiency is double this.

In his interesting address to this Section in 1878, Mr. Easton expressed the opinion that the question of water-power was one deserving more consideration than it had lately received, and he pointed to the variation of volume of flow of streams as the principal objection to their larger utilisation. Since that time the progress made in systems of transporting and distributing power has given quite a new importance to the question of the utilisation of water-power. There seems to be a probability that in many localities water-power will, before long, be used on a quite unprecedented scale, and under conditions involving so great convenience and economy that it may involve a quite sensible movement of manufacturers towards districts where water-power is available.

If we go back to a period not very distant in the history of the world, to the middle of the last century, we reach the time when textile manufactures began to pass from the condition of purely domestic industries to that of a factory system. The fly-shuttle was introduced in 1750, the spinning-jenny was invented in 1767, and Crompton's machine only began to be generally used in 1787. It was soon found that the new machines were most suitably driven by a rotary motion, and after some attempts to drive them by horses, water-power was generally resorted to. In an interesting pamphlet on the Rise of the Cotton Trade, by John Kennedy, of Ardwick Hall, written in 1815, it is pointed out that the necessity of locating the mills where water-power was available, had the disadvantages of taking them away from the places where skilled workmen were found, and from the markets for the manufactured goods. Nevertheless, Mr. Kennedy states that for some time after Arkwright's first mill was built at Cromford, all the principal mills were erected near river falls, no other power than water-power having been found practically useful. 'About 1790,' says Mr. Kennedy, ' Mr. Watt's steam-engine began to be understood, and waterfalls became of less value. Instead of carrying the workpeople to the power, it was found preferable to place the power amongst the people.'

The whole tendency of the conditions created by the use of steam-power has been to concentrate the industrial population in large communities, and to restrict manufacturing operations to large factories. Economy in the production of power, economy in superintendence, the convenience of the subdivision of labour, and the costliness of the machines employed, all favoured the growth of large factories. The whole social conditions of manufacturing centres have been profoundly influenced by these two conditions—that coal for raising steam can be easily brought to any place where it is wanted, and that steam-power is more cheaply produced on a large scale than on a small scale. It looks rather, just now, as if facilities for distributing power will to some extent reverse this tendency.

Let me first point out that water-power, where it is available, is so much cheaper and more convenient than steam-power that it has never been quite vanquished by steam-power.

I find, from a report by Mr. Weissenbach, that in 1876, 70,000 horse-power derived from waterfalls were used in manufacturing in Switzerland. According to a census in 1880, it appears that the total steam and water power employed in manufacturing operations in the United States was 3,400,000 horse-power. Of this, 2,185,000 horse-power, or 64 per cent., was derived from steam, and 1,225,000 horse-power, or 36 per cent., from water. In the manufacture of cotton and woollen goods, of paper and of flour, 760,000 horse-power were obtained from water, and 515,000 horse-power from steam. If statistics could be obtained from other countries, I believe it would be found that a very large amount of water-power is actually made available. The firm of Escher Wyss and Company, of Zurich, have constructed more than 1,800 turbines of an aggregate power of 111,460 horse-power.

With a very limited exception all the water-power at present used is employed in the neighbourhood of the fall where it is generated. If means were available for transporting the power from the site of the fall to localities more convenient for manufactures, there can be no doubt that a much larger amount of water-power would be used, and the relative importance of water and steam power in some countries would probably be reversed. It is because recent developments seem to make such a transport of power possible without excessive cost and without excessive loss, that a most remarkable interest has been excited in the question of the utilisation of water-power. Take the case of Switzerland for instance. At the present time Switzerland is said to pay to other countries 800,000*l.* annually for coal. But the total available water-power of Switzerland is estimated at no less than 582,000 horse-power, of which probably only 80,000 are at present utilised. I found a year ago that nearly every large industrial concern in Switzerland was preparing to make use of water-power, transported a greater or less distance. Besides the great schemes actually carried out at Schaffhausen, Bellegarde, Geneva, and Zurich, where water-power is already utilised on a very large scale, there is a project to develop 10,000 horse-power on the Dranse near Martigny.

Hence it is easy to see that problems of distribution of power—that is, the transformation of energy into forms easily transportable and easily utilisable—have now a great interest for engineers

Besides the power required for manufacturing operations, there is a steadily increasing demand for easily available mechanical energy in large towns. For tramways, for lifts, for handling goods, for small industries, for electric lighting, and sometimes for sanitation, power is required. Hitherto steam-engines, or more lately gas-engines, have been used, placed near the work to be done. But this sporadic generation of power is uneconomical and costly, especially when the work is intermittent; the cost of superintendence is large, and the risk of accident considerable. Hence attention is being directed to systems in which the mechanical energy of fuel or falling water is first generated in large central stations, transformed into some form in which it is conveniently transportable and capable of being rendered available by simpler motors than steam-engines.

Just as in great towns it has become necessary to supersede private means of water supply by a municipal supply; just as it has proved convenient to distribute coal-gas for lighting and heating, and to provide a common system of sewerage, so

it will probably be found convenient to have in all large towns some means of obtaining mechanical power in any desired quantity at a price proportionate to the quantity used, and in a form in which it can be rendered available, either directly or by simple motors requiring but little skilled superintendence.

*Telodynamic Transmission.*—First, then, let me say a few words as to modes of distributing power which it is possible to adopt. In 1850, at Logelbach in Alsace, M. Ferdinand Hirn used a flat steel belt to transmit power directly a distance of eighty metres. Subsequently a wire rope was used on grooved pulleys. This worked so well that a second transmission to a distance of 240 metres was erected. The details of the system were worked out with great care with a view to securing the least cost of construction, the least waste of energy, and the greatest durability of the ropes. So successful did this system of telodynamic transmission prove that within ten years M. Martin Stein, of Mulhouse, had erected 400 transmissions, conveying 4,200 horse-power, and covering a distance of 72,000 metres.

Just at this time a very able and far-seeing manufacturer at Schaffhausen, Herr Moser, had formed a project for reviving the failing industries of the town by utilising part of the water-power of the Rhine: Hirn's system of wire rope transmission rendered this project practicable. The works were commenced in 1863. Three turbines of 750 horse-power were erected on a fall which varies from 12 to 16 feet, created by a weir across the river. From the turbines the power is transmitted by two cables, in one span of 392 feet, across the river. Similar cables distribute the power to factories along the river bank. In 1870 the transmission extended to a distance of 3,400 feet. Power is sold at rates varying from 5*l.* to 6*l.* per horse-power per annum. In 1887 there were twenty-three consumers of power paying a rental of 3,500*l.* per annum for power. The project has been financially successful, and is still working. At Zurich, Freiberg, and Bellegarde there are similar installations, and a large scheme of the same kind has recently been carried out at Gokak in India. Wire-rope transmissions are of great mechanical simplicity, and the loss of power in transmission is exceedingly small. They are extremely suitable for certain cases where a moderate amount of power has to be transmitted a moderate distance, to one or to a few factories. On the other hand, they become cumbrous if the amount of power transmitted exceeds 600 or 1,000 horse-power. The wear of the ropes, which only last a year, has proved greater than was expected, and is a source of considerable expense.

The practical introduction of a system of distributing power by *pressure water* is due to Lord Armstrong. Such a system involves a central pumping station, a series of distributing mains, and suitable working motors. From its first introduction the peculiar advantages of this system for driving intermittently working machines, such as lifts, dock machinery, railway cranes, and hauling gear, became obvious. But, with intermittent working machines, there arose the need of an appliance for storing energy during periods of minimum demand and restoring it in periods of maximum demand. The invention of the accumulator by Lord Armstrong made the system of hydraulic transmission a success, and at the same time fixed its character as a system specially adapted for those cases where intermittent work is required to be done. Lord Armstrong's system of hydraulic distribution by water at a pressure of 700 or 800 lbs. per square inch, with the use of accumulators for equalising the variations of supply and demand, has now been widely adopted. The most extensive scheme of that kind hitherto executed is the important scheme carried out by the Hydraulic Power Company. Over fifty miles of pressure mains have now been laid in the streets of London. The Falcon Wharf pumping station contains four sets of compound pumping engines, each of 200 horse-power. Two additional pumping stations have now been erected, and 1,500 lifts are worked from the pressure mains. The minimum charge for water is 2*s.* per 1,000 gallons. This rate of charge is economical for such machines as lifts, but it would be extravagant for machines working continuously. It would be equivalent to a charge of nearly 50*l.* per horse-power per year of 3,000 working hours, apart from interest and maintenance of machines.

I shall indicate later on that in some cases where local conditions are favour-

able, where there is cheap water-power, and the possibility of constructing high-level storage reservoirs, then hydraulic transmission can be adopted with success for distributing power for ordinary manufacturing purposes. But neither telodynamic transmission nor hydraulic transmission have proved suitable as methods for the general distribution of motive power from central stations. Distribution by steam and distribution by heated water have both been tried in the United States, but not with very remarkable success. Only two other methods are available—distribution by compressed air and distribution by electricity.

For many years *compressed air* has been used to distribute power in tunnelling and mining operations to considerable distances. It is only recently that it has been used as a general method of distributing power to many consumers. In many installations the machinery has been rough and unscientific, and the waste of energy very considerable. It is through experience gained and improvements carried out in the remarkable system now at work in Paris, and known as the Popp system, that the great advantages of compressed air distribution have been proved. The Paris system has very gradually developed. About 1870 a small compressing station was erected to actuate public and private clocks by intermittent pulses of air conveyed along pipes chiefly laid in the sewers. In 1889 about 8,000 clocks were thus driven. Meanwhile the compressed air had also been applied to drive motors for small industries. The demand for power thus supplied grew so rapidly that a second compressing station was built in the Rue de Saint Fargeau. In 1889 steam air compressors of 2,000 horse-power were at work, and additional compressors were under construction. The pressure at that time was five atmospheres, and the largest air mains were 12 inches in diameter. Ingenious and simple rotary machines were used as air motors for small powers, and for larger powers any ordinary steam-engine was converted into an air motor. Professor Kennedy made tests in 1889, which were communicated to this Association. He found that a motor four miles from the compressing station indicated 10 horse-power for 20 indicated horse-power expended at the compressing station, an efficiency of 50 per cent only. There were then 225 motors worked from the air mains.

Since 1889 more extended investigations have been made by Professor Riedler of Berlin, and the chief part of the waste of work has been traced to inefficiency of the air compressors. Compound air compressors of much higher efficiency have now been constructed. The plant at the Saint Fargeau station has been increased to 4,000 horse-power. A new station has been erected on the Quai de la Gare, intended ultimately to contain compressors of 24,000 horse-power. Compressors of 10,000 horse-power are already under construction.

Compressed air transmission, whether or not it is the most economical system, is undoubtedly applicable for the distribution of power on a very large scale and to very considerable distances. There is nothing in any of the appliances which is novel or imperfectly understood. The air is used in the consumer's premises in machinery of well-understood types, and old steam engines can be converted into air motors without difficulty and without alteration of existing transmissive machinery in the factories. Not least important, the air can be measured with accuracy enough for practical purposes by simple meters, and charged for in proportion to the power consumed. Air compressors and air motors are not as efficient as dynamos and electric motors, but in one respect distribution by air and electricity are similar. For distances which are not more than a few miles the loss of energy in transmission is small enough to be insignificant.

There is yet one other mode of power distribution which promises to become the most important of all, and which, in the case of transmission to very great distances, if such transmission becomes necessary, has undoubtedly great advantages over every other method.

About *electrical distribution* of power I shall not venture to say much, partly because I am not an electrical expert, partly because it has been lately pretty fully discussed. In the United States there has been an enormous development of electric tramways, which are essentially cases of electric power distribution. In this country we have the South London and some other railways worked electrically. There are others also on the Continent. But electrical power distribution

to private consumers for industrial purposes has not yet made as much progress as might have been expected. Perhaps electrical engineers have been so busy with problems of electric lighting that they have had no time to settle the corresponding problems of power distribution.

No doubt continuous current distribution presents at the moment the fewest difficulties, or, at any rate, involves the fewest comparatively untried expedients. Several continuous current plants for distributing power are in operation, of which perhaps the most interesting is that at Oyonaz, which was described in Section G last year by Professor G. Forbes. There 300 horse-power obtained by turbines is transmitted 8 kilometres at 1,800 volts. It is then let down by motor transformers to a voltage suitable for lighting and driving motors. A number of small workshops are driven, the power being supplied at a fixed rent.

At the Calumet and Hecla mines on Lake Superior, at the Dalmatia mines in California, and some other places, energy derived from turbines is transmitted distances of a mile or two by continuous electric currents and used in driving mining machinery. and some cases of the use of electrical distribution in mines in this country were mentioned by my predecessor in his address last year.

At Bradford a few electric motors are being worked from the electric lighting mains. The largest of these is of 20 horse-power. The price at which the electricity is supplied is not given, but I believe the cost is high when reckoned for continuous working It would seem that it must be so when the electric current is generated by steam-power.

At Schaffhausen an electric transmission has now been constructed alongside of the wire-rope transmission. The power is derived from two turbines, and is transmitted across the Rhine, a distance of 750 yards, at 624 volts. The current drives a spinning-mill, in which the largest motor is 380 horse-power. The power is sold, I believe, at 3*l* per horse-power of the motors per annum.

Many engineers have now apparently come to the conclusion that alternating currents will be better for power transmission to considerable distances than continuous currents. One interesting alternate current transmission, partly for power, partly for lighting purposes, has been for some time in operation at Genoa.

On the line of the aqueduct bringing water from the Gorzente rivulet three electric stations are being established. The reservoirs are 2,050 feet above Genoa, and as this is a much greater fall than is required for water-supply purposes, part can be used to generate about 1,600 horse-power.

In the first of the power stations erected there are turbines of 450 horse-power driving two dynamos. A second larger station was completed in November. In this there are eight alternate current dynamos of 70 horse-power each. Six alternators are worked in series, transmitting a current of 6,000 volts. The current is transmitted sixteen miles by bare copper wires, 8·5 mm. diameter, placed overhead. The current is used both for lighting and power purposes.

Another method of using alternating currents was adopted in the remarkable experiment at Frankfort last year. In that case energy obtained by turbines at Lauffen was transmitted to Frankfort, a distance of 108 miles, and used for lighting and driving a motor. The current was obtained at low tension, transformed up to a tension of 18,000 to 27,000 volts for transmission, and then transformed down again for distribution. The loss in the conducting wires ranged from 5 horse-power when the turbines worked at 100 horse-power, to 25 horse-power when the turbines worked at 200 horse-power. The efficiency of dynamo, two transformers, and line ranged from 68 to 75 per cent., a remarkably satisfactory result

There can be little doubt that if efficient and durable transformers can be constructed, they do give a considerable advantage to an alternate current system. To an ordinary engineer it appears also that the system of producing current at low tension in the dynamo, and using it at low tension in the motors, permits the construction of dynamos and motors more mechanically unexceptionable than those working at high voltage.

I have spoken of the growth of a demand for power distributed in a convenient form in towns. The power distribution in London, Manchester, Birmingham,

and Liverpool by pressure water, and that by compressed air in Paris, shows how rapidly, when power is available, a demand for it arises. A striking instance may be found in the small town of Geneva.

In 1871, soon after the completion of the earlier system of low-pressure water supply, Col. Turrettini applied to the municipal council to place a pressure engine on the town mains for driving the factory of the Society for Manufacturing Physical Instruments. The plan proved so convenient that nine years after, in 1880, there were in Geneva 111 water-motors supplied from the low pressure mains, using 34,000,000 cubic feet of water annually, and paying to the municipality nearly 2,000l a year. The cost of the power was not low. It was charged at a rate equivalent to from 36l to 48l per horse-power per year of 3,000 working hours. But even the high price did not prevent the use of power so conveniently obtainable.

Since then a high-pressure water service has been established, the water being pumped by turbines in the Rhone. From this high-pressure service power is supplied more cheaply. On the high-pressure system the cost of the power is about 0·7d per horse-power hour, or 8l. per horse-power for 3,000 working hours.

In 1889 the annual income from water sold for power purposes on the low-pressure system was 2,085l., and on the high-pressure system 4,500l. On the high-pressure system the receipts in 1889 were increasing at the rate of 880l. per year.

In 1889 the motive power distributed, on the high-pressure system alone, amounted to 1,500,000 horse-power hours, there being seventy-nine motors of an aggregate working power of 1,279 horses.

In Zurich there is a quite similar system and power, amounting to 9,000,000 horse-power hours in the year, distributed hydraulically to various consumers, who pay a rental of 1,200l. per annum. It will be noted that all this power in Geneva and Zurich is obtained from water which has been pumped, and it is the low cost of the water-power which does the pumping which makes this possible.

But, further, in both Geneva and Zurich the whole of the dynamos supplying electric light are also driven by turbines using pumped water. The convenience of this arises in this way. The fall obtainable in the river in both cases is a small one, and varies. Large turbines are required, and these cannot work at a constant speed. Further, it is expensive to use these large low-pressure turbines to drive directly dynamos which only work with a considerable load for a short portion of the day. The low-pressure turbines in the river are therefore used to pump water to a high-level reservoir, and they work with a constant load all the twenty-four hours.

From the high-level reservoir water is taken as power is required to drive the dynamos, and the turbines driving the dynamos are small high-pressure turbines, working always on a constant fall at a regular speed, and easily adjusted by a governor to a varying load. The system seems a roundabout one, but it is perfectly rational, effective, and economical.

Few persons can have seen Niagara Falls without reflecting on the enormous energy which is there continuously expended, and for any useful purpose wasted. The exceptional constancy of the volume of flow, the invariability of the levels, the depth of the plunge over the escarpment, the solid character of the rocks, all mark out Niagara as an ideally perfect water-power station, while, on the other hand, the remarkable facilities of transport, both by steam navigation on the lakes and by four systems of railway, afford commercial advantages of the highest importance. From a catchment basin of 240,000 square miles, an area greater than that of France, a volume of water amounting to 265,000 cubic feet per second descends from Lake Erie to Lake Ontario, a vertical distance of 326 feet, in 37½ miles.

Supposing the whole stream could be utilised, it would supply 7,000,000 horse-power. This is more than double the total steam and water power at present employed in manufacturing industry in the United States.

Immediately below the Falls the river bends at right angles, and flows through a narrow gorge. The town of Niagara Falls on the American side occupies the table-land in this angle

The earliest traders who settled near the Falls erected stream mills in the

Upper River in 1725 for preparing timber. Later, the Porter family erected factories on the islands in the rapids above the falls. It was not, however, till about thirty years ago that any systematic attempt was made to utilise part of the water-power of the Falls. Then a canal was constructed from Port Day, about three-quarters of a mile above the Falls, to a forebay or head-race along the cliff overlooking the lower river. In 1874 the Cataract Mill was established, taking power from this canal, and other mills were gradually erected till about 6,000 horse-power was utilised. These mills have been exceedingly prosperous, but since the growth of a feeling against the disfigurement of the Falls it has become impossible to extend works of the same kind.

The idea of a method of utilising the Falls, capable of greater development, and free from the objections to the hydraulic canal with mills discharging tail water on the face of the cliff, is due to the late Mr. Thomas Evershed, Division Engineer of the New York State Canals. He proposed to construct head-race canals on unoccupied land some two miles above the Falls. From these the water was to fall through vertical turbine pits into tail-race tunnels, converging into a great main tunnel, discharging into the lower river. Apart from an inappreciable diminution of the volume of flow over the Falls, this plan avoids any disfigurement of the scenery near the Falls, and permits a head of nearly 200 feet to be made available. It is, however, essential to such a plan that work should be undertaken on a very large scale. In 1886 the Niagara Falls Company was incorporated, and obtained options over a considerable area of land, extending from Port Day for two miles along the Niagara River. In 1889 the Cataract Construction Company was formed to mature and carry out the constructional works required.

The present plans contemplate the utilisation of 100,000 effective horse-power. The principal work of construction is a great tunnel 7,250 feet long, which is to form a tail-race to the turbines, starting from land belonging to the Company and discharging into the lower river. The tunnel is 19 feet by 21 feet, or 386 square feet in area, inside a brickwork lining 16 inches thick.

The base of the tunnel is 205 feet below the sill of the head gate, and permits a fall of 140 to be rendered available at the turbines The brickwork of the tunnel is lined for 200 feet from the mouth with cast-iron plates.

The tunnel has been excavated with remarkable rapidity with the aid of drills worked by compressed air.

The main head-race, about 200 feet wide, will run for about 5,000 feet parallel with the river, having entrances from the river at both ends. Near the lower reach the Soo Paper Company is already arranging to utilise 6,000 horse-power, discharging the water from the turbines through a lateral tunnel into the main tunnel. Near this lower reach will also be placed two principal power stations, from which power will be distributed, either electrically or otherwise in ways not yet fully determined. The first turbines to be erected in these power stations will be twin turbines of the outward flow type of 5,000 effective horse-power. These turbines have a vertical shaft for driving dynamos or other machinery placed above ground.

According to Mr. Evershed's original plans, it was intended to distribute water by surface canals to different power users, each of whom would sink his own turbine pits, connected below by lateral tunnels to the main discharge tunnel. Some of the power at Niagara will undoubtedly be used in this way, and in the case of industries requiring a large amount of power it will be economical to purchase a site and water rights.

Such a plan is, however, not adapted to smaller factories. Obviously for them it would be more economical to develop the power in one or more central stations by turbines of large size under common management. Further, once given the means of distributing power instead of water, an important extension of the project becomes possible.

Besides supplying power to industries which may locate themselves at Niagara, the power may be transmitted to the existing factories in Buffalo and Tonawanda.

Arrangements are already proceeding to transmit 3,000 horse-power to Buffalo, a distance of 18 miles, to work an electric lighting station.

In 1890, Mr. Adams, the President of the Niagara Construction Company, visited Europe to examine systems of power distribution. It was in consequence of this visit that the important modification of the plans of the company involved in the substitution, to a large extent, of a system of power distribution for a system of water distribution came to be adopted. The American engineers were anxious to obtain the best European advice as to the methods best suited to the local conditions  A commission was formed, consisting of Lord Kelvin, Dr. Coleman Sellers, Professor Mascart, and Colonel Turrettini, and an invitation was given to engineers and engineering firms in Europe and America to send in competitive projects for the utilisation of the power at Niagara and its distribution to different consumers at Niagara and in Buffalo by electrical or other means. Many of the plans sent in were worked out with great care and completeness.  As to the hydraulic part of the projects there was some approach to general consent as to the arrangements to be adopted, but as to the methods of distributing the power there was an extraordinary diversity.

Generally the Commission reported in favour of electrical distribution, with perhaps a partial use of compressed air as an auxiliary method.

Generally also they reported in favour of methods of distribution by continuous currents in preference to alternating currents. Since the date at which the Commission reported, the Frankfort-Lauffen experiment has been made, and in the opinion of some electrical engineers a distinct advance has been achieved in the use of alternating currents at high potential.

The Company has not yet decided to adopt any plan for the central stations except in a tentative way. One or more turbines of 5,000 horse-power are to be erected, and probably at first this power will be distributed to Buffalo by an alternating current system.

The cost of a steam horse-power at Buffalo is reckoned at $35 per annum. I believe the Company will be able to deliver power at from $10 for large amounts and a greater price for small amounts, this price being reckoned for twenty-four hour days.

The new industry of electric lighting has made necessary the provision of large amounts of motive power. Electric traction similarly depends on the supply of motive power. New chemical and metallurgical processes are being introduced which entirely depend for their commercial success on the supply of motive power at a low price.

Niagara is likely to become not only a seat of large manufacturing operations of familiar types, but also the home of important new industries.

# ADDRESS

TO THE

# ANTHROPOLOGICAL SECTION

OF THE

# BRITISH ASSOCIATION,

BY

ALEXANDER MACALISTER, M.D., F.R.S., Professor of
Anatomy in the University of Cambridge,

PRESIDENT OF THE SECTION.

ON an irregular and unfenced patch of waste land, situated on the outskirts of a small town in which I spent part of my boyhood, there stood a notice-board bearing the inscription 'A Free Coup,' which, when translated into the language of the Southron, conveyed the intimation, 'rubbish may be shot here.' This place, with its ragged mounds of unconsidered trifles, the refuse of the surrounding households, was the favourite playground of the children of the neighbourhood, who found a treasury of toys in the broken tiles and oyster-shells, the crockery and cabbage-stalks, which were liberally scattered around. Many a make-believe house and road, and even village, was constructed by these mimic builders out of this varied material, which their busy little feet had trodden down until its undulated surface assumed a fairly coherent consistence.

Passing by this place ten years later I found that its aspect had changed; terraces of small houses had sprung up, mushroom-like, on the unsavoury foundation of heterogeneous refuse. Still more recently I notice that these in their turn have been swept away, and now a large factory, wherein some of the most ingenious productions of human skill are constructed, occupies the site of the original waste.

This commonplace history is, in a sense, a parable in which is set forth the past, present, and possible future of that accumulation of lore in reference to humanity to which is given the name Anthropology, and for the study of which this Section of our Association is set apart. At first nothing better than a heap of heterogeneous facts and fancies, the leavings of the historian, of the adventurer, of the missionary, it has been for long, and alas is still, the favourite playground of *dilettanti* of various degrees of seriousness. But upon this foundation there is rapidly rising a more comely superstructure, fairer to see than the original chaos, but still bearing marks of transitoriness and imperfection, and I dare hazard the prediction that this is destined in the course of time to give place to the more solid fabric of a real Science of Anthropology.

We cannot yet claim that our subject is a real science in the sense in which that name is applied to those branches of knowledge, founded upon ascertained laws, which form the subjects of most of our sister Sections; but we can justify our separate existence, in that we are honestly endeavouring to lay a definite and stable foundation, upon which in time to come a scientific Anthropology may be based.

The materials with which we have to do are fully as varied as were those in my illustration, for we as anthropologists take for our motto the sentiment of Chremes, so often quoted in this Section, *humani nihil a nobis alienum putamus*, and they are too often fully as fragmentary. The bones, weapons, and pottery

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which form our only sources of knowledge concerning prehistoric races of men, generally come to us as much altered from their original forms as are the rusty polyhedra which once were the receptacles for biscuits or sardines. The traditions, customs, and scraps of folk-lore which are treasures to the constructive anthropologist, are usually discovered as empty shells, in form as much altered from their original conditions as are those smooth fragments of hollow white cylinders which once held the delicate products of the factory of Keiller or Cairns.

I have said that Anthropology has not yet made good its title to be ranked as an independent science. This is indicated by the difficulty of framing a definition at the same time comprehensive and distinctive. Mr. Galton characterises it as the study of what men are in body and mind, how they came to be what they are, and whither the race is tending; General Pitt-Rivers, as the science which ascertains the true causes for all the phenomena of human life. I shall not try to improve upon these definitions, although they both are manifestly defective. On the one side our subject is a branch of biology, but we are more than biologists compiling a monograph on the natural history of our species, as M. de Quatrefages would have it. Many of the problems with which we deal are common to us and to psychologists; others are common to us and to students of history, of sociology, of philology, and of religion; and, in addition, we have to treat of a large number of other matters æsthetic, artistic, and technical, which it is difficult to range under any subordinate category.

In view of the encyclopædic range of knowledge necessary for the equipment of an accomplished anthropologist, it is little wonder that we should be, as we indeed are, little better than smatterers. Its many-sided affinities, its want of definite limitation, and the recent date of its admission to the position of an independent branch of knowledge, have hitherto caused Anthropology to fare badly in our Universities. In this respect, however, we are improving, and now in the two great English Universities there are departments for the study of the natural history of man and of his works.

Out of the great assemblage of topics which come within our sphere, I can only select a few which seem at present to demand special consideration. The annual growth of our knowledge is chiefly in matters of detail which are dull to chronicle, and the past year has not been fertile in discoveries bearing on those great questions which are of popular interest.

On the subject of the antiquity of man there are no fresh discoveries of serious importance to record. My esteemed predecessor at the Leeds meeting two years ago, after reviewing the evidence as to the earliest traces of humanity, concluded his survey with the judgment, 'On the whole, therefore, it appears to me that the present verdict as to tertiary man must be in the form of " Not Proven."' Subsequent research has not contributed any new facts which lead us to modify that finding. The most remarkable of the recent discoveries under this head is that of the rude implements of the Kentish chalk-plateau described by Professor Prestwich, but while these are evidently of archaic types, it must be admitted that there is even yet room for difference of opinion as to their exact geological age.

Neither has the past year's record shed new light on the darkness which enshrouds the origin of man. What the future may have in store for us in the way of discovery we cannot forecast; at present we have nothing but hypothesis, and we must still wait for further knowledge with the calmness of philosophic expectancy.

I may, however, in this connexion refer to the singularly interesting observations of Dr. Louis Robinson on the prehensile power of the hands of children at birth, and to the graphic pictures with which he has illustrated his paper. Dr. Robinson has drawn, from the study of the one end of life, the same conclusion which Mr. Robert Louis Stevenson deduced from the study of his grandfather, that there still survive in the human structure and habit traces of our probably arboreal ancestry.

Turning from these unsolved riddles of the past to the survey of mankind as it appears to us in the present, we are confronted in that wide range of outlook with many problems well-nigh as difficult and obscure.

Mankind, whenever and however it may have originated, appears to us at present as an assemblage of tribes, each not necessarily homogeneous, as their component elements may be derived from diverse genealogical lines of descent. It is much to be regretted that there is not in our literature a more definite nomenclature for these divisions of mankind, and that such words as *race, people, nationality, tribe,* and *type* are often used indiscriminately as though they were synonyms.

In the great mass of knowledge with which we deal there are several collateral series of facts, the terminologies of which should be discriminated. In the first place there are those ethnic conditions existing now, or at any other point in time, whereby the individuals of mankind are grouped into categories of different comprehension, as *clans* or families, as *tribes* or groups of allied clans, and as *nations,* the inhabitants of restricted areas under one political organisation. This side of our subject constitutes Ethnology.

In the second place, the individuals of mankind may be regarded as the descendants of a limited number of original parents, and consequently each person has his place on the genealogical tree of humanity. As the successive branches became in their dispersion subjected to the influences of diverse environments, they have eventually differentiated in characteristics. To each of these subdivisions of the phylum thus differentiated the name *race* may appropriately be restricted, and the sum of the peculiarities of each race may be termed *race-characters* This is the phylogenetic side of Anthropology, and its nomenclature should be kept clearly separate from that of the ethnological side. The great and growing literature of Anthropology consists largely of the records of attempts to discover and formulate these distinctive race-characters. *Race* and *tribe* may be terms of equal extension, but the standpoint from which these categories are viewed is essentially different in the two cases.

There is yet a third series of names in common use in Descriptive Anthropology. The languages in use among men are unfortunately numerous, and as the component individuals in each community usually speak a common language, the mistake is often made of confounding the tribal name with that of the tribal language. Sometimes these categories are co-extensive; but it is not always so, for it is a matter of history that communities have been led to adopt new languages from considerations quite independent of phylogenetic or ethnic conditions These linguistic terms should not be confounded with the names in either of the other series, for, as my learned predecessor once said in a presidential address, it is as absurd to speak of an Aryan skull as it would be to say that a family spoke a brachycephalic language.

In the one clan there may be, by intermarriage, the representatives of different races ; in the one nation there may be dissimilar tribes, each derived by composite lines of ancestry from divergent phyla, yet all speaking the same language.

We have an excellent illustration of the confusion resulting from this disregard of precision in the case of the word *Celtic,* a term which has sometimes been employed as an ethnic, sometimes as a phylogenetic, and sometimes as a linguistic species. In the last-named sense, that to which I believe the use of the name should be restricted, it is the appropriate designation of a group of cognate languages spoken by peoples whose physical characters show that they are not the descendants of one common phylum in the near past There are fair-haired, long-headed families in Scotland and Ireland, fair, broad-headed Bretons ; dark-haired, round-headed Welshmen ; and dark-haired, long-headed people in the outer Hebrides, McLeans, ' Sancho Panza type '—men obviously of different races, who differ not only in colour, stature, and skull-form, but whose traditions also point to a composite descent, and yet all originally speaking a Celtic tongue. The use of the word *Celtic* as if it were the name of a phylogenetic species has naturally led to hopeless confusion in the attempts to formulate race-characters for the Celtic skull—confusions of a kind which tend to bring physical anthropology into discredit. Thus Retzius characterises the Celtic crania as being dolichocephalic, and compares them with those of the modern Scandinavians. Sir Daniel Wilson considers the true Celtic type of skull as intermediate between the dolichocephali

and the brachycephali; and Topinard figures as the typical Celtic skull that of an
Auvergnat, extremely brachycephalic, with an index of 85!

Our traditional history tells that we, the Celtic-speaking races of Britain, are
not of one common ancestry, but are the descendants of two distinct series of
immigrants, a British and a Gaelic. Whatever may have been the origin of the
former, we know that the latter are not homogeneous, but are the mixed
descendants of the several Fomorian, Nemedian, Firbolg, Tuatha de Danaan, and
Milesian immigrations, with which has been combined in later times a strong
admixture of Scandinavian blood. It is now scarcely possible to ascertain to
which of these component strains in our ancestry we owe the Celtic tongue which
overmastered and supplanted the languages of the other tribes, but it is strictly in
accordance with what we know of the history of mankind that this change should
have taken place. We have instances in modern times of the adoption by conquered
tribes of the language of a dominant invading people. For example, Mr Hale has
lately told us that the speech of the Hûpas has superseded the languages of those
Californian Indians whom they have subdued. In like manner, nearer home,
the English language is slowly but surely supplanting the Celtic tongues them-
selves.

We may here parenthetically note that what has been observed in the case of
language has also taken place in reference to ritual and custom. Observances
which have a history and a meaning for one race have, in not a few instances, been
adopted by or imposed upon other races to whom they have no such significance,
and who in incorporating them give to them a new local colour  These pseudo-
morphs of the earlier cultures are among the most perplexing of the problems
which the student of comparative religion or folk-lore has to resolve.

But we want more than a perfect nomenclature to bring Anthropology into range
with the true sciences. We need a broader basis of ascertained fact for inductive
reasoning in almost all parts of our subject, we want men trained in exact method
who will work patiently at the accumulation, verification, and sorting of facts, and
who will not prematurely rush into theory. We have had enough of the untrained
writer of papers, the jerry-builder of unfounded hypotheses whose ruins cumber our
field of work.

The present position of our subject is critical and peculiar; while on the one
hand the facilities for anthropological research are daily growing greater, yet in
some directions the material is diminishing in quantity and accessibility. We are
accumulating in our museums treasures both of the structure and the works of man,
classified according to his distribution in time and space; but at the same time
some of the most interesting tribes have vanished, and others are rapidly disap-
pearing or becoming fused with their neighbours. As these pass out of existence
we, with them, have lost their thoughts, their tongues, and their traditions; for
even when they survive, blended with other races, that which was a religion has
become a fragmentary superstition, then a nursery tale or a child's game, and is
destined finally to be buried in oblivion. The unifying influences of commerce,
aided by steam and electricity, are effectually effacing the landmarks between
people and people, so that if we are to preserve in a form fit for future use the
shreds which remain of the myths, folk-lore, and linguistic usages of many of the
tribes of humanity, we must be up and doing without delay. It is on this account
that systematic research such as that which Mr. Risley has advocated with regard
to the different races of India is of such pressing and urgent importance. It is for
this reason likewise that we hail with pleasure the gathering of folk-lore while yet
it survives, and welcome such societies for the purpose as the Folk-lore Congress
recently inaugurated.

I have said that in the department of Physical Anthropology our facilities for
research are increasing  The newly-founded anthropometric laboratories are
beginning to bring forth results in the form of carefully compiled statistical tables,
embodying the fruits of accurate observations, which are useful as far as they go.
Were these extended in their scope the same machinery might easily gather parti-
culars as to the physical characters of the inhabitants of different districts, which
would enable the anthropologist to complete in a systematic manner the work

which Dr. Beddoe has so well begun. I would commend this work to the consideration of the provincial university colleges, especially those in outlying districts.

Of all the parts of the human frame, the skull is that upon which anthropologists have in the past expended the most of their time and thought. We have now, in Great Britain alone, at least four collections of skulls, each of which includes more than a thousand specimens, and in the other great national and university museums of Europe there are large collections available for study and comparison.

Despite all the labour that has been bestowed on the subject, craniometric literature is at present as unsatisfactory as it is dull. Hitherto observations have been concentrated on cranial measurements as methods for the discrimination of the skulls of different races. Scores of lines, arcs, chords, and indexes have been devised for this purpose, and the diagnosis of skulls has been attempted by a process as mechanical as that whereby we identify certain issues of postage-stamps by counting the nicks in the margin. But there is underlying all these no unifying hypothesis, so that when we, in our sesquipedalian jargon, describe an Australian skull as microcephalic, phænozygous, tapeino-dolichocephalic, prognathic, platyrhine, hypselopalatine, leptostaphyline, dolichuranic, chamæprosopic, and microseme, we are no nearer to the formulation of any philosophic concept of the general principles which have led to the assumption of these characters by the cranium in question, and we are forced to echo the apostrophe of Von Torok, ' Vanity, thy name is Craniology.'

It was perhaps needful in the early days of the subject that it should pass through the merely descriptive stage ; but the time has come when we should seek for something better, when we should regard the skull not as a whole complete in itself, nor as a crystalline geometrical solid, nor as an invariable structure, but as a marvellously plastic part of the human frame, whose form depends on the co-operation of influences, the respective shares of which in moulding the head are capable of qualitative if not of quantitative analysis. Could measurements be devised which would indicate the nature and amounts of these several influences, then, indeed, would craniometry pass from its present empirical condition and become a genuine scientific method. We are yet far from the prospect of such an ideal system, and all practical men will realise the immense, but not insuperable, difficulties in the way of its formulation.

In illustration of the profound complexity of the problem which the craniologist has to face, I would ask your indulgence while I set out a few details to show the several factors whose influence should be numerically indicated by such a mode of measurement.

The parts composing the skull may be resolved into four sets : there is, first, the brain-case ; secondly, the parts which subserve mastication and the preparation of the food for digestion ; thirdly, the cavities containing the organs of the senses of hearing, sight, and smell ; and, fourthly, those connected with the production of articulate speech. If our measurements are to mean anything, they should give us a series of definite numbers indicating the forms, modifications, and relative size of these parts, and their settings with regard to each other and to the rest of the body.

To take the last point first, it needs but a small consideration to show that the parts of the skull are arranged above and below a certain horizontal plane, which is definite (although not easily ascertained) in every skull, human or animal. This is the plane of vision. The familiar lines of Ovid—

> Pronaque cum spectent animalia cetera terram,
> Os homini sublime dedit ; cœlumque tueri
> Jussit, et erectos ad sidera tollere vultus—

are anatomically untrue, for the normal quadruped and man alike, in their most natural position, have their axes of vision directed to the horizon. Systems of measurement based upon any plane other than this are essentially artificial. There are at the outset difficulties in marking the plane accurately on the skull, and it is to be deplored that the anthropologists of different nations should

have allowed themselves to be affected by extraneous influences, which have hindered their unanimous agreement upon some one definite horizontal plane in craniometry.

The Frankfort plane drawn through the upper margins of the auditory foramina and the lowest points of the orbital borders has the advantage of being easily traced and differs so little from the plane of vision that we may without substantial error adopt it.

The largest part of the skull is that which is at once the receptacle and the protector of the brain, a part which, when unmodified by external pressure, premature synostosis, or other adventitious conditions, owes its form to that of the cerebral hemispheres which it contains. Speaking in this city of George and Andrew Combe, I need not do more than indicate in this matter that observation and experiment have established on a firm basis certain fundamental points regarding the growth of the brain. The study of its development shows that the convolutioning of the cerebral hemisphere is primarily due to the connexion, and different rate of growth, of the superficial layer of cells with the underlying layers of white nerve fibres; and that so far from the shape being seriously modified by the constraining influence of the surrounding embryonic skull, the form of the soft membranous brain-case is primarily moulded upon the brain within it, whose shape it may however be, to some extent, a secondary agent in modifying in later growth. We have also learned that, although in another sense from that of the crude phrenology of Aristotle, Porta, or Gall, the cerebrum is not a single organ acting as a functional unit, but consists of parts, each of which has its specific province ; that the increase in the number of cells in any area is correlated with an increase in the size and complexity of pattern of the convolutions of that area ; and that this in turn influences the shape of the enclosing shell of membrane and subsequently of bone.

The anatomist and the physiologist have worked hand in hand in the delimitation of these several functional areas, and pathology and surgery have confirmed what experimental physiology has taught. The topography of each part of the cerebrum, so important to the operating surgeon, should be pressed into the service of the anthropologist, whose measurements of the brain-case should have definite relation to these several areas. In the discussion which is to take place on this subject, I hope that some such relationships will be taken account of. This is not the place to work out in detail how this may be done; I only desire to emphasise the fundamental principle of the method.

The second factor which determines the shape of the individual skull is the size of the teeth. That these differ among different races is a matter of common observation ; thus the average area of the crowns of the upper-jaw teeth in the male Australian is 1,536 sq. mm., while in the average Englishman it is only 1,286 sq. mm., less than 84 per cent. of that size.[1]

It is easy to understand how natural selection will tend to increase the size of the teeth among those races whose modes of feeding are not aided by the cook or the cutler; and how, on the other hand, the progress of civilised habits, assisted by the craft of the dentist, interferes with the action of selection in this matter among the more cultured races.

For larger teeth a more extensive alveolar arch of implantation is necessary ; and as the two jaws are commensurately developed, the lower jaw of the macrodontal races exceeds that of meso- or microdontal races in weight. Thus that of a male Australian exceeds that of the average Englishman in the proportion of 100 : 91.

To work this heavier jaw more powerful muscles are needed. In the average well-developed Englishman with perfect teeth the weight of the fleshy portion of the great jaw-muscles, masseters and temporals, is 60 grammes, while the weight of those as ascertained in two Australians was 74 grammes.

Correlated with this greater musculature a sharper definition of the areas for the attachments of the jaw-muscles is required. The muscular fascicles are approxi-

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[1] These and the succeeding averages are from my own measurements, taken from never less than ten individual cases.

mately of uniform size in both microdonts and macrodonts, as the range of motion of the jaw differs little in different races ; but when the skull is smaller on account of the smaller size of the brain which it contains, the temporal crest ascends higher on the side-wall. In the average Englishman the temporal crests at their points of greatest approximation anteriorly across the brows are 112 mm. apart, but in the Australian they are only separated by 103 mm.: the interstephanic distances in these two are respectively 132 and 114 mm.

The more powerful stroke of the mandibular teeth upon the anvil of the upper-jaw teeth in macrodonts renders necessary a proportionally stronger construction of the bases of support for the upper alveolar arch. In any skull this arch requires to be solidly connected to the wall of the brain-case to which the shock of the impact is ultimately transmitted, and in order to protect from pressure the delicate intervening organs of sight and smell, the connexion is accomplished by the reversed arches of the infraorbital margins with their piers, malar and maxillary, founded on the frontal angular processes. These foundations are tied together by the strong supraorbital ridge, so that the whole orbital edge is a ring, made up of the hardest and toughest bone in the skeleton.

A twofold modification of this arrangement is required in the macrodont skull. The bony circum-orbital ring becomes stronger, especially along its lateral piers ; and also as the alveolar arch is longer, and consequently projects farther forward, its basis of support must be extended to meet and bear the malar and maxillary piers. But macrodonts are often microcephalic, and therefore the frontal region of the skull must be adjusted to form a foundation for this arch. In the average English male skull, held with its visual axes horizontal, a perpendicular dropped from the anterior-surface of the fronto-nasal suture will cut the plane of the alveolar arch between the premolar teeth or through the first premolar. In an Australian skull the perpendicular cuts the horizontal plane at the anterior border of the first molar teeth.

It is obvious, therefore, that to ensure firmness, the piers of the arches must be obliquely set ; hence the jaw is prognathous, but it is also needful that the supra-orbital arcade should be advanced to meet and bear these piers, as the mandibular stroke is always vertical.

But the inner layer of the skull is moulded on the small frontal lobes of the brain, so this forward extension must affect only the much thicker and tougher outer table of the skull, which, at the period of the second dentition, here separates from the inner table, the interval becoming lined by an extension of the mucosa of the anterior ethmoidal cell. In this way an air space, the frontal sinus, is formed, whose development is thus directly correlated to the two factors of brain development and size of the teeth. If the frontal lobes are narrow in a macrodont skull, then the foundations of the outer or malar piers of the orbital arch must be extended outwards as well as forwards, the external angular process becoming a prominent abutment at the end of a strong low-browed supraorbital arch, whose overhanging edge gives to the orbital aperture a diminished vertical height.

The crania of the two most macrodont races of mankind, Australian and African, differ in the relation of the jaw to the frontal bone. In the microcephalic Australian, the maxillæ are founded upon the under side of the shelf-like projection of the outer table of the frontal, which juts out as a buttress to bear it. On the other hand the nasal processes of the mesocephalic negro ascend with greater obliquity to abut on the frontal, and have, by their convergence, crushed the nasal bones together, and caused their coalescence and diminution.

The crania of the two most microcephalic races present distinctive features of contrast along the same lines. The Bushman's skull is usually orthognathous, with a straight forehead and a shallow frontonasal recess, while the Australian skull is prognathous with heavy overhanging brows. These conditions are correlated to the mesodontism of the Bushman and the macrodontism of the Australian respectively.

In the course of the examination of the relations of brain development to skull growth, some interesting collateral points are elicited. The frontal bone grows from lateral symmetrical centres, which medially coalesce, union taking place

usually between the second and sixth years of age. It has been noticed by anthropologists that metopism, as the anomalous non-union of the halves of this bone has been termed, is rare among microcephalic races, occurring only in about 1 per cent. among Australian skulls. Increased growth of the frontal lobes as the physical accompaniment of increased intellectual activity interposes an obstacle to the easy closure of this median suture, and so in such races as the ancient Egyptian, with a broader forehead, metopism becomes commoner, rising to 7 per cent. In modern civilised races the percentage ranges from 5 to 10. In following out the details of this enumeration, I have spoken as if the micro-dontal condition had been the primary one, whereas all the available evidence leads to show that the contrary was the case. The characters of all the early crania, Neanderthal, Engis, and Cromagnon, are those of macrodonts. The progress has been from the macrodont to the microdont, as it probably was from the microcephalic to the macrocephalic.

The effects of the variations in size of the teeth are numerous and far-reaching. The fluctuation in the weight of the jaw depending on these variations has an important influence on the centre of gravity of the head, and affects the set of the skull on the vertebral column. This leads to a consequent change in the axes of the occipital condyles, and it is one of the factors which determines the size of the neck-muscles, and therefore the degree of prominence of the nuchal crests and mastoid process.

As the teeth and alveolar arches constitute a part of the apparatus for articulate speech, so these varieties in dental development are not without considerable influence on the nature of the sound produced. The necessarily larger alveolar arch of the macrodont is hypseloid or elliptical, more especially when it has to be supported on a narrow frontal region, and this is associated with a more extensive and flatter palatine surface. This, in turn, alters the shape of the mouth cavity and is associated with a wide flat tongue, whose shape participates in the change of form of the cavity of which it is the floor. The musculature of the tongue varies with its shape, and its motions, upon which articular speech depends, become correspondingly modified. For example, the production of the sharp sibilant S requires the approximation of the raised flexible edge of the tongue to the inner margins of the teeth behind the canines, and to the palatine margin close behind the roots of the canine and lateral incisor teeth. This closes the vocal tube laterally, and leaves a small lacuna about five mm. wide anteriorly, through which the vibrating current of air is forced. A narrow strip of the palate behind the medial halves of the median incisors bounds this lacuna above, and the slightly concave raised tongue-tip limits it below.

With the macrodont alveolar arch, and the correspondingly modified tongue, sibilation is a difficult feat to accomplish, and hence the sibilant sounds are practically unknown in all the Australian dialects.

It is worthy of note that the five sets of muscular fibres, whose function it is to close laterally the flask-like air-space between the tongue and the palate, are much less distinct and smaller in the tongues of the Australians which I have examined than in the tongues of ordinary Europeans.

There is a wide field open to the anatomical anthropologist in this investigation of the physical basis of dialect. It is one which requires minute and careful work, but it will repay any student who can obtain the material, and who takes time and opportunity to follow it out. The anatomical side of phonology is yet an imperfectly known subject, if one may judge by the crudeness of the descriptions of the mechanism of the several sounds to be found even in the most recent text-books. As a preliminary step in this direction we are in urgent need of an appropriate nomenclature and an accurate description of the muscular fibres of the tongue. The importance of such a work can be estimated when we remember that there is not one of the 260 possible consonantal sounds known to the phonologist which is not capable of expression in terms of lingual, labial, and palatine musculature.

The acquisition of articulate speech became possible to man only when his alveolar arch and palatine area became shortened and widened, and when his

tongue, by its accommodation to the modified mouth, became shorter and more horizontally flattened, and the higher refinements of pronunciation depend for their production upon more extensive modifications in the same directions.

I can only allude now very briefly to the effects of the third set of factors, the sizes of the sense organs, on the conformation of the skull. We have already noted that the shape and the size of the orbital opening depend on the jaw as much as on the eye. A careful set of measurements has convinced me that the relative or absolute capacity of the orbital cavity is of very little significance as a characteristic of race. The microseme Australian orbit and the megaseme Kanaka are practically of the same capacity, and the eyeballs of the two Australians that I have had the opportunity of examining are a little larger than those of the average of mesoseme Englishmen.

The nasal fossæ are more variable in size than the orbits, but the superficial area of their lining and their capacity are harder to measure, and bear no constant proportion to the size of their apertures, because it is impossible without destroying the skull to shut off the large air sinuses from the nasal fossæ proper for purposes of measurement. Thus the most leptorhine of races, the Esquimaux, with an average nasal index of 43·7 has a nasal capacity of 55 c.c.m., equal to that of the platyrhine Australian, whose average is 54·5, and both exceed the capacity of the leptorhine English, which average about 50 c.c m. There is an intimate and easily proved connexion between dental size and the extent of the nasal floor and of the pyriform aperture.

These are but a few of the points which a scientific craniometry should take into consideration. There are many others to which I cannot now refer, but which will naturally occur to the thoughtful anatomist.

In this rapid review of the physical side of our subject the study of these race-characters naturally suggests the vexed question as to the hereditary transmission of acquired peculiarities This is too large a controversy for us now to engage in, but in the special instances before us there are grounds for the presumption that these characters of microdontism and megacephaly have been acquired at some stage in the ancestral history of humanity, and that they are respectively correlated, with diminution of use in the one case, and increase of activity in the other. It is a matter of observation that these qualities have become hereditary, and the point at issue is not the fact, but the mechanism, of the transmission. We know that use or disuse affects the development of structure in the individual, and it is hard to believe that the persistent disuse of a part through successive generations does not exercise a cumulative influence on its ultimate condition.

There is a statement in reference to one of these characters which has gained an entrance into the text-books, to the effect that the human alveolar arch is shortening, and that the last molar tooth is being crowded out of existence. I have examined 400 crania of men of the long, and round-barrow races, Romano-British and early Saxon, and have not found among all these a single instance of absence of the third molar or of overcrowded teeth. On the other hand, out of 200 ancient Egyptian skulls 9 per cent. showed displacement or disease, and 1½ per cent. show the want of one molar tooth. Out of 200 modern English skulls there was no third molar tooth in 1 per cent. So far this seems to confirm the current opinion.

Yet the whole history of the organism bears testimony to the marvellous persistence of parts in spite of contumely and disuse. Take, for example, the present position of the little toe in man. We know not the condition of this digit in prehistoric man, and have but little information as to its state among savage tribes at the present day, but we do know that in civilised peoples, whose feet are from infancy subjected to conditions of restraint, it is an imperfect organ—

Of every function shorn
Except to act as basis for a corn.

In 1 per cent. of adults the second and third joints have ankylosed, in 3 per cent. the joint between them is rudimentary, with scarcely a trace of a cavity, in

20 per cent. of feet the organ has lost one or more of its normal complement of muscles. But though shorn of some of its elements, and with others as mere shreds, the toe persists, and he would be a bold prophet who would venture to forecast how many generations of booted ancestry would suffice to eliminate it from the organisation of the normal man.

Nevertheless, although it is difficult to demonstrate, in the present imperfect state of knowledge, the method whereby race-characters have originated, I think that the most of our anthropologists at least covertly adopt the philosophy of the ancient proverb, ʻThe fathers have eaten sour grapes and the children's teeth are set on edge.'

But there are other branches ⸲of Anthropology of far greater interest than these simple problems upon which we have tarried so long. The study of man's intellectual nature is equally a part of our subject, and the outcomes of that nature are to be traced in the tripartite record of human progress which we call the history of culture. It is ours to trace the progress of man's inventions and their fruits in language and the arts, the direct products of the human mind. It is also ours to follow the history of man's discovery of those secrets of nature to the unfolding of which we give the name of science. The task is also ours to inquire into that largest and most important of all sections of the history of culture, which deals with the relation of human life to the unseen world, and to disentangle out of the complex network of religion, mythology, and ritual those elements which are real truths, either discovered by the exercise of man's reason, or learned by him in ways whereof science takes no account, from those adventitious and invented products of human fear and fancy which obscure the view of the central realities. In this country it matters less that our time forbids us to wander in these fascinating fields wherein the anthropologist loves to linger, as the munificent benefaction of Lord Gifford has ensured that there shall be an annual fourfold presentation of the subject before the students of our Scottish universities. There is no fear that interest in these questions will flag for want of diversity in the method of treatment or of varieties in the standpoints of the successive Gifford lecturers.

From the ground of our present knowledge we can but faintly forecast the future of Anthropology, when its range is extended by further research, and when it is purged of fancies, false analogies, and imperfect observations. It may be that there is in store for us a clearer view of the past history of man, of the place and time of his first appearance, of his primitive character, and of his progress. But has this knowledge, interesting as it may be for its own sake, any bearing on the future of mankind? Hitherto growth in knowledge has not been accompanied with a commensurate increase in the sum of human happiness, but this is probably due to the imperfection which characterises even our most advanced attainments. For example, while the medical and sanitary sciences, by their progress, are diminishing the dangers which beset humanity, they have also been the means of preserving and permitting the perpetuation of the weaklings of the race, which, had natural selection exercised its unhindered sway, would have been crushed out of existence in the struggle for life.

It is, however, of the essence of true scientific knowledge, when perfected, that it enables us to predict, and if we ever rise to the possession of a true appreciation of the influences which have affected mankind in the past, we should endeavour to learn how to direct these influences in the future that they shall work for the progress of the race. With such a knowledge we shall be able to advance in that practical branch of Anthropology, the science of education; and so to guide and foster the physical, intellectual, and moral growth of the individual that he will be enabled to exercise all his powers in the best possible directions. And lastly, we shall make progress in that kindred department, Sociology, the study of which does for the community what the science of education does for the individual. Is it a dream that the future has in store for us such an Anthropological Utopia?

# British Association for the Advancement of Science.

## NOTTINGHAM, 1893.

## ADDRESS

BY

# J. S. BURDON-SANDERSON,

M.A., M.D , LL.D., D.C.L , F.R S., F.R S E , Professor of Physiology
in the University of Oxford,

## PRESIDENT.

WE are assembled this evening as representatives of the sciences—men
and women who seek to advance knowledge by scientific methods. The
common ground on which we stand is that of belief in the paramount
value of the end for which we are striving, of its inherent power to make
men wiser, happier, and better ; and our common purpose is to strengthen
and encourage one another in our efforts for its attainment. We have
come to learn what progress has been made in departments of knowledge
which lie outside of our own special scientific interests and occupations,
to widen our views, and to correct whatever misconceptions may have
arisen from the necessity which limits each of us to his own field of study ;
and, above all, we are here for the purpose of bringing our divided ener-
gies into effectual and combined action.

Probably few of the members of the Association are fully aware of
the influence which it has exercised during the last half-century and
more in furthering the scientific development of this country. Wide as
is the range of its activity, there has been no great question in the field
of scientific inquiry which it has failed to discuss; no important line of
investigation which it has not promoted; no great discovery which it has
not welcomed. After more than sixty years of existence it still finds
itself in the energy of middle life, looking back with satisfaction to what
it has accomplished in its youth, and forward to an even more efficient
future. One of the first of the national associations which exist in different
countries for the advancement of science, its influence has been more felt
than that of its successors because it is more wanted. The wealthiest

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country in the world, which has profited more—vastly more—by science than any other, England stands alone in the discredit of refusing the necessary expenditure for its development, and cares not that other nations should reap the harvest for which her own sons have laboured.

It is surely our duty not to rest satisfied with the reflection that England in the past has accomplished so much, but rather to unite and agitate in the confidence of eventual success. It is not the fault of governments, but of the nation, that the claims of science are not recognised. We have against us an overwhelming majority of the community, not merely of the ignorant, but of those who regard themselves as educated, who value science only in so far as it can be turned into money ; for we are still in great measure—in greater measure than any other—a nation of shopkeepers. Let us who are of the minority—the remnant who believe that truth is in itself of supreme value, and the knowledge of it of supreme utility—do all that we can to bring public opinion to our side, so that the century which has given Young, Faraday, Lyell, Darwin, Maxwell, and Thomson to England, may before it closes see us prepared to take our part with other countries in combined action for the full development of natural knowledge.

Last year the necessity of an imperial observatory for physical science was, as no doubt many are aware, the subject of a discussion in Section A, which derived its interest from the number of leading physicists who took part in it, and especially from the presence and active participation of the distinguished man who is at the head of the National Physical Laboratory at Berlin. The equally pressing necessity for a central institution for chemistry, on a scale commensurate with the practical importance of that science, has been insisted upon in this Association and elsewhere by distinguished chemists. As regards biology I shall have a word to say in the same direction this evening. Of these three requirements it may be that the first is the most pressing. If so, let us all, whatever branch of science we represent, unite our efforts to realise it, in the assurance that if once the claim of science to liberal public support is admitted, the rest will follow.

In selecting a subject on which to address you this evening I have followed the example of my predecessors in limiting myself to matters more or less connected with my own scientific occupations, believing that in discussing what most interests myself I should have the best chance of interesting you. The circumstance that at the last meeting of the British Association in this town, Section D assumed for the first time the title which it has since held, that of the Section of Biology, suggested to me that I might take the word 'biology' as my starting-point, giving you some account of its origin and first use, and of the relations which subsist between biology and other branches of natural science.

## Origin and Meaning of the Term 'Biology.'

The word 'biology,' which is now so familiar as comprising the sum of the knowledge which has as yet been acquired concerning living nature, was unknown until after the beginning of the present century. The term was first employed by Treviranus, who proposed to himself as a life-task the development of a new science, the aim of which should be to study the forms and phenomena of life, its origin and the conditions and laws of its existence, and embodied what was known on these subjects in a book of seven volumes, which he entitled 'Biology, or the Philosophy of Living Nature.' For its construction the material was very scanty, and was chiefly derived from the anatomists and physiologists. For botanists were entirely occupied in completing the work which Linnæus had begun, and the scope of zoology was in like manner limited to the description and classification of animals. It was a new thing to regard the study of living nature as a science by itself, worthy to occupy a place by the side of natural philosophy, and it was therefore necessary to vindicate its claim to such a position. Treviranus declined to found this claim on its useful applications to the arts of agriculture and medicine, considering that to regard any subject of study in relation to our bodily wants—in other words to utility—was to narrow it, but dwelt rather on its value as a discipline and on its surpassing interest. He commends biology to his readers as a study which, above all others, 'nourishes and maintains the taste for simplicity and nobleness; which affords to the intellect ever new material for reflection, and to the imagination an inexhaustible source of attractive images.'

Being himself a mathematician as well as a naturalist, he approaches the subject both from the side of natural philosophy and from that of natural history, and desires to found the new science on the fundamental distinction between living and non-living material. In discussing this distinction, he takes as his point of departure the constancy with which the activities which manifest themselves in the universe are balanced, emphasising the impossibility of excluding from that balance the vital activities of plants and animals. The difference between vital and physical processes he accordingly finds, not in the nature of the processes themselves, but in their co-ordination; that is, in their adaptedness to a given purpose, and to the peculiar and special relation in which the organism stands to the external world. All of this is expressed in a proposition difficult to translate into English, in which he defines life as consisting in the reaction of the organism to external influences, and contrasts the uniformity of vital reactions with the variety of their exciting causes.[1]

[1] 'Leben besteht in der Gleichförmigkeit der Reaktionen bei ungleichförmigen Einwirkungen der Aussenwelt.'—Treviranus, *Biologie oder Philosophie der lebenden Natur*, Göttingen, 1802. vol. i p. 83.

The purpose which I have in view in taking you back as I have done
to the beginning of the century is not merely to commemorate the work
done by the wonderfully acute writer to whom we owe the first scientific
conception of the science of life as a whole, but to show that this con-
ception, as expressed in the definition I have given you as its foundation,
can still be accepted as true.  It suggests the *idea of organism* as that to
which all other biological ideas must relate.  It also suggests, although
perhaps it does not express it, that *action* is not an attribute of the
organism but of its essence—that if, on the one hand, protoplasm is the
basis of life, life is the basis of protoplasm.  Their relations to each
other are reciprocal.  We think of the visible structure only in con-
nection with the invisible process.  The definition is also of value as
indicating at once the two lines of inquiry into which the science has
divided by the natural evolution of knowledge.  These two lines may be
easily educed from the general principle from which Treviranus started,
according to which it is the fundamental characteristic of the organism
that all that goes on in it is to the advantage of the whole.  I need
scarcely say that this fundamental conception of organism has at all
times presented itself to the minds of those who have sought to under-
stand the distinction between living and non-living.  Without going
back to the true father and founder of biology, Aristotle, we may recall
with interest the language employed in relation to it by the physiologists
of three hundred years ago.  It was at that time expressed by the term
*consensus partium*—which was defined as the concurrence of parts in
action, of such a nature that each does *quod suum est*, all combining
to bring about one effect 'as if they had been in secret council,' but at
the same time *constanti quadam naturæ lege*.[1]  Professor Huxley has
made familiar to us how a century later Descartes imagined to himself a
mechanism to carry out this *consensus*, based on such scanty knowledge
as was then available of the structure of the nervous system.  The dis-
coveries of the early part of the present century relating to reflex action
and the functions of sensory and motor nerves, served to realise in a
wonderful way his anticipations as to the channels of influence, afferent
and efferent, by which the *consensus* is maintained ; and in recent times
(as we hope to learn from Professor Horsley's lecture on the physiology
of the nervous system) these channels have been investigated with
extraordinary minuteness and success.

Whether with the old writers we speak about *consensus*, with Treviranus
about *adaptation*, or are content to take *organism* as our point of departure,
it means that, regarding a plant or an animal as an organism, we concern
ourselves primarily with its activities or, to use the word which best ex-
presses it, its energies.  Now the first thing that strikes us in beginning
to think about the activities of an organism is that they are naturally

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[1] Bausner, *De Consensu Partium Humani Corporis*, Amst., 1556, Præf. ad lec-
torem, p 4.

distinguishable into two kinds, according as we consider the action of the whole organism in its relation to the external world or to other organisms, or the action of the parts or organs in their relation to each other. The distinction to which we are thus led between the *internal* and *external* relations of plants and animals has of course always existed, but has only lately come into such prominence that it divides biologists more or less completely into two camps—on the one hand those who make it their aim to investigate the actions of the organism and its parts by the accepted methods of physics and chemistry, carrying this investigation as far as the conditions under which each process manifests itself will permit; on the other those who interest themselves rather in considering the place which each organism occupies, and the part which it plays in the economy of nature. It is apparent that the two lines of inquiry, although they equally relate to what the organism *does*, rather than to what it *is*, and therefore both have equal right to be included in the one great science of life, or biology, yet lead in directions which are scarcely even parallel. So marked, indeed, is the distinction, that Professor Haeckel some twenty years ago proposed to separate the study of organisms with reference to their place in nature under the designation of ' œcology,' defining it as comprising ' the relations of the animal to its organic as well as to its inorganic environment, particularly its friendly or hostile relations to those animals or plants with which it comes into direct contact.' [1] Whether this term expresses it or not, the distinction is a fundamental one. Whether with the œcologist we regard the organism in relation to the world, or with the physiologist as a wonderful complex of vital energies, the two branches have this in common, that both studies fix their attention, not on stuffed animals, butterflies in cases, or even microscopical sections of the animal or plant body—all of which relate to the framework of life—but on life itself.

The conception of biology which was developed by Treviranus as far as the knowledge of plants and animals which then existed rendered possible, seems to me still to express the scope of the science. I should have liked, had it been within my power, to present to you both aspects of the subject in equal fulness; but I feel that I shall best profit by the present opportunity if I derive my illustrations chiefly from the division of biology to which I am attached—that which concerns the *internal* relations of the organism, it being my object not to specialise in either direction, but, as Treviranus desired to do, to regard it as part—surely a very important part—of the great science of nature.

The origin of life, the first transition from non-living to living, is a

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[1] These he identifies with 'those complicated mutual relations which Darwin designates as conditions of the struggle for existence.' Along with chorology—the distribution of animals—œcology constitutes what he calls *Relations-physiologie.* Haekel, 'Entwickelungsgang u. Aufgaben der Zoologie,' *Jenaische Zeitschr.* vol. v. 1869, p. 353.

riddle which lies outside of our scope. No seriously-minded person, however, doubts that organised nature as it now presents itself to us has become what it is by a process of gradual perfecting or advance‑ ment, brought about by the elimination of those organisms which failed to obey the fundamental principle of adaptation which Treviranus indi‑ cated. Each step, therefore, in this evolution is a reaction to external influences, the motive of which is essentially the same as that by which from moment to moment the organism governs itself. And the whole process is a necessary outcome of the fact that those organisms are most prosperous which look best after their own welfare. As in that part of biology which deals with the internal relations of the organism, the interest of the individual is in like manner the sole motive by which every energy is guided. We may take what Treviranus called *selfish* adaptation — *Zweckmassigkeit fur sich selber*—as a connecting link between the two branches of biological study. Out of this relation springs another which I need not say was not recognised until after the Darwinian epoch—that, I mean, which subsists between the two evolu‑ tions, that of the race and that of the individual. Treviranus, no less distinctly than his great contemporary Lamarck, was well aware that the affinities of plants and animals must be estimated according to their developmental value, and consequently that classification must be founded on development; but it occurred to no one what the real link was between descent and development; nor was it, indeed, until several years after the publication of the 'Origin' that Haeckel enunciated that 'biogenetic law' according to which the development of any individual organism is but a memory, a recapitulation by the individual of the development of the race—of the process for which Fritz Muller had coined the excellent word 'phylogenesis'; and that each stage of the former is but a transitory reappearance of a bygone epoch in its ancestral history. If, therefore, we are right in regarding ontogenesis as dependent on phylogenesis the origin of the former must correspond with that of the latter; that is, on the power which the race or the organism at every stage of its existence possesses of profiting by every condition or circumstance for its own advancement.

From the short summary of the connection between different parts of our science you will see that biology naturally falls into three divisions, and these are even more sharply distinguished by their methods than by their subjects; namely, *Physiology*, of which the methods are entirely ex‑ perimental; *Morphology*, the science which deals with the forms and structure of plants and animals, and of which it may be said that the body is anatomy, the soul, development; and finally *Œcology*, which uses all the knowledge it can obtain from the other two, but chiefly rests on the exploration of the endless varied phenomena of animal and plant life as they manifest themselves under natural conditions. This last branch of biology—the science which concerns itself with the external relations of

plants and animals to each other, and to the past and present conditions of their existence—is by far the most attractive. In it those qualities of mind which especially distinguish the naturalist find their highest exercise, and it represents more than any other branch of the subject what Treviranus termed the 'philosophy of living nature.' Notwithstanding the very general interest which several of its problems excite at the present moment I do not propose to discuss any of them, but rather to limit myself to the humbler task of showing that the fundamental idea which finds one form of expression in the world of living beings regarded as a whole—the prevalence of the best—manifests itself with equal distinctness, and plays an equally essential part in the internal relations of the organism in the great science which treats of them—Physiology.

### Origin and Scope of Modern Physiology.

Just as there was no true philosophy of living nature until Darwin, we may with almost equal truth say that physiology did not exist as a science before Johannes Müller. For although the sum of his numerous achievements in comparative anatomy and physiology, notwithstanding their extraordinary number and importance, could not be compared for merit and fruitfulness with the one discovery which furnished the key to so many riddles, he, no less than Darwin, by his influence on his successors was the beginner of a new era.

Muller taught in Berlin from 1833 to 1857. During that time a gradual change was in progress in the way in which biologists regarded the fundamental problem of life. Muller himself, in common with Treviranus and all the biological teachers of his time, was a vitalist, i e., he regarded what was then called the *vis vitalis*—the *Lebenskraft*—as something capable of being correlated with the physical forces ; and as a necessary consequence held that phenomena should be classified or distinguished, according to the forces which produced them, as vital or physical, and that all those processes—that is groups or series of phenomena in living organisms—for which, in the then very imperfect knowledge which existed, no obvious physical explanation could be found, were sufficiently explained when they were stated to be dependent on so-called vital laws. But during the period of Muller's greatest activity times were changing, and he was changing with them. During his long career as professor at Berlin he became more and more objective in his tendencies, and exercised an influence in the same direction on the men of the next generation, teaching them that it was better and more useful to observe than to philosophise ; so that, although he himself is truly regarded as the last of the vitalists—for he was a vitalist to the last—his successors were adherents of what has been very inadequately designated the mechanistic view of the phenomena of life. The change thus brought about just before the middle of this century was a revolution. It was not a substitution of one point of view for another, but simply a frank aban-

donment of theory for fact, of speculation for experiment.  Physiologists ceased to theorise because they found something better to do.  May I try to give you a sketch of this era of progress ?

Great discoveries as to the structure of plants and animals had been made in the course of the previous decade, those especially which had resulted from the introduction of the microscope as an instrument of research.  By its aid Schwann had been able to show that all organised structures are built up of those particles of living substance which we now call cells, and recognise as the seats and sources of every kind of vital activity.  Hugo Mohl, working in another direction, had given the name ' protoplasm ' to a certain hyaline substance which forms the lining of the cells of plants, though no one as yet knew that it was the essential constituent of all living structures—the basis of life no less in animals than in plants.  And, finally, a new branch of study—histology—founded on observations which the microscope had for the first time rendered possible, had come into existence.  Bowman, one of the earliest and most successful cultivators of this new science, called it physiological anatomy,[1] and justified the title by the very important inferences as to the secreting function of epithelial cells and as to the nature of muscular contraction, which he deduced from his admirable anatomical researches.  From structure to function, from microscopical observation to physiological experiment, the transition was natural.  Anatomy was able to answer some questions, but asked many more.  Fifty years ago physiologists had microscopes but had no laboratories.  English physiologists—Bowman, Paget, Sharpey—were at the same time anatomists, and in Berlin Johannes Muller, along with anatomy and physiology, taught comparative anatomy and pathology.  But soon that specialisation which, however much we may regret its necessity, is an essential concomitant of progress, became more and more inevitable.  The structural conditions on which the processes of life depend had become, if not known, at least accessible to investigation ; but very little indeed had been ascertained of the nature of the processes themselves—so little indeed that if at this moment we could blot from the records of physiology the whole of the information which had been acquired, say in 1840, the loss would be difficult to trace—not that the previously known facts were of little value, but because every fact of moment has since been subjected to experimental verification.  It is for this reason that, without any hesitation, we accord to Müller and to his successors Brücke, du Bois-Reymond, Helmholtz, who were his pupils, and Ludwig, in Germany, and to Claude Bernard[2] in France, the title of founders of our science.  For it is

[1] The first part of the *Physiological Anatomy* appeared in 1843.  It was concluded in 1856.

[2] It is worthy of note that these five distinguished men were nearly contemporaries . Ludwig graduated in 1839, Bernard in 1843, the other three between those dates.  Three survive—Helmholtz, Ludwig, du Bois-Reymond.

the work which they began at that remarkable time (1845-55), and which is now being carried on by their pupils or their pupils' pupils in England, America, France, Germany, Denmark, Sweden, Italy, and even in that youngest contributor to the advancement of science, Japan, that physiology has been gradually built up to whatever completeness it has at present attained.

What were the conditions which brought about this great advance which coincided with the middle of the century? There is but little difficulty in answering the question. I have already said that the change was not one of doctrine, but of method. There was, however, a leading idea in the minds of those who were chiefly concerned in bringing it about. That leading notion was, that, however complicated may be the conditions under which vital energies manifest themselves, they can be split into processes which are identical in nature with those of the non-living world, and, as a corollary to this, that the analysing of a vital process into its physical and chemical constituents, so as to bring these constituents into measurable relation with physical or chemical standards, is the only mode of investigating them which can lead to satisfactory results.

There were several circumstances which at that time tended to make the younger physiologists (and all of the men to whom I have just referred were then young) sanguine, perhaps too sanguine, in the hope that the application of experimental methods derived from the exact sciences would afford solutions of many physiological problems. One of these was the progress which had been made in the science of chemistry, and particularly the discovery that many of the compounds which before had been regarded as special products of vital processes could be produced in the laboratory, and the more complete knowledge which had been thereby acquired of their chemical constitutions and relations. In like manner, the new school profited by the advances which had been made in physics, partly by borrowing from the physical laboratory various improved methods of observing the phenomena of living beings, but chiefly in consequence of the direct bearing of the crowning discovery of that epoch (that of the conservation of energy) on the discussions which then took place as to the relations between vital and physical forces; in connection with which it may be noted that two of those who (along with Mr. Joule and your President at the last Nottingham meeting) took a prominent part in that discovery—Helmholtz and J. R. Mayer—were physiologists as much as they were physicists. I will not attempt even to enumerate the achievements of that epoch of progress. I may, however, without risk of wearying you, indicate the lines along which research at first proceeded, and draw your attention to the contrast between then and now. At present a young observer who is zealous to engage in research finds himself provided with the most elaborate means of investigation, the chief obstacle to his success being that the problems which have

been left over by his predecessors are of extreme difficulty, all of the
easier questions having been worked out.    There were then also difficul-
ties, but of an entirely different kind.    The work to be done was in itself
easier, but the means for doing it were wanting, and every investigator
had to depend on his own resources.    Consequently the successful men
were those who, in addition to scientific training, possessed the ingenuity
to devise and the skill to carry out methods for themselves.    The work
by which du Bois-Reymond laid the foundation of animal electricity
would not have been possible had not its author, besides being a trained
physicist, known how to do as good work in a small room in the upper
floor of the old University Building at Berlin as any which is now done
in his splendid laboratory.    Had Ludwig not possessed mechanical apti-
tude, in addition to scientific knowledge, he would have been unable to
devise the apparatus by which he measured and recorded the variations
of arterial pressure (1848), and verified the principles which Young had
laid down thirty years before as to the mechanics of the circulation.
Nor, lastly, could Helmholtz, had he not been a great deal more than a
mere physiologist, have made those measurements of the time-relations of
muscular and nervous responses to stimulation, which not only afford a
solid foundation for all that has been done since in the same direction,
but have served as models of physiological experiment, and as evidence
that perfect work was possible and was done by capable men, even when
there were no physiological laboratories.

Each of these examples relates to work done within a year or two of
the middle of the century.[1]    If it were possible to enter more fully on the
scientific history of the time, we should, I think, find the clearest evidence,
first, that the foundation was laid in anatomical discoveries, in which it
is gratifying to remember that English anatomists (Allen Thomson,
Bowman, Goodsir, Sharpey) took considerable share; secondly, that
progress was rendered possible by the rapid advances which, during the
previous decade, had been made in physics and chemistry, and the partici-
pation of physiology in the general awakening of the scientific spirit
which these discoveries produced.    I venture, however, to think that,
notwithstanding the operation of these two causes, or rather combinations
of causes, the development of our science would have been delayed had it
not been for the exceptional endowments of the four or five young experi-
menters whose names I have mentioned, each of whom was capable of
becoming a master in his own branch, and of guiding the future progress
of inquiry.

Just as the affinities of an organism can be best learned from its
development, so the scope of a science may be most easily judged of by

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[1] The *Untersuchungen uber thierische Electricitat* appeared in 1848; Ludwig's
researches on the circulation, which included the first description of the 'kymo-
graph' and served as the foundation of the 'graphic method' in 1847; Helmholtz's
research on the propagation in motor nerves in 1851

the tendencies which it exhibits in its origin. I wish now to complete the sketch I have endeavoured to give of the way in which physiology entered on the career it has since followed for the last half-century, by a few words as to the influence exercised on general physiological theory by the progress of research. We have seen that no real advance was made until it became possible to investigate the phenomena of life by methods which approached more or less closely to those of the physicist, in exactitude. The methods of investigation being physical or chemical, the organism itself naturally came to be considered as a complex of such processes, and nothing more. And in particular the idea of adaptation, which, as I have endeavoured to show, is not a consequence of organism, but its essence, was in great measure lost sight of. Not, I think, because it was any more possible than before to conceive of the organism otherwise than as a working together of parts for the good of the whole, but rather that, if I may so express it, the minds of men were so occupied with new facts that they had not time to elaborate theories. The old meaning of the term 'adaptation' as the equivalent of ' design ' had been abandoned, and no new meaning had yet been given to it, and consequently the word ' mechanism ' came to be employed as the equivalent of ' process,' as if the constant concomitance or sequence of two events was in itself a sufficient reason for assuming a mechanical relation between them. As in daily life so also in science, the misuse of words leads to misconceptions. To assert that the link between $a$ and $b$ is mechanical, for no better reason than that $b$ always follows $a$, is an error of statement, which is apt to lead the incautious reader or hearer to imagine that the relation between $a$ and $b$ is understood, when in fact its nature may be wholly unknown. Whether or not at the time which we are considering, some physiological writers showed a tendency to commit this error, I do not think that it found expression in any generally accepted theory of life. It may, however, be admitted that the rapid progress of experimental investigation led to too confident anticipations, and that to some enthusiastic minds it appeared as if we were approaching within measurable distance of the end of knowledge. Such a tendency is, I think, a natural result of every signal advance. In an eloquent Harveian oration, delivered last autumn by Dr. Bridges, it was indicated how, after Harvey's great discovery of the circulation, men were too apt to found upon it explanations of all phenomena whether of health or disease, to such an extent that the practice of medicine was even prejudicially affected by it. In respect of its scientific importance the epoch we are considering may well be compared with that of Harvey, and may have been followed by an undue preference of the new as compared with the old, but no more permanent unfavourable results have shown themselves. As regards the science of medicine we need only remember that it was during the years between 1845 and 1860 that Virchow made those researches by which he brought the processes of disease into immediate relation with the normal processes

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of cell-development and growth, and so, by making pathology a part of physiology, secured its subsequent progress and·its influence on practical medicine. Similarly in physiology, the achievements of those years led on without any interruption or drawback to those of the following generation ; while in general biology, the revolution in the mode of regarding the internal processes of the animal or plant organism which resulted from these achievements, prepared the way for the acceptance of the still greater revolution which the Darwinian epoch brought about in the views entertained by naturalists of the relations of plants and animals to each other and to their surroundings.

It has been said that every science of observation begins by going out botanising, by which, I suppose, is meant that collecting and recording observations is the first thing to be done in entering on a new field of inquiry. The remark would scarcely be true of physiology, even at the earliest stage of its development, for the most elementary of its facts could scarcely be picked up as one gathers flowers in a wood. Each of the processes which go to make up the complex of life requires separate investigation, and in each case the investigation must consist in first splitting up the process into its constituent phenomena, and then determining their relation to each other, to the process of which they form part, and to the conditions under which they manifest themselves. It will, I think, be found that even in the simplest inquiry into the nature of vital processes some such order as this is followed. Thus, for example, if muscular contraction be the subject on which we seek information, it is obvious that, in order to measure its duration, the mechanical work it accomplishes, the heat wasted in doing it, the electro-motive forces which it develops, and the changes of form associated with these phenomena, special modes of observation must be used for each of them, that each measurement must be in the first instance separately made, under special conditions, and by methods specially adapted to the required purpose. In the synthetic part of the inquiry the guidance of experiment must again be sought for the purpose of discriminating between apparent and real causes, and of determining the order in which the phenomena occur. Even the simplest experimental investigations of vital processes are beset with difficulties. For, in addition to the extreme complexity of the phenomena to be examined and the uncertainties which arise from the relative inconstancy of the conditions of all that goes on in the living organism, there is this additional drawback, that, whereas in the exact sciences experiment is guided by well-ascertained laws, here the only principle of universal application is that of adaptation, and that even this cannot, like a law of physics, be taken as a basis for deductions, but only as a summary expression of that relation between external exciting causes and the reactions to which they give rise, which, in accordance with Treviranus' definition, is the essential character of vital activity.

## THE SPECIFIC ENERGIES OF THE ORGANISM.

When in 1826 J. Müller was engaged in investigating the physiology of vision and hearing he introduced into the discussion a term, ' specific energy,' the use of which by Helmholtz [1] in his physiological writings has rendered it familiar to all students. Both writers mean by the word energy, not the ' capacity of doing work,' but simply *activity*, using it in its old-fashioned meaning, that of the Greek word from which it is derived. With the qualification ' specific ' it serves, perhaps, better than any other expression to indicate the way in which adaptation manifests itself. In this more extended sense the ' specific energy ' of a part or organ—whether that part be a secreting cell, a motor cell of the brain or spinal cord, or one of the photogenous cells which produce the light of the glowworm, or the protoplasmic plate which generates the discharge of the torpedo—is simply the special action which it *normally* performs, its norma or rule of action being in each instance the *interest of the organism* as a whole of which it forms part, and the exciting cause some influence outside of the excited structure, technically called a stimulus. It thus stands for a characteristic of living structures which seems to be universal. The apparent exceptions are to be found in those bodily activities which, following Bichat, we call vegetative, because they go on, so to speak, as a matter of course; but the more closely we look into them the more does it appear that they form no exception to the general rule, that every link in the chain of living action, however uniform that action may be, is a response to an antecedent influence. Nor can it well be doubted that, as every living cell or tissue is called upon to act in the interest of the whole, the organism must be capable of influencing every part so as to regulate its action. For, although there are some instances in which the channels of this influence are as yet unknown, the tendency of recent investigations has been to diminish the number of such instances. In general there is no difficulty in determining both the nature of the central influence exercised and the relation between it and the normal function. It may help to illustrate this relation to refer to the expressive word *Auslosung* by which it has for many years been designated by German writers. This word stands for the performance of function by the ' letting off' of ' specific energies.' Carrying out the notion of ' letting off' as expressing the link between action and reaction, we might compare the whole process to the mode of working of a repeating clock (or other similar mechanism), in which case the pressure of the finger on the button would represent the external influence or stimulus, the striking of the clock, the normal reaction. And now may I ask you to consider in detail one or two illustrations of physiological reaction—of the *letting off of specific energy ?*

[1] *Handb. der physiologischen Optik*, 1886, p. 233   Helmholtz uses the word in the plural—the ' energies of the nerves of special sense '

The repeater may serve as a good example, inasmuch as it is, in biological language, a highly differentiated structure, to which a single function is assigned.   So also in the living organism, we find the best examples of specific energy where Müller found them, namely, in the most differentiated, or, as we are apt to call them, the *highest* structures. The retina, with the part of the brain which belongs to it, together constitute such a structure, and will afford us therefore the illustration we want, with this advantage for our present purpose, that the phenomena are such as we all have it in our power to observe in ourselves.   In the visual apparatus the principle of *normality* of reaction is fully exemplified.   In the physical sense the word 'light' stands for ether vibrations, but in the sensuous or subjective sense for sensations.   The swings are the stimulus, the sensations are the reaction.   Between the two comes the link, the 'letting off,' which it is our business to understand.   Here let us remember that the man who first recognised this distinction between the physical and the physiological was not a biologist, but a physicist.   It was Young who first made clear (though his doctrine fell on unappreciative ears) that, although in vision the external influences which give rise to the sensation of light are infinitely varied, the responses need not be more than three in number, each being, in Müller's language, a 'specific energy' of some part of the visual apparatus.   We speak of the organ of vision as *highly differentiated*, an expression which carries with it the suggestion of a distinction of rank between different vital processes.   The suggestion is a true one; for it would be possible to arrange all those parts or organs of which the bodies of the *higher* animals consist in a series, placing at the lower end of the series those of which the functions are continuous, and therefore called vegetative; at the other, those highly specialised structures, as, *e.g.*, those in the brain, which in response to physical light produce physiological, that is subjective, light; or, to take another instance, the so-called motor cells of the surface of the brain, which in response to a stimulus of much greater complexity produce voluntary motion.   And just as in civilised society an individual is valued according to his power of doing one thing well, so the high rank which is assigned to the structure, or rather to the 'specific energy' which it represents, belongs to it by virtue of its specialisation.   And if it be asked how this conformity is manifested, the answer is, by the quality, intensity, duration, and extension of the response, in all which respects vision serves as so good an example, that we can readily understand how it happened that it was in this field that the relation between response and stimulus was first clearly recognised.   I need scarcely say that, however interesting it might be to follow out the lines of inquiry thus indicated, we cannot attempt it this evening.   All that I can do is to mention one or two recent observations which, while they serve as illustrations, may perhaps be sufficiently novel to interest even those who are at home in the subject.

Probably everyone is acquainted with some of the familiar proofs that an object is seen for a much longer period than it is actually exposed to view; that the visual reaction lasts much longer than its cause. More precise observations teach us that this response is regulated according to laws which it has in common with all the higher functions of an organism. If, for example, the cells in the brain of the torpedo are 'let off'— that is, awakened by an external stimulus—the electrical discharge, which, as in the case of vision, follows after a certain interval, lasts a certain time, first rapidly increasing to a maximum of intensity, then more slowly diminishing. In like manner, as regards the visual apparatus, we have, in the response to a sudden invasion of the eye by light, a rise and fall of a similar character. In the case of the electrical organ, and in many analogous instances, it is easy to investigate the time relations of the successive phenomena, so as to represent them graphically. Again, it is found that in many physiological reactions, the period of rising 'energy' (as Helmholtz called it) is followed by a period during which the responding structure is not only inactive, but its capacity for energising is so completely lost that the same exciting cause which a moment before 'let off' the characteristic response is now without effect. As regards vision, it has long been believed that these general characteristics of physiological reaction have their counterpart in the visual process, the most striking evidence being that in the contemplation of a lightning flash—or, better, of an instantaneously illuminated white disc [1]— the eye seems to receive a double stroke, indicating that, although the stimulus is single and instantaneous, the response is reduplicated. The most precise of the methods we until lately possessed for investigating the wax and wane of the visual reaction, were not only difficult to carry out but left a large margin of uncertainty. It was therefore particularly satisfactory when M. Charpentier, of Nancy, whose merits as an investigator are perhaps less known than they deserve to be, devised an experiment of extreme simplicity which enables us, not only to observe, but to measure with great facility both phases of the reaction. It is difficult to explain even the simplest apparatus without diagrams, you will, however, understand the experiment if you will imagine that you are contemplating a disc, like those ordinarily used for colour mixing; that it is divided by two radial lines which diverge from each other at an angle of 60°; that the sector which these lines enclose is white, the rest black; that the disc revolves slowly, about once in two seconds. You then see, close to the front edge of the advancing sector, a black bar, followed by a second at the same distance from itself but much fainter. Now the scientific value of the experiment consists in this, that the angular distance of the bar from the black border is in proportion to the frequency of the revolutions—the faster the wider. If, for

[1] The phenomenon is best seen when, in a dark room, the light of a luminous spark is thrown on to a white screen with the aid of a suitable lens.

example, when the disc makes half a revolution in a second the distance is ten degrees, this obviously means that when light bursts into the eye, the extinction happens one-eighteenth of a second after the excitation.[1]

The fact thus demonstrated, that the visual reaction consequent on an instantaneous illumination exhibits the alternations I have described, has enabled M. Charpentier to make out another fact in relation to the visual reaction which is, I think, of equal importance. In all the instances, excepting the retina, in which the physiological response to stimulus has a definite time-limitation, and in so far resembles an explosion—in other words, in all the higher forms of specific energy, it can be shown experimentally that the process is propagated from the part first directly acted on to other contiguous parts of similar endowment. Thus in the simplest of all known phenomena of this kind, the electrical change, by which the leaf of the Dionæa plant responds to the slightest touch of its sensitive hairs, is propagated from one side of the leaf to the other, so that in the opposite lobe the response occurs after a delay which is proportional to the distance between the spot excited and the spot observed. That in the retina there is also such propagation has not only been surmised from analogy, but inferred from certain observed facts. M. Charpentier has now been able by a method which, although simple, I must not attempt to describe, not only to prove its existence, but to measure its rate of progress over the visual field.

There is another aspect of the visual response to the stimulus of light which, if I am not trespassing too long on your patience, may, I think, be interesting to consider. As the relations between the sensations of colour and the physical properties of the light which excites them, are among the most certain and invariable in the whole range of vital reactions, it is obvious that they afford as fruitful a field for physiological investigation as those in which white light is concerned. We have on one side physical facts, that is, wave-lengths or vibration-rates; on the other, facts in consciousness—namely, sensations of colour—so simple that notwithstanding their subjective character there is no difficulty in measuring either their intensity or their duration. Between these there are *lines of influence*, neither physical nor psychological, which pass from the former to the latter through the visual apparatus (retina, nerve, brain). It is these lines of influence which interest the physiologist The structure of the visual apparatus affords us no clues to trace them by. The most important fact we know about them is that they must be at least three in number.

It has been lately assumed by some that vision, like every other specific energy, having been developed progressively, objects were seen

[1] Charpentier, 'Réaction oscillatoire de la Rétine sous l'influence des excitations lumineuses,' *Archives de Physiol.*, vol. xxiv. p. 541, and *Propagation de l'action oscillatoire*, &c., p. 362.

by the most elementary forms of eye only in chiaroscuro, that afterwards some colours were distinguished, eventually all. As regards hearing it is so. The organ which, on structural grounds, we consider to represent that of hearing in animals low in the scale of organisation—as, *e.g.*, in the Ctenophora—has nothing to do with sound,[1] but confers on its possessor the power of judging of the direction of its own movements in the water in which it swims, and of guiding these movements accordingly. In the lowest vertebrates, as, *e g.*, in the dogfish, although the auditory apparatus is much more complicated in structure, and plainly corresponds with our own, we still find the particular part which is concerned in hearing scarcely traceable. All that is provided for is that sixth sense, which the higher animals also possess, and which enables them to judge of the direction of their own movements. But a stage higher in the vertebrate series we find the special mechanisms by which we ourselves appreciate sounds beginning to appear—not supplanting or taking the place of the imperfect organ, but added to it. As regards hearing, therefore, a new function is acquired without any transformation or fusion of the old into it. We ourselves possess the sixth sense, by which we keep our balance and which serves as the guide to our bodily movements. It resides in the part of the internal ear which is called the labyrinth. At the same time we enjoy along with it the possession of the cochlea, that more complicated apparatus by which we are able to hear sounds and to discriminate their vibration-rates.

As regards vision, evidence of this kind is wanting. There is, so far as I know, no proof that visual organs which are so imperfect as to be incapable of distinguishing the forms of objects, may not be affected differently by their colours. Even if it could be shown that the least perfect forms of eye possess only the power of discriminating between light and darkness, the question whether in our own such a faculty exists separately from that of distinguishing colours is one which can only be settled by experiment. As in all sensations of colour the sensation of brightness is mixed, it is obvious that one of the first points to be determined is whether the latter represents a 'specific energy' or merely a certain combination of specific energies which are excited by colours. The question is not whether there is such a thing as white light, but whether we possess a separate faculty by which we judge of light and shade—a question which, although we have derived our knowledge of it chiefly from physical experiment, is one of eye and brain, not of wavelengths or vibration-rates, and is therefore essentially physiological.

There is a German proverb which says, 'Bei Nacht sind alle Katzen grau.' The fact which this proverb expresses presents itself experimentally when a spectrum projected on a white surface is watched, while the

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[1] Verworn, 'Gleichgewicht u Otolithenorgan,' *Pfluger's Archiv*, vol. l., p. 423; also Ewald's Researches on the Labyrinth as a Sense-organ (*Ueber das Endorgan des Nervus octavus*, Wiesbaden, 1892).

intensity of the light is gradually diminished.  As the colours fade away
they become indistinguishable as such, the last seen being the primary
red and green.  Finally they also disappear, but a grey band of light
still remains, of which the most luminous part is that which before was
green.[1]  Without entering into details, let us consider what this tells us
of the specific energy of the visual apparatus.  Whether or not the
faculty by which we see grey in the dark is one which we possess in
common with animals of imperfectly developed vision, there seems little
doubt that there are individuals of our own species who, in the fullest
sense of the expression, have no eye for colour ; in whom all colour sense
is absent ; persons who inhabit a world of grey, seeing all things as they
might have done had they and their ancestors always lived nocturnal
lives.  In the theory of colour vision, as it is commonly stated, no reference
is made to such a faculty as we are now discussing.

Professor Hering, whose observations as to the diminished spectrum
I referred to just now, who was among the first to subject the vision of
the *totally* colour-blind to accurate examination, is of opinion, on that
and on other grounds, that the sensation of light and shade is a specific
faculty.  Very recently the same view has been advocated on a wide basis
by a distinguished psychologist, Professor Ebbinghaus.[2]  Happily, as
regards the actual experimental results relating to both these main
subjects, there seems to be a complete coincidence of observation between
observers who interpret them differently.  Thus the recent elaborate
investigations of Captain Abney [3] (with General Festing), representing
graphically the results of his measurements of the subjective values of
the different parts of the diminished spectrum, as well as those of the
fully illuminated spectrum as seen by the totally colour-blind, are in the
closest accord with the observations of Hering, and have, moreover, been
substantially confirmed in both points by the measurements of Dr. König
in Helmholtz' laboratory at Berlin.[4]  That observers of such eminence
as the three persons whom I have mentioned, employing different methods
and with a different purpose in view, and without reference to each
other's work, should arrive in so complicated an inquiry at coincident
results, augurs well for the speedy settlement of this long-debated
question.  At present the inference seems to be that such a specific
energy as Hering's theory of vision postulates actually exists, and that
it has for associates the colour-perceiving activities of the visual appa-
ratus, provided that these are present ; but that whenever the intensity

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[1] Hering, 'Untersuch. eines total Farbenblinden,' *Pflüger's Arch.*, vol. xlix., 1891,
p. 563.

[2] Ebbinghaus, 'Theorie des Farbensehens,' *Zeitschr. f Psychol.*, vol v., 1893, p. 145.

[3] Abney and Festing, Colour Photometry, Part III.  *Phil. Trans*, vol. clxxxii A,
1891, p. 531.

[4] König, 'Ueber den Helligkeitwerth der Spectralfarben bei verschiedener
absoluter Intensität,' *Beitrage zur Psychologie*, &c., 'Festschrift zu H. von Helmholtz,
70. Geburtstage,' 1891, p. 309.

of the illumination is below the chromatic threshold—that is, too feeble
to awaken these activities—or when, as in the totally colour-blind, they
are wanting, it manifests itself independently; all of which can be most
easily understood on such a hypothesis as has lately been suggested in
an ingenious paper by Mrs. Ladd Franklin,[1] that each of the elements of
the visual apparatus is made up of a central structure for the sensation
of light and darkness, with collateral appendages for the sensations of
colour—it being, of course, understood that this is a mere diagrammatic
representation, which serves no purposes beyond that of facilitating
the conception of the relation between the several ' specific energies.'

### EXPERIMENTAL PSYCHOLOGY.

Resisting the temptation to pursue this subject further, I will now ask
you to follow me into a region which, although closely connected with
the subjects we have been considering, is beset with greater difficulties—
the subject in which, under the name of Physiological or Experimental
Psychology, physiologists and psychologists have of late years taken a
common interest—a borderland not between fact and fancy, but between
two methods of investigation of questions which are closely related,
which here, though they do not overlap, at least interdigitate. It is
manifest that, quite irrespectively of any foregone conclusion as to the
dependence of mind on processes of which the biologist is accustomed to
take cognizance, mind must be regarded as one of the ' specific energies '
of the organism, and should on that ground be included in the subject-
matter of physiology. As, however, our science, like other sciences, is
limited not merely by its subject but also by its method, it actually takes
in only so much of psychology as is experimental. Thus sensation,
although it is psychological, and the investigation of its relation to the
special structures by which the mind keeps itself informed of what goes
on in the outside world, have always been considered to be in the physio-
logical sphere. And it is by anatomical researches relating to the
minute structure and to the development of the brain, by observation
of the facts of disease, and, above all, by physiological experiment,
that those changes in the ganglion cells of the brain and spinal cord
which are the immediate antecedents of every kind of bodily action have
been traced. Between the two—that is, between sensation and the
beginning of action—there is an intervening region which the physio-
logist has hitherto willingly resigned to psychology, feeling his incompe-
tence to use the only instrument by which it can be explored—that of
introspection. This consideration enables us to understand the course
which the new study (I will not claim for it the title of a new science,
regarding it as merely a part of the great science of life) has hitherto

[1] Christine Ladd Franklin, ' Eine neue Theorie der Lichtempfindungen,' *Zeitschr.
fur Psychologie,* vol iv., 1893, p. 211; see also the Proceedings of the last Psycho-
logical Congress in London, 1892.

followed, and why physiologists have been unwilling to enter on it. The study of the less complicated internal relations of the organism has afforded so many difficult problems that the most difficult of all have been deferred; so that although the psycho-physical method was initiated by E. H. Weber in the middle of the present century, by investigations [1] which formed part of the work done at that epoch of discovery, and although Professor Wundt, also a physiologist, has taken a larger share in the more recent development of the new study, it is chiefly by psychologists that the researches which have given to it its importance as a new discipline have been conducted.

Although, therefore, experimental psychology has derived its methods from physical science, the result has been not so much that physiologists have become philosophers, as that philosophers have become experimental psychologists. In our own universities, in those of America, and still more in those of Germany, psychological students of mature age are to be found who are willing to place themselves in the dissecting-room side by side with beginners in anatomy, in order to acquire that exact knowledge of the framework of the organism without which no man can understand its working. Those, therefore, who are apprehensive lest the regions of mind should be invaded by the *insaniens sapientia* of the laboratory, may, I think, console themselves with the thought that the invaders are for the most part men who before they became laboratory workers had already given their allegiance to philosophy; their purpose being not to relinquish definitively, but merely to lay aside for a time, the weapons in the use of which they had been trained, in order to learn the use of ours. The motive that has encouraged them has not been any hope of finding an experimental solution of any of the ultimate problems of philosophy, but the conviction that, inasmuch as the relation between mental stimuli and the mental processes which they awaken is of the same order with the relation between every other vital process and its specific determinant, the only hope of ascertaining its nature must lie in the employment of the same methods of comparative measurement which the biologist uses for similar purposes. Not that there is necessarily anything scientific in mere measurement, but that measurement affords the only means by which it can be determined whether or not the same conformity in the relation between stimulus and reaction which we have accepted as the fundamental characteristic of life, is also to be found in mind, notwithstanding that mental processes have no known physical concomitants. The results of experimental psychology tend to show that it is so, and consequently that in so far the processes in question are as truly functions of organism as the contraction of a muscle, or as the changes produced in the retinal pigment by light.

I will make no attempt even to enumerate the special lines of inquiry

[1] Weber's researches were published in Wagner's *Handwörterbuch*, I think, in 1849.

which during the last decade have been conducted with such vigour in all parts of the world, all of them traceable to the influence of the Leipzig school; but will content myself with saying that the general purpose of these investigations has been to determine with the utmost attainable precision the nature of psychical relations. Some of these investigations begin with those simpler reactions which more or less resemble those of an automatic mechanism, proceeding to those in which the resulting action or movement is modified by the influence of auxiliary or antagonistic conditions, or changed by the simultaneous or antecedent action on the reagent of other stimuli, in all of which cases the effect can be expressed quantitatively; others lead to results which do not so readily admit of measurement. In pursuing this course of inquiry the physiologist finds himself as he proceeds more and more the *coadjutor* of the psychologist, less and less his *director*; for whatever advantage the former may have in the mere *technique* of observation, the things with which he has to do are revealed only to introspection, and can be studied only by methods which lie outside of his sphere. I might in illustration of this refer to many recent experimental researches—such, for example, as those by which it has been sought to obtain exact data as to the physiological concomitants of pleasure and of pain, or as to the influence of weariness and recuperation, as modifiers of psychological reactions. Another outwork of the mental citadel which has been invaded by the experimental method is that of memory. Even here it can be shown that in the comparison of transitory as compared with permanent memory—as, for example, in the getting off by heart of a wholly uninteresting series of words, with subsequent oblivion and reacquisition—the labour of acquiring and reacquiring may be measured, and consequently the relation between them; and that this ratio varies according to a simple numerical law.

I think it not unlikely that the only effect of what I have said may be to suggest to some of my hearers the question, What is the use of such inquiries? Experimental psychology has, to the best of my knowledge, no technical application. The only satisfactory answer I can give is that it has exercised, and will exercise in future, a helpful influence on the science of life. Every science of observation, and each branch of it, derives from the peculiarities of its methods certain tendencies which are apt to predominate unduly. We speak of this as specialisation, and are constantly striving to resist its influence. The most successful way of doing so is by availing ourselves of the counter-acting influence which two opposite tendencies mutually exercise when they are simultaneous. He that is skilled in the methods of introspection naturally (if I may be permitted to say so) looks at the same thing from an opposite point of view to that of the experimentalist. It is, therefore, good that the two should so work together that the tendency of the experimentalist to imagine the existence of mechanism where none

is proved to exist—of the psychologist to approach the phenomena of mind too exclusively from the subjective side—may mutually correct and assist each other.

## PHOTOTAXIS AND CHEMIOTAXIS.

Considering that every organism must have sprung from a unicellular ancestor, some have thought that unless we are prepared to admit a deferred epigenesis of mind, we must look for psychical manifestations even among the lowest animals, and that as in the protozoon all the vital activities are blended together, mind should be present among them not merely potentially but actually, though in diminished degree.

Such a hypothesis involves ultimate questions which it is unnecessary to enter upon: it will, however, be of interest in connection with our present subject to discuss the phenomena which served as a basis for it—those which relate to what may be termed the behaviour of unicellular organisms and of individual cells, in so far as these last are capable of reacting to external influences. The observations which afford us most information are those in which the stimuli employed can be easily measured, such as electrical currents, light, or chemical agents in solution.

A single instance, or at most two, must suffice to illustrate the influence of light in directing the movements of freely moving cells, or, as it is termed, phototaxis. The rod-like purple organism called by Engelmann *Bacterium photometricum*,[1] is such a light-lover that if you place a drop of water containing these organisms under the microscope, and focus the smallest possible beam of light on a particular spot in the field, the spot acts as a light trap and becomes so crowded with the little rodlets as to acquire a deep port-wine colour. If instead of making his trap of white light, he projected on the field a microscopic spectrum, Engelmann found that the rodlets showed their preference for a spectral colour which is absorbed when transmitted through their bodies. By the aid of a light trap of the same kind, the very well-known spindle-shaped and flagellate cell of Euglena can be shown to have a similar power of discriminating colour, but its preference is different. This familiar organism advances with its flagellum forwards, the sharp end of the spindle having a red or orange eye point. Accordingly, the light it loves is again that which is most absorbed—viz., the blue of the spectrum (line F).

These examples may serve as an introduction to a similar one in which the directing cause of movement is not physical but chemical. The spectral light trap is used in the way already described; the or-

[1] Engelmann, 'Bacterium photometricum,' *Onderzoek. Physiol. Lab. Utrecht*, vol. vii p. 200; also Ueber Licht- u. Farbenperception niederster Organismen, *Pflüger's Arch.*, vol xxix p. 387.

ganisms to be observed are not coloured, but bacteria of that common sort which twenty years ago we used to call *Bacterium termo*, and which is recognised as the ordinary determining cause of putrefaction. These organisms do not care for light, but are great oxygen-lovers. Consequently, if you illuminate with your spectrum a filament of a confervoid alga, placed in water containing bacteria, the assimilation of carbon and consequent disengagement of oxygen is most active in the part of the filament which receives the red rays (B to C). To this part, therefore, where there is a dark band of absorption, the bacteria which want oxygen are attracted in crowds  The motive which brings them together is their desire for oxygen. Let us compare other instances in which the source of attraction is food.

The plasmodia of the myxomycetes, particularly one which has been recently investigated by Mr. Arthur Lister,[1] may be taken as a typical instance of what may be called the chemical allurement of living protoplasm. In this organism, which in the active state is an expansion of labile living material, the delicacy of the reaction is comparable to that of the sense of smell in those animals in which the olfactory organs are adapted to an aquatic life. Just as, for example, the dogfish is attracted by food which it cannot see, so the plasmodium of Badhamia becomes aware, as if it smelled it, of the presence of its food—a particular kind of fungus. I have no diagram to explain this, but will ask you to imagine an expansion of living material, quite structureless, spreading itself along a wet surface; that this expansion of transparent material is bounded by an irregular coast-line ; and that somewhere near the coast there has been placed a fragment of the material on which the Badhamia feeds. The presence of this bit of *Stereum* produces an excitement at the part of the plasmodium next to it. Towards this centre of activity streams of living material converge. Soon the afflux leads to an outgrowth of the plasmodium, which in a few minutes advances towards the desired fragment, envelopes, and incorporates it.

May I give you another example also derived from the physiology of plants ? Very shortly after the publication of Engelmann's observations of the attraction of bacteria by oxygen, Pfeffer made the remarkable discovery that the movements of the antherozoids of ferns and of mosses are guided by impressions derived from chemical sources, by the allurement exercised upon them by certain chemical substances in solution—in one of the instances mentioned by sugar, in the other by an organic acid. The method consisted in introducing the substance to be tested, in any required strength, into a minute capillary tube closed at one end, and placing it under the microscope in water inhabited by antherozoids, which thereupon showed their predilection for the substance, or the contrary, by its effect on their movements. In accordance with the

[1] Lister, ' On the Plasmodium of *Badhamia utricularis*, &c.,' *Annals of Botany*, No. 5  June 1888

principle followed in experimental psychology, Pfeffer [1] made it his object to determine, not the relative effects of different doses, but the smallest perceptible increase of dose which the organism was able to detect, with this result—that, just as in measurements of the relation between stimulus and reaction in ourselves we find that the sensational value of a stimulus depends, not on its absolute intensity, but on the ratio between that intensity and the previous excitation, so in this simplest of vital reagents the same so-called psycho-physical law manifests itself. It is not, however, with a view to this interesting relation that I have referred to Pfeffer's discovery, but because it serves as a centre around which other phenomena, observed alike in plants and animals, have been grouped. As a general designation of reactions of this kind Pfeffer devised the term Chemotaxis, or, as we in England prefer to call it, Chemiotaxis. Pfeffer's contrivance for chemiotactic testing was borrowed from the pathologists, who have long used it for the purpose of determining the relation between a great variety of chemical compounds or products, and the colourless corpuscles of the blood I need, I am sure, make no apology for referring to a question which, although purely pathological, is of very great biological interest—the theory of the process by which, not only in man, but also, as Metschnikoff has strikingly shown, in animals far down in the scale of development, the organism protects itself against such harmful things as, whether particulate or not, are able to penetrate its framework. Since Cohnheim's great discovery in 1867 we have known that the central phenomenon of what is termed by pathologists *inflammation* is what would now be called a chemiotactic one; for it consists in the gathering together, like that of vultures to a carcase, of those migratory cells which have their home in the blood stream and in the lymphatic system, to any point where the living tissue of the body has been injured or damaged, as if the products of disintegration which are set free where such damage occurs were attractive to them.

The fact of chemiotaxis, therefore, as a constituent phenomenon of the process of inflammation, was familiar in pathology long before it was understood. Cohnheim himself attributed it to changes in the channels along which the cells moved, and this explanation was generally accepted, though some writers, at all events, recognised its incompleteness. But no sooner was Pfeffer's discovery known than Leber,[2] who for years had been working at the subject from the pathological side, at once saw that the two processes were of similar nature. Then followed a variety of researches of great interest, by which the importance of chemiotaxis in relation to the destruction of disease-producing microphytes was proved,

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[1] Pfeffer, *Untersuch a d botan Institute z. Tübingen*, vol. i., part 3, 1884
[2] Leber, 'Die Anhaufung der Leucocyten am Orte des Entzündungsreizes,' &c. *Die Entstehung der Enztundung*, &c., pp. 423–464, Leipzig, 1891.

by that of Buchner[1] on the chemical excitability of leucocytes being among the most important. Much discussion has taken place, as many present are aware, as to the kind of wandering cells, or leucocytes, which in the first instance attack morbific microbes, and how they deal with them. The question is not by any means decided. It has, however, I venture to think, been conclusively shown that the process of destruction is a chemical one, that the destructive agent has its source in the chemiotactic cells—that is, cells which act under the orders of chemical stimuli. Two Cambridge observers, Messrs. Kanthack and Hardy,[2] have lately shown that, in the particular instance which they have investigated, the cells which are most directly concerned in the destruction of morbific *bacilli*, although chemiotactic, do not possess the power of incorporating either bacilli or particles of any other kind. While, therefore, we must regard the relation between the process of devitalising and that of incorporating as not yet sufficiently determined, it is now no longer possible to regard the latter as essential to the former.

There seems, therefore, to be very little doubt that chemiotactic cells are among the agents by which the human or animal organism protects itself against infection. There are, however, many questions connected with this action which have not yet been answered. The first of these are chemical ones—that of the nature of the attractive substance and that of the process by which the living carriers of infection are destroyed. Another point to be determined is how far the process admits of adaptation to the particular infection which is present in each case, and to the state of liability or immunity of the infected individual. The subject is therefore of great complication. None of the points I have suggested can be settled by experiments in glass tubes such as I have described to you. These serve only as indications of the course to be followed in much more complicated and difficult investigations—when we have to do with acute diseases as they actually affect ourselves or animals of similar liabilities to ourselves, and find ourselves face to face with the question of their causes.

It is possible that many members of the Association are not aware of the unfavourable—I will not say discreditable—position that this country at present occupies in relation to the scientific study of this great subject—the causes and mode of prevention of infectious diseases. As regards administrative efficiency in matters relating to public health England was at one time far ahead of all other countries, and still retains its superiority ; but as regards scientific knowledge we are, in this subject as in others, content to borrow from our neighbours. Those who desire either to learn the methods of research or to carry out scientific

[1] Buchner, 'Die chem Reizbarkeit der Leucocyten,' &c , *Berliner klin. Woch* , 1890, No 17

[2] Kanthack and Hardy, ' On the Characters and Behaviour of the Wandering Cells of the Frog,' *Proceedings of the Royal Society*, vol lii , p 267

inquiries have to go to Berlin, to Munich, to Breslau, or to the Pasteur Institute in Paris to obtain what England ought long ago to have provided. For to us, from the spread of our race all over the world, the prevention of acute infectious diseases is more important than to any other nation. At the beginning of this address I urged the claims of pure science. If I could, I should feel inclined to speak even more strongly of the application of science to the discovery of the causes of acute diseases. May I express the hope that the effort which is now being made to establish in England an Institution for this purpose not inferior in efficiency to those of other countries, may have the sympathy of all present? And now may I ask your attention for a few moments more to the subject that more immediately concerns us?

## CONCLUSION.

The purpose which I have had in view has been to show that there is one principle—that of adaptation—which separates biology from the exact sciences, and that in the vast field of biological inquiry the end we have is not merely, as in natural philosophy, to investigate the relation between a phenomenon and the antecedent and concomitant conditions on which it depends, but to possess this knowledge in constant reference to the interest of the organism. It may perhaps be thought that this way of putting it is too teleological, and that in taking, as it were, as my text this evening so old-fashioned a biologist as Treviranus, I am yielding to a retrogressive tendency. It is not so. What I have desired to insist on is that *organism* is a fact which encounters the biologist at every step in his investigations; that in referring it to any general biological principle, such as adaptation, we are only referring it to itself, not explaining it; that no explanation will be attainable until the conditions of its coming into existence can be subjected to experimental investigation so as to correlate them with those of processes in the non-living world.

Those who were present at the meeting of the British Association at Liverpool will remember that then, as well as at some subsequent meetings, the question whether the conditions necessary for such an inquiry could be realised was a burning one. This is no longer the case. The patient endeavours which were made about that time to obtain experimental proof of what was called *abiogenesis*, although they conduced materially to that better knowledge which we now possess of the conditions of life of bacteria, failed in the accomplishment of their purpose. The question still remains undetermined; it has, so to speak, been adjourned *sine die*. The only approach to it lies at present in the investigation of those rare instances in which, although the relations between a living organism and its environment ceases as a watch stops when it

has not been wound, these relations can be re-established—the process of life re-awakened—by the application of the required stimulus.

I was also desirous to illustrate the relation between physiology and its two neighbours on either side, natural philosophy (including chemistry) and psychology. As regards the latter I need add nothing to what has already been said. As regards the former, it may be well to notice that although physiology can never become a mere branch of applied physics or chemistry, there are parts of physiology wherein the principles of these sciences may be applied directly. Thus, in the beginning of the century, Young applied his investigations as to the movements of liquids in a system of elastic tubes, directly to the phenomena of the circulation; and a century before, Borelli successfully examined the mechanisms of locomotion and the action of muscles, without reference to any, excepting mechanical principles. Similarly, the foundation of our present knowledge of the process of nutrition was laid in the researches of Bidder and Schmidt, in 1851, by determinations of the weight and composition of the body, the daily gain of weight by food or oxygen, the daily loss by the respiratory and other discharges, all of which could be accomplished by chemical means. But in by far the greater number of physiological investigations, both methods (the physical or chemical and the physiological) must be brought to bear on the same question—to co-operate for the elucidation of the same problem. In the researches, for example, which during several years have occupied Professor Bohr, of Copenhagen, relating to the exchange of gases in respiration, he has shown that factors purely physical—namely, the partial pressures of oxygen and carbon dioxide in the blood which flows through the pulmonary capillaries—are, so to speak, interfered with in their action by the ' specific energy' of the pulmonary tissue, in such a way as to render this fundamental process, which, since Lavoisier, has justly been regarded as one of the most important in physiology, much more complicated than we for a long time supposed it to be. In like manner Heidenhain has proved that the process of lymphatic absorption, which before we regarded as dependent on purely mechanical causes—i.e., differences of pressure—is in great measure due to the specific energy of cells, and that in various processes of secretion the principal part is not, as we were inclined not many years ago to believe, attributable to liquid diffusion, but to the same agency. I wish that there had been time to have told you something of the discoveries which have been made in this particular field by Mr. Langley, who has made the subject of ' specific energy' of secreting-cells his own. It is in investigations of this kind, of which any number of examples could be given, in which vital reactions mix themselves up with physical and chemical ones so intimately that it is difficult to draw the line between them, that the physiologist derives most aid from whatever chemical and physical training he may be fortunate enough to possess.

There is, therefore, no doubt as to the advantages which physiology derives from the exact sciences. It could scarcely be averred that they would benefit in anything like the same degree from closer association with the science of life. Nevertheless, there are some points in respect of which that science may have usefully contributed to the advancement of physics or of chemistry. The discovery of Graham as to the characters of colloid substances, and as to the diffusion of bodies in solution through membranes, would never have been made had not Graham 'ploughed,' so to speak, 'with our heifer.' The relations of certain colouring matters to oxygen and carbon dioxide would have been unknown, had no experiments been made on the respiration of animals and the assimilative process in plants; and, similarly, the vast amount of knowledge which relates to the chemical action of ferments must be claimed as of physiological origin. So also there are methods, both physical and chemical, which were originally devised for physiological purposes. Thus the method by which meteorological phenomena are continuously recorded graphically, originated from that used by Ludwig (1847) in his 'Researches on the Circulation'; the mercurial pump, invented by Lothar Meyer, was perfected in the physiological laboratories of Bonn and Leipzig; the rendering the galvanometer needle aperiodic by damping was first realised by du Bois-Reymond—in all of which cases invention was prompted by the requirements of physiological research.

Let me conclude with one more instance of a different kind, which may serve to show how, perhaps, the wonderful ingenuity of contrivance which is displayed in certain organised structures—the eye, the ear, or the organ of voice—may be of no less interest to the physicist than to the physiologist. Johannes Muller, as is well known, explained the compound eye of insects on the theory that an erect picture is formed on the convex retina by the combination of pencils of light, received from different parts of the visual field through the eyelets (ommatidia) directed to them. Years afterwards it was shown that in each eyelet an image is formed which is reversed. Consequently, the mosaic theory of Müller was for a long period discredited on the ground that an erect picture could not be made up of 'upside-down' images. Lately the subject has been reinvestigated, with the result that the mosaic theory has regained its authority. Professor Exner [1] has proved photographically that behind each part of the insect's eye an erect picture is formed of the objects towards which it is directed. There is, therefore, no longer any difficulty in understanding how the whole field of vision is mapped out as consistently as it is imaged on our own retina, with the difference, of course, that the picture is erect. But behind this fact lies a physical question—that of the relation between the erect picture which is photographed and the optical structure of the crystal cones which produce it—

[1] Exner, *Die Physiologie der facettirten Augen von Krebsen u Insecten*, Leipzig, 1891.

a question which, although we cannot now enter upon it, is quite as interesting as the physiological one.

With this history of a theory which, after having been for thirty years disbelieved, has been reinstated by the fortunate combination of methods derived from the two sciences, I will conclude. It may serve to show how, though physiology can never become a part of natural philosophy, the questions we have to deal with are cognate. Without forgetting that every phenomenon has to be regarded with reference to its useful purpose in the organism, the aim of the physiologist is not to inquire into final causes, but to investigate processes. His question is ever How, rather than Why.

May I illustrate this by a simple, perhaps too trivial, story, which derives its interest from its having been told of the childhood of one of the greatest natural philosophers of the present century? [1] He was even then possessed by that insatiable curiosity which is the first quality of the investigator, and it is related of him that his habitual question was 'What is the *go* of it?' and if the answer was unsatisfactory, 'What is the particular go of it?' That North Country boy became Professor Clerk Maxwell. The questions he asked are those which in our various ways we are all trying to answer.

[1] *Life of Clerk Maxwell* (Campbell and Garnett) p 28

# ADDRESS

### TO THE

# MATHEMATICAL AND PHYSICAL SECTION

### OF THE

# BRITISH ASSOCIATION,

### BY

## R. T. GLAZEBROOK, M.A., F.R.S.,

#### PRESIDENT OF THE SECTION.

BEFORE dealing with the subject which I hope to bring to your notice this morning, I wish to express my deep regret for the circumstances which have evented Professor Clifton, who had accepted the nomination of the Council, from ng your President this year.

It was specially fitting that he who has done so much for this college, and particularly for this laboratory in which we meet, should take the chair at Nottingham. The occasions on which we see him are all too seldom; and we who come frequently to these meetings were looking forward to help and encouragement in our work, derived from his wide experience. You would desire, I feel sure, that I should convey to him the expressions of your sympathy. For myself I must ask that you will pass a lenient judgment on my efforts to fill his place.

Let me commence, then, with a brief retrospect of the past year and the events which concern our Section.

From the days of Galileo the four satellites of Jupiter have been objects of interest to the astronomer. Their existence was one of the earliest of the discoveries of the telescope ; they proved conclusively that all the bodies of the solar system did not move round the earth. The year which has passed since our last meeting is memorable for the discovery of a fifth satellite. It is a year to-day (Sept. 13–14, 1892) since Professor Barnard convinced himself that he had seen with the great telescope of the Lick Observatory this new member of our system as a star of the thirteenth magnitude, revolving round the planet in 11 hours 57 minutes 23 seconds.[1]

The conference on electrical standards held at our meeting last year has had important results. The resolutions adopted at Edinburgh were communicated to the Standards Committee of the Board of Trade. A supplementary report accepting these resolutions was agreed to by that Committee (Nov. 29, 1892), and presented to the President of the Board of Trade. The definitions contained in this report will be made the basis of legislation throughout the world. They have been accepted by France, Germany, Austria, and Italy. There is good reason to believe that the congress at Chicago now being held will ratify them, and then

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[1] 'In general,' he says, 'the satellite has been faint. . . . On the 13th, however, when the air was very clear, it was quite easy.'—*Nature*, Oct. 20, 1892.

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we may claim that your Committee, co-operating with the leaders of physical science in other lands, has secured international agreement on these fundamental points.

Among the physical papers of the year I would mention a few as specially calling for notice. Mr. E. H. Griffiths's re-determination of the value of the mechanical equivalent of heat has just been published,[1] and is a monumental work. With untiring energy and great ability he struggled for five years against the difficulties of his task, and has produced results which, with the exception of one group of experiments, do not differ by more than 1 part in 10,000 ; while the results of that one excepted group differ from the mean only by 1 part in 4,000.

The number of ergs of work required to raise 1 gramme of water 1° C. at 15° C. is $4.194 \times 10^7$. Expressed in foot-pounds and Fahrenheit degrees, the value of J is 778 99. The value obtained by Joule from his experiments on the friction of water, when corrected in 1880 by Rowland so as to reduce his readings to the air thermometer, is 778.5 at 12°.7 C. The result at this temperature of Rowland's own valuable research is 780.1. Another satisfactory outcome of Mr. Griffiths's work is the very exact accordance between the scale of temperature as determined by the comparison of his platinum thermometer with the air thermometer, which was made by Callendar and himself in 1890, and that of the nitrogen thermometer of the Bureau International at Sèvres

Another great work now happily complete is Rowland's 'Table of Standard Wave-lengths.'[2] Nearly a thousand lines have been measured with the skill and accuracy for which Rowland has made himself famous; and in this table we see the results achieved by the genius which designed the concave grating and the mechanical ingenuity which contrived the almost perfect screw.

Those of us who have seen Mr. Higgs's wonderful photographs of the solar spectrum taken with a Rowland grating will rejoice to know that his map also is now finished.

Lord Rayleigh's paper on 'The Intensity of Light reflected from Water and Mercury at nearly perpendicular incidence,'[3] combined with the experiments on reflexion from liquid surfaces in the neighbourhood of the polarising angle,[4] establishes results of the utmost importance to optical theory. 'There is thus,' Lord Rayleigh concludes, 'no experimental evidence against the rigorous application of Fresnel's formulæ'—for the reflexion of polarised light—'to the ideal case of an abrupt transition between two uniform transparent media.'

Professor Dewar has, during the year, continued his experiments on the liquefaction of oxygen and nitrogen on a large scale. To a physicist perhaps the most important results of the research are the discovery of the magnetic properties of liquid oxygen, and the proof of the fact that the resistance of certain pure metals vanishes at absolute zero.[6] The last discovery is borne out by Griffiths and Callendar's experiments with their platinum thermometers.[7]

Mr. Williams's article on 'The Relation of the Dimensions of Physical Quantities to Directions in Space'[5] has led to an interesting discussion. Some of his deductions will be noticed later.

The title-page of the first edition of Maxwell's 'Electricity and Magnetism' bears the date 1873. This year, 1893, we welcome a third edition, edited by Maxwell's distinguished successor, and enriched by a supplementary volume, in which Professor J. J. Thomson describes some of the advances made by electrical science in the last twenty years. The subject matter of this volume might well serve as a text for a Presidential Address.

The choice of a subject on which to speak to-day has been no easy task. The field of physics and mathematics is a wide one. There is one matter, however, to which for a few minutes I should like to call your attention, inadequately though it be. Optical theories have, since the year 1876, when I first read Sir George Stokes's 'Report on Double Refraction,'[8] had a special interest for me, and I think

[1] *Phil Trans*, vol. clxxxiv.
[2] *Phil. Mag.*, July 1893.
[3] *Ibid.*, October 1892.
[4] *Ibid.*, January 1892.
[5] *Phil. Mag.*, September 1892.
[6] *Ibid.*, October 1892.
[7] *Ibid.*, December, 1892.
[8] *British Association Report*, 1862.

the time has come when we may with advantage review our position with regard to them, and sum up our knowledge.[1]

That light is propagated by an undulatory motion through a medium which we call the ether is now an established fact, although we know but little of the nature or constitution of the ether. The history of this undulatory theory is full of interest, and has, it appears to me, in its earlier stages been not quite clearly apprehended. Two theories have been proposed to account for optical phenomena. Descartes was the author of the one, the emission theory. Hooke, though his work was very incomplete, was the founder of the undulatory theory. In his 'Micrographia,' 1664, page 56, he asserts that light is a quick and short vibratory motion, 'propagated every way through an homogeneous medium by direct or straight lines extended every way like rays from the centre of a sphere. . . Every pulse or vibration of the luminous body will generate a sphere which will continually increase and grow bigger, just after the same manner, though indefinitely swifter, as the waves or rings on the surface do swell into bigger and bigger circles about a point on it;' and he gives on this hypothesis an account of reflexion, refraction, dispersion, and the colours of thin plates. In the same work, page 58, he describes an experiment practically identical with Newton's famous prism experiment, published in 1672. Hooke used for a prism a glass vessel about two feet long, filled with water, and inclined so that the sun's rays might enter obliquely at the upper surface and traverse the water. 'The top surface is covered by an opacous body, all but a hole through which the sun's beams are suffered to pass into the water, and are thereby refracted' to the bottom of the glass, 'against which part if a paper be expanded on the outside there will appear all the colours of the rainbow—that is, there will be generated the two principal colours, scarlet and blue, with all the intermediate ones which arise from the composition and diluting of these two.' But Hooke could make no use of his own observation, he attempted to substantiate from it his own theory of colours, and wrote pure nonsense in the attempt; and though his writings contain the germ of the theory, and in the light of our present knowledge it seems possible that he understood it more thoroughly than his contemporaries believed, yet his reasoning is so utterly vague and unsatisfactory that there is little ground for surprise that he convinced but few of its truth.

And then came Newton. It is claimed for him, and that with justice, that he was the true founder of the emission theory. In Descartes' hands it was a vague hypothesis. Newton deduced from it by rigid reasoning the laws of reflexion and refraction, he applied it with wondrous ingenuity to explain the colours of thin and of thick plates and the phenomena of diffraction, though in doing this he had to suppose a mechanism which he must have felt to be almost impossible; a mechanism which in time, as it was applied to explain other and more complex phenomena, became so elaborate that, in the words of Verdet, referring to a period one hundred years later, 'all that is necessary to overturn this laborious scaffolding is to look at it and try to understand it.'

But though Newton may with justice be called the founder of the emission theory, it is unjust to his memory to state that he accepted it as giving a full and satisfactory account of optics as they were known to him. When he first began his optical work he realised that facts and measurements were needed, and his object was to furnish the facts. He may have known of Hooke's theories. The copy of the 'Micrographia' now at Trinity College was in the Library while Newton was working with his prism in rooms in college, and may have been consulted by him. An early note-book of his contains quotations from it. Still there was nothing in the theories but hypotheses unsupported by facts, and these would have no charm for Newton. The hypotheses in the main are right. Light is due to wave motion in an all-pervading ether: the principle of interference, vaguely foreshadowed by Hooke ('Micrographia,' p. 66), was one which a

[1] This address was in the printer's hands when I saw Sir G. Stokes's paper on 'The Luminiferous Ether,' *Nature*, July 27. Had I known that so great a master of my subject had dealt with it so lately, my choice might have been different; under the circumstances it was too late to change.

century later was to remove the one difficulty which Newton felt. For there was one fact which Hooke's theory could not then explain, and till that explanation was given the theory must be rejected; the test was crucial, the answer was decisive.

Newton tells us repeatedly what the difficulty was. In reply to a criticism of Hooke's in 1672, he writes: 'For to me the fundamental supposition itself seems impossible, namely, that the waves or vibrations of any fluid can, like the rays of light, be propagated in straight lines without continual and very extravagant spreading and bending into the quiescent medium where they are terminated by it. I mistake if there be not both experiment and demonstration to the contrary. . . . For it seems impossible that any of those motions or pressions can be propagated in straight lines without the like spreading every way into the shadowed medium.'

Nor was there anything in the controversy with Hooke, which took place about 1675, to shake this belief. Hooke had read his paper describing his discovery of diffraction. He had announced it two years earlier, and there is no doubt in my mind that this was an original discovery, and not, as Newton seemed to imply soon after, taken from Grimaldi; but his paper does not remove the difficulty. Accordingly we find in the 'Principia' Newton's attempted proof (lib. ii. prop 42) that 'motus omnis per fluidum propagatus divergit a recto tramite in spatia immota'—a demonstration which has convinced but few and leaves the question unsolved as before.

Again, in 1690 Huygens published his great 'Traité de la Lumière,' written in 1678  Huygens had clearer views than Hooke on all he wrote; many of his demonstrations may be given now as completely satisfactory, but on the one crucial matter he was fatally weak. He, rather than Hooke, is the true founder of the undulatory theory, for he showed what it would do if it could but explain the rectilinear propagation. The reasoning of the latter part of Huygens' first chapter becomes forcible enough when viewed in the light of the principle of interference enunciated by Young, November 12, 1801, and developed, independently of Young, by Fresnel in his great memoir on 'Diffraction' in 1815; but without this aid it was not possible for Huygens's arguments to convince Newton, and hence in the 'Opticks' (2nd edit., 1717) he wrote the celebrated Query 28 :—'Are not all hypotheses erroneous in which light is supposed to consist in pressure or motion propagated through a fluid medium? If it consisted in motion propagated either in an instant or in time it would bend into the shadow. For pressure or motion cannot be propagated in a fluid in right lines beyond an obstacle which stops part of the motion, but will bend and spread every way into the quiescent medium which lies outside the shadow.' These were his last words on the subject. They prove that he could not accept the undulatory theory; they do not prove that he believed the emission theory to give the true explanation. Yet, in spite of this, I think that Newton had a clearer view of the undulatory theory than his contemporaries, and saw more fully than they did what that theory could achieve if but the one difficulty were removed.

This was Young's belief, who writes : [1]—'A more extensive examination of Newton's various writings has shown me that he was in reality the first who suggested such a theory as I shall endeavour to maintain; that his own opinions varied less from this theory than is now almost universally believed; and that a variety of arguments have been advanced as if to meet him which may be found in a nearly similar form in his own works.' I wish to call attention to this statement, and to bring into more prominent view the grounds on which it rests, to place Newton in his true position as one of the founders of the undulatory theory.

The emission theory in Newton's hands was a dynamical theory; he traced the motion of material particles under certain forces, and found their path to coincide with that of a ray of light; and in the 'Principia,' prop. xcvi., Scholium, he calls attention to the similarity between these particles and light. The particles obey the laws of reflexion and refraction; but to explain why some of the incident light was reflected and some refracted Newton had to invent his hypothesis of fits

[1] *Phil. Trans*, November 12, 1801.

of easy reflexion and transmission. These are explained in the 'Opticks,' book iii., props. xi., xii., and xiii. (1704), thus:—

'Light is propagated from luminous bodies in time, and spends about seven or eight minutes of an hour in passing from the sun to the earth.

'Every ray of light in its passage through any refracting surface is put into a certain transient constitution or state, which in the progress of the ray returns at equal intervals, and disposes the ray at each return to be easily transmitted through the next refracting surface, and between the returns to be easily reflected by it.

'*Definition.*—The return of the disposition of any ray to be reflected I will call its fit of easy reflexion, and those of the disposition to be transmitted its fits of easy transmission, and the space it passes between every return and the next return the interval of its fits.

'The reason why the surfaces of all thick transparent bodies reflect part of the light incident on them and refract the rest is that some rays at their incidence are in their fits of easy reflexion, some in their fits of easy transmission.'

Such was Newton's theory. It accounts for some or all of the observed facts; but what causes the fits? Newton, in the 'Opticks,' states that he does not inquire, he suggests, for those who wish to deal in hypotheses, that the rays of light striking the bodies set up waves in the reflecting or refracting substance which move faster than the rays and overtake them. When a ray is in that part of a vibration which conspires with its motion it easily breaks through the refracting surface—it is in a fit of easy transmission; and, conversely, when the motion of the ray and the wave are opposed, it is in a fit of easy reflexion.

But he was not always so cautious At an earlier date (1675) he sent to Oldenburg, for the Royal Society, an 'Hypothesis explaining the Properties of Light'; and we find from the journal book that 'these observations so well pleased the society that they ordered Mr. Oldenburg to desire Mr. Newton to permit them to be published.' Newton agreed, but asked that publication should be deferred till he had completed the account of some other experiments which ought to precede those he had described. This he never did, and the hypothesis was first printed in Birch's 'History of the Royal Society,' vol. iii., pp. 247, 262, 272, &c.; it is also given in Brewster's 'Life of Newton,' vol. i, App II., and in the 'Phil. Mag.,' September 1846, pp. 187–213.

'Were I,' he writes in this paper, 'to assume an hypothesis, it should be this, if propounded more generally, so as not to assume what light is further than that it is something or other capable of exciting vibrations of the ether. First, it is to be assumed that there is an ethereal medium, much of the same constitution with air, but far rarer, subtiller, and more strongly elastic. . . . In the second place, it is to be supposed that the ether is a vibrating medium, like air, only the vibrations far more swift and minute; those of air made by a man's ordinary voice succeeding at more than half a foot or a foot distance, but those of ether at a less distance than the hundred-thousandth part of an inch. And as in air the vibrations are some larger than others but yet all equally swift . . . so I suppose the ethereal vibrations differ in bigness but not in swiftness. . . . In the fourth place, therefore, I suppose that light is neither ether nor its vibrating motion, but something of a different kind propagated from lucid bodies. They that will may suppose it an aggregate of various peripatetic qualities Others may suppose it multitudes of unimaginable small and swift corpuscles of various sizes springing from shining bodies at great distances one after the other, but yet without any sensible interval of time. . . To avoid dispute and make this hypothesis general, let every man here take his fancy; only, whatever light be, I would suppose it consists of successive rays differing from one another in contingent circumstances, as bigness, force, or vigour, like as the sands on the shore . . . and, further, I would suppose it diverse from the vibrations of the ether. . . . Fifthly, it is to be supposed that light and ether mutually act upon one another.' It is from this action that reflexion and refraction come about, 'æthereal vibrations are therefore,' he continues, 'the best means by which such a subtile agent as light can shake the gross particles of solid bodies to heat them. And so, supposing that light impinging on a refracting or

reflecting ethereal superficies puts it into a vibrating motion, that physical super-ficies being by the perpetual appulse of rays always kept in a vibrating motion, and the ether therein continually expanded and compressed by turns, if a ray of light impinge on it when it is much compressed, I suppose it is then too dense and stiff to let the ray through, and so reflects it; but the rays that impinge on it at other times, when it is either expanded by the interval between two vibrations or not too much compressed and condensed, go through and are refracted. . . . And now to explain colours. I suppose that as bodies excite sounds of various tones and consequently vibrations in the air of various bignesses, so when the rays of light by impinging on the stiff refracting superficies excite vibrations in the ether, these rays excite vibrations of various bignesses . . . therefore, the ends of the capillamenta of the optic nerve which front or face the retina being such refract-ing superficies, when the rays impinge on them they must there excite these vibra-tions, which vibrations (like those of sound in a trumpet) will run along the aqueous pores or crystalline pith of the capillamenta through the optic nerves into the sensorium (which light itself cannot do), and there, I suppose, affect the sense with various colours, according to their bigness and mixture—the biggest with the strongest colours, reds and yellows; the least with the weakest, blues and violets; the middle with green; and a confusion of all with white.'

The last idea, the relation of colour to the bigness of wave-length, is put even more plainly in the 'Opticks,' Query 13 (ed. 1704) ·—'Do not several sorts of rays make vibrations of various bignesses, which according to their bignesses excite sensations of various colours . . . and, particularly, do not the most refrangible rays excite the shortest vibrations for making a sensation of deep violet; the least refrangible the largest for making a sensation of deep red?'

The whole is but a development of a reply, written in 1672, to a criticism of Hooke's on his first optical paper, in which Newton says: 'It is true that from my theory I argue the corporeity of light, but I do it without any absolute positiveness, as the word perhaps intimates, and make it at most a very plausible consequence of the doctrine and not a fundamental supposition' 'Certainly,' he continues, ' my hypothesis has a much greater affinity with his own [Hooke's] than he seems to be aware of, the vibrations of the ether being as useful and necessary in this as in his.'

Thus Newton, while in the 'Opticks' he avoided declaring himself as to the mechanism by which the fits of easy reflexion and transmission were produced, has in his earlier writings developed a theory practically identical in many respects with modern views, though without saying that he accepted it   It was an hypothesis; one difficulty remained, it would not account for the rectilinear propagation, and it must be rejected till it did.

Light is neither ether nor its vibrating motion; it is energy which, emitted from luminous bodies, is carried by wave motion in rays, and falling on a reflecting surface sets up fresh waves by which it is in part transmitted and in part reflected. Light is not material, but Newton nowhere definitely asserts that it is. He ' argues the corporeity of light, but without any absolute positiveness.' In the 'Principia,' writing of his particles, his words are: 'Harum attractionum haud multum dissimiles sunt Lucis reflexiones et refractiones;' and the Scholium con-cludes with: 'Igitur ob analogiam quæ est inter propagationem radiorum lucis et progressum corporum, visum est propositiones sequentes in usus opticos subjungere; interea de natura radiorum (utrum sint corpora necne) nihil omnino disputans, sed trajectorias corporum trajectoriis radiorum persimiles solummodo determi-nans.'[1]

No doubt Newton's immediate successors interpreted his words as meaning that he believed in the corpuscular theory, conceived, as Herschel says, by Newton, and

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[1] The reflexions and refractions of light are not very unlike these attractions Therefore, because of the analogy which exists between the propagation of rays of light and the motion of bodies, it seemed right to add the following propositions for optical purposes, not at all with any view of discussing the nature of rays (whether they are corporeal or not), but only to determine paths of particles which closely resemble the paths of rays.—*Principia*, lib. 1, sect xiv., prop. xcvi, Scholium.

called by his illustrious name.  Men learnt from the 'Principia' how to deal with the motion of small particles under definite forces.  The laws of wave motion were obscure, and till the days of Young and Fresnel there was no second Newton to explain them.  There is truth in Whewell's words ('Inductive Sciences,' ii., chap. x.): 'That propositions existed in the "Principia" which proceeded on this hypothesis was with many ground enough for adopting the doctrine.'  Young's view, already quoted, appears to me more just; and I see in Newton's hypothesis the first clear indication of the undulatory theory of light, the first statement of its fundamental laws.

·Three years later (1678) Huygens wrote his 'Traité de la Lumière,' published in 1690.  He failed to meet the main difficulty of the theory, but in other respects he developed its consequences to a most remarkable degree.  For more than a century after this there was no progress, until in 1801 the principle of interference was discovered by Young, and again independently a few years later by Fresnel, whose genius triumphed over the difficulties to which his predecessors had succumbed, and, by combining the principles of interference and transverse vibrations, established an undulatory theory as a fact, thus making Newton's theory a *vera causa*.

There is, however, a great distinction between the emission theory as Newton left it and Fresnel's undulatory theory.  The former was dynamical, though it could explain but little : the particles of light obeyed the laws of motion, like particles of matter.  The undulatory theory of Huygens and Fresnel was geometrical or kinematical : the structure of the ether was and is unknown; all that was needed was that light should be due to the rapid periodic changes of some vector property of a medium capable of transmitting transverse waves.  Fresnel, it is true, attempted to give a dynamical account of double refraction, and of the reflexion and refraction of polarised light, but the attempt was a failure ; and not the least interesting part of Mr. L. Fletcher's recent book on double refraction ('The Optical Indicatrix') is that in which he shows that Fresnel himself in the first instance arrived at his theory by purely geometrical reasoning, and only attempted at a later date to give it its dynamical form.  'If we reflect,' says Stokes,[1] 'on the state of the subject as Fresnel found it and as he left it, the wonder is, not that he failed to give a rigorous dynamical theory, but that a single mind was capable of effecting so much.'  Every student of optics should read Fresnel's great memoirs.

But the time was coming when the attempt to construct a dynamical theory of light could be made.  Navier, in 1821, gave the first mathematical theory of elasticity.  He limited himself to isotropic bodies, and worked on Boscovitch's hypothesis as to the constitution of matter.  Poisson followed on the same lines, and the next year (1822) Cauchy wrote his first memoir on elasticity.  The phenomena of light afforded a means of testing this theory of elasticity, and accordingly the first mechanical conception of the ether was that of Cauchy and Neumann, who conceived it to consist of distinct hard particles acting upon one another with forces in the line joining them, which vary as some function of the distances between the particles.  It was now possible to work out a mechanical theory of light which should be a necessary consequence of these hypotheses.  Cauchy's and the earlier theories do not represent the facts either in an elastic solid or in the ether.  At present we are not concerned with the cause of this; we must recognise it as the first attempt to explain on a mechanical basis the phenomena observed.  According to his theory in its final form, there are, in an isotropic medium, two waves which travel with velocities $\sqrt{A/\rho}$ and $\sqrt{B/\rho}$, A and B being constants and $\rho$ the density.  Adopting Cauchy's molecular hypothesis, there must be a definite relation between A and B.

A truer view of the theory of elasticity is given by Green in his paper read before the Cambridge Philosophical Society in 1837.  This theory involves the two constants, but they are independent, and to account for certain optical effects A must either vanish or be infinite.  The first supposition was, until a few years since, thought to be inconsistent with stability; the second leads to consequences which in part agree with the results of optical experiment, but which differ fatally from those

[1] 'Report on Double Refraction,' *Brit. Assoc. Report* 1862, p 254

results on other points. And so the first attempt to construct a mechanical theory of light failed. We have learnt much from it. At the death of Green the subject had advanced far beyond the point at which Fresnel left it. The causes of the failure are known, and the directions in which to look for modifications have been pointed out.

Now I believe that the effort to throw any theory into mechanical form, to conceive a model which is a concrete representation of the truth, to arrive at that which underlies our mathematical equations wherever possible, is of immense value to every student. Such a course, I am well aware, has its dangers It may be thought that we ascribe to the reality all the properties of the model, that, in the case of the ether, we look upon it as a collection of gyrostatic molecules and springs, or of pulleys and indiarubber bands, instead of viewing it from the standpoint of Maxwell, who hoped, writing of his own model, 'that by such mechanical fictions, anyone who understands the provisional and temporary character of his hypothesis will find himself helped rather than hindered in his search after the true interpretation of the phenomena.' Professor Boltzmann, in his most interesting paper on ' The Methods of Theoretical Physics,'[1] has quoted these words, and has expressed far more ably than I can hope to do the idea I wish to convey.

The elastic solid theory, then, has failed; but are we therefore without any mechanical theory of light? Are we again reduced to merely writing down our equations, and calling some quantity which appears in them the amplitude of the light vibration, and the square of that quantity the intensity of the light? Or can we take a further step? Let us inquire what the properties of the ether must be which will lead us by strict reasoning to those equations which we know represent the laws of the propagation of light.

These equations resemble in many respects those of an elastic solid; let us, then, for a moment identify the displacement in a light-wave with an actual displacement of a molecule of some medium having properties resembling that of a solid. Then this medium must have rigidity or quasi-rigidity in order that it may transmit transverse waves ; at the same time it must be incapable of transmitting normal waves, and this involves the supposition that the quantity A which appears in Green's equations must vanish or be infinite. To suppose it infinite is to recur to the incompressible solid theory ; we will assume, therefore, that it is zero. Reflexion and refraction show us that the ether in a transparent medium such as glass differs in properties from that in air. It may differ either (1) in density or effective density,[2] or (2) in rigidity or effective rigidity. The laws of double refraction and the phenomena of the scattering of light by small particles show us that the difference is, in the main, in density or effective density ; the rigidity of the ether does not greatly vary in different media. Dispersion, absorption, and anomalous dispersion all tell us that in some cases energy is absorbed from the light-vibrations by the matter through which they pass, or, to be more general, by something very intimately connected with the matter.

We do not know sufficient to say what that action must be ; we can, however, try the consequences of various hypotheses. Guided by the analogy of the motion of a solid in a fluid, let us assume that the action is proportional to the acceleration of the ether particles relative to the matter, and, further, that under certain circumstances some of the energy of the ether particles is transferred to the matter, thus setting them in vibration. If such action be assumed, the actual density of the ether may be the same in all media, the mathematical expression for the forces will lead to the same equations as those we obtain by supposing that there is a variation of density, and since it is clearly reasonable to suppose that this action between

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[1] *Phil. Mag*, July 1893.

[2] The equations of motion for a medium such as is supposed above can be written—

$\rho \times$ acceleration of ether $+ \rho' \times$ acceleration of matter $= \Sigma \text{ B} \times$ function of ether displacements, and their differential coefficients with respect to the co-ordinates $+ \Sigma \text{ B}' \times$ similar function for matter displacements

The quantity $\rho$ may be spoken of as the effective ether density, the quantities B as the effective elasticity or rigidity.

matter and ether is, in a crystal, a function of the direction of vibration, the apparent or effective density of the ether in such a body will depend on the direction of displacement.

Now these hypotheses will conduct us by strict mathematical reasoning to laws for the propagation, reflexion and refraction, double refraction and polarisation, dispersion, absorption, and anomalous dispersion and aberration of light which are in complete accordance with the most accurate experiments.

The rotatory polarisation of quartz, sugar, and other substances points to a more complicated action between the ether and matter than is contemplated above ; and, accordingly, other terms have to be introduced into the equations to account for these effects. It will be noted as a defect, and perhaps a fatal one, that the connection between electricity and light is not hinted at, but I hope to return to that point shortly.

Such a medium as I have described is afforded us by the labile ether of Lord Kelvin. It is an elastic solid or quasi-solid incapable of transmitting normal waves. The quantity A is zero, but Lord Kelvin has shown that the medium would still be stable provided its boundaries are fixed, or, which comes to the same thing, provided it extends to infinity. Such a medium would collapse if it were not held fixed at its boundaries; but if it be held fixed, and if then all points on any closed spherical surface in the medium receive a small normal displacement, so that the matter within the surface is compressed into a smaller volume, there will be no tendency either to aid or to prevent this compression, the medium in its new state will still be in equilibrium, the stresses in any portion of it which remains unaltered in shape are independent of its volume, and are functions only of the rigidity and, implicitly, of the forces which hold the boundary of the whole medium fixed.

A soap film affords in two dimensions an illustration of such a medium; the tension at any point of the film does not depend on the dimensions; we may suppose the film altered in area in any way we please—so long as it remains continuous—without changing the tension. Waves of displacement parallel to the surface of the film would not be transmitted. But such a film in consequence of its tension has an apparent rigidity for displacements normal to its surface: it can transmit transverse waves with a velocity which depends on the tension. Now the labile ether is a medium which has, in three dimensions, characteristics resembling those of the two-dimensional film. Its fundamental property is that the potential energy per unit volume, in an isotropic body, so far as it arises from a given strain, is proportional to the square of the resultant twist. In an incompressible elastic ether this potential energy depends upon the shearing strain. Given such a medium—and there is nothing impossible in its conception—the main phenomena of light follow as a necessary consequence. We have a mechanical theory by the aid of which we can explain the phenomena, we can go a few steps behind the symbols we use in our mathematical processes. Lord Kelvin, again, has shown us how such a medium might be made up of molecules having rotation in such a way that it could not be distinguished from an ordinary fluid in respect to any irrotational motion ; it would, however, resist rotational movements with a force proportional to the twist, just the force required ; the medium has no real rigidity, but only a quasi-rigidity conferred on it by its rotational motion. The actual periodic displacements of such a medium may constitute light. We may claim, then, with some confidence to have a mechanical theory of light.

But nowadays the ether has other functions to perform, and there is another theory to consider, which at present holds the field. Maxwell's equations of the electromagnetic field are practically identical with those of the quasi-labile ether. The symbols which occur can have an electromagnetic meaning; we speak of permeability and inductive capacity instead of rigidity and density, and take as our variables the electric or magnetic displacements instead of the actual displacement or the rotation.

Still such a theory is not mechanical. Electric force acts on matter charged with electricity, and the ratio of the force to the charge can be measured in mechanical units. A fundamental conception in Maxwell's theory is electric displacement,

and this is proportional to the electric force. Moreover, its convergence measures the quantity of electricity present per unit volume; but we have no certain mechanical conception of electric displacement or quantity of electricity, we have no satisfactory mechanical theory of the electromagnetic field. The first edition of the 'Electricity and Magnetism' appeared twenty years ago. In it Maxwell says: 'It must be carefully borne in mind that we have made only one step in the theory of the action of the medium. We have supposed it to be in a state of stress, but we have not in any way accounted for this stress or explained how it is maintained. This step, however, appears to me to be an important one, as it explains by the action of consecutive parts of the medium phenomena which were formerly supposed to be explicable only by direct action at a distance. I have not been able to make the next step, namely, to account by mechanical considerations for these stresses in the dielectric.' And these words are true still.

But, for all this, I think it may be useful to press the theory of the quasi-labile ether as far as it will go, and endeavour to see what the consequences must be.

The analogy between the equations of the electromagnetic field and those of an elastic solid has been discussed by many writers. In a most interesting paper on the theory of dimensions, read recently before the Physical Society, Mr. Williams has called attention to the fact that two only of these analogies have throughout a simple mechanical interpretation. These two have been developed at some length by Mr. Heaviside in his paper in the 'Electrician' for January 23, 1891. To one of them Lord Kelvin had previously called attention ('Collected Papers,' vol. iii. p. 450.)

Starting with a quasi-labile ether, then, we may suppose that $\mu$, the magnetic permeability of the medium, is $4\pi\rho$,[1] where $\rho$ is the density, and that K, the inductive capacity, is $1/4\pi B$, B being the rigidity, or the quasi-rigidity conferred by the rotation.

The kinetic energy of such a medium is $\frac{1}{2}\rho\,(\xi^2+\eta^2+\zeta^2)$, where $\xi$, $\eta$, $\zeta$ are the components of the displacement. Let us identify this with the electromagnetic energy $(a^2+\beta^2+\gamma^2)8\pi$, $a$, $\beta$, $\gamma$ being components of the magnetic force, so that $a=\dot{\xi}$, $\beta=\dot{\eta}$, $\gamma=\dot{\zeta}$. Then the components of the electric displacement, assuming them to be zero initially, are given by

$$=\frac{1}{4\pi}\left(\frac{d\zeta}{dy}-\frac{d\eta}{dz}\right),\ \&c.\ ;$$

that is, the electric displacement $\mathfrak{D}$ multiplied by $4\pi$ is equal to the rotation in the medium. Denote this by $\Omega$.

The potential energy due to the strain is

$$\tfrac{1}{2}B\Omega^2,\ \text{or}\ \tfrac{1}{2}16\pi^2 B\mathfrak{D}^2,$$

and on substituting for B this becomes

$$\frac{1}{2}\frac{4\pi}{K}\mathfrak{D}^2,$$

which is Maxwell's expression for the electrostatic energy of the field.

Thus so far, but no farther, the analogy is complete; the kinetic energy of the medium measures the magnetic energy, the potential energy measures the electrostatic energy. The stresses in the ether, however, are not those given by Maxwell's theory.

In the other form of the analogy we are to take the inductive capacity as $4\pi\rho$ and the magnetic permeability as $1/4\pi B$. The velocity measures the electric force, and the rotation the magnetic force, so that electrostatic energy is kinetic, and magnetic energy potential. Such an arrangement is not so easy to grasp as the other. Optical experiments, however, show us that in all probability it is $\rho$, and not B, which varies, while from our electrical measurements we know that K is variable and $\mu$ constant; hence this is a reason for adopting the second form.

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[1] If we adopted Mr. Heaviside's rational system of units the $4\pi$ would disappear

In either case we look upon the field as the seat of energy distributed per unit of volume according to Maxwell's law. The total energy is obtained by integration throughout the field.

Now we can transform this integral by Green's theorem to a surface integral over the boundary, together with a volume integral through the space; and the form of these integrals shows us that we may look upon the effects, dealing for the present with electrostatics only, as due to the attractions and repulsions of a certain imaginary matter distributed according to a definite law over the boundary and throughout the space. To this imaginary matter, then, in the ordinary theory we give the name of Electricity.

Thus an electrified conducting sphere, according to these analogies, is not a body charged with a quantity of something we call electricity, but a surface at which there is a discontinuity in the rotation impressed upon the medium, or in the flow across the surface; for in the conductor a viscous resistance to the motion takes the place of rigidity. No permanent strain can be set up.

From this standpoint we consider electrical force as one of the manifestations of some action between ether and matter. There are certain means by which we can strain the ether : the friction of two dissimilar materials, the chemical action in a cell are two , and when, adopting the first analogy, this straining is of such a nature as to produce a rotational twist in the ether, the bodies round are said to be electrified , the energy of the system is that which would arise from the presence over their surfaces of attracting and repelling matter, attracting or repelling according to the inverse square law. We falsely assign this energy to such attractions instead of to the strains and stresses in the ether.

Such a theory has many difficulties. It is far from being proved; perhaps I have erred in trespassing on your time with it in this crude form. The words of the French *savant*, quoted by Poincaré, will apply to it : ' I can understand all Maxwell except what he means by a charged body.' It is not, of course, the only hypothesis which might be formed to explain the facts, perhaps not even the most probable. For many points the vortex sponge theory is its superior. Still I feel confident that in time we shall come to see that the phenomena of the electro-magnetic field may be represented by some such mechanism as has been outlined, and that confidence must be my excuse for having ventured to call your attention to the subject.

# ADDRESS

TO THE

# CHEMICAL SECTION

OF THE

# BRITISH ASSOCIATION.

BY

PROFESSOR EMERSON REYNOLDS, M.D., Sc.D., F.R.S.,

PRESIDENT OF THE SECTION.

AT the Nottingham Meeting of the British Association in 1866, Dr. H. Bence Jones addressed the Section over which I have now the honour to preside on the place of Chemical Science in Medical Education. Without dwelling on this topic to-day, it is an agreeable duty to acknowledge the foresight of my predecessor as to the direction of medical progress. Twenty-seven years ago the methods of inquiry and instruction in medicine were essentially based on the formal lines of the last generation. Dr. Bence Jones saw that modern methods of research in chemistry—and in the experimental sciences generally—must profoundly influence medicine, and he urged the need of fuller training of medical students in those sciences.

The anticipated influence is now operative as a powerful factor in the general progress of medicine and medical education; but much remains to be desired in regard to the chemical portion of that education. In the later stages of it, undue importance is still attached to the knowledge of substances rather than of principles; of products instead of the broad characters of the chemical changes in which they are formed. Without this higher class of instruction it is unreasonable to expect an intelligent perception of complex physiological and pathological processes which are chemical in character, or much real appreciation of modern pharmacological research. I have little doubt, however, that the need for this fuller chemical education will soon be so strongly felt that the necessary reform will come from within a profession which has given ample proof in recent years of its zeal in the cause of scientific progress.

In our own branch of science the work of the year has been substantial in character, if almost unmarked by discoveries of popular interest. We may probably place in the latter category the measure of success which the skill of Moissan has enabled him to attain in the artificial production of the diamond form of carbon, apparently in minute crystals similar to those recognised by Koenig, Mallard, Daubrée, and by Friedel in the supposed meteorite of Cañon de Diablo in Arizona. Members of the Section will probably have the opportunity of examining some of these artificial diamonds through the courtesy of M. Moissan, who has also, at my request, been so good as to arrange for us a demonstration of the properties of the element fluorine, which he succeeded in isolating in 1887.

Not less interesting or valuable are the studies of Dr. Perkins, on electro-magnetic rotation, of Lord Rayleigh, on the relative densities of gases; of Dewar, on chemical relations at extremely low temperatures; of Clowes, on exact measurements of flame-cap indications afforded by Miner's testing lamps; of Horace Brown and Morris, on the chemistry and physiology of foliage leaves, by which they have been led to the startling conclusion that cane-sugar is the first sugar produced during the assimilation of carbon, and that starch is formed at its expense as a more stable

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reserve material for subsequent use of the plant; or of Cross, Bevan, and Beadle, on the interaction of alkali-cellulose and carbon bisulphide, in the course of which they have proved that a cellulose residue can act like an alcohol radical in the formation of thiocarbonates, and thus have added another to the authors' valuable contributions to our knowledge of members of the complex group of celluloses

But it is now an idle task for a President of this Section to attempt a slight sketch of the works of chemical philosophers even during the short space of twelve months; they are too numerous and generally too important to be lightly treated, hence we can but apply to them a paraphrase of the ancient formula—Are they not written in the books of the chronicles we term 'Jahresberichte,' 'Annales,' or 'Transactions and Abstracts,' according to our nationality?

I would, however, in this connection ask your consideration for a question relating to the utilisation of the vast stores of facts laid up—some might even say buried—in the records to which reference has just been made. The need exists, and almost daily becomes greater, for facile reference to this accumulated wealth, and of such a kind that an investigator, commencing a line of inquiry with whose previous history he is not familiar, can be certain to learn *all* the facts known on the subject up to a particular date, instead of having only the partial record to be found in even the best edited of the dictionaries now available. The best and most obvious method of attaining this end is the publication of a subject-matter index of an ideally complete character. I am glad to know that the Chemical Society of London will probably provide us in the years to come with a compilation which will doubtless aim at a high standard of value as a work of reference to memoirs, and in some degree to their contents, so far as the existing indexes of the volumes of the Society's Journal supply the information. Whether this subject-matter index is published or not, the time has certainly arrived for adopting the immediately useful course of publishing monographs, analogous to those now usual in Natural Science, which shall contain all the information gained up to a particular date in the branch of chemistry with which the author is specially familiar by reason of his own work in the subject. Such monographs should include much more than any mere compilation, and would form the best material from which a complete subject-matter index might ultimately be evolved.

My attention was forcibly drawn to the need of such special records by noting the comparatively numerous cases of re-discovery and imperfect identification of derivatives of thiourea. In my laboratory, where this substance was isolated, we naturally follow with interest all work connected with it, and therefore readily detect lapses of the kind just mentioned. But when it is remembered that the distinct derivatives of thiourea now known number considerably over six hundred substances, and that their descriptions are scattered through numerous British and foreign journals, considerable excuse can be found for workers overlooking former results. The difficulty which exists in this one small department of the science I hope shortly to remove, and trust that others may be induced to provide similar works of reference to the particular branches of chemistry with which they are personally most familiar.

When we consider the drift of investigation in recent years, it is easy to recognise a distinct reaction from extreme specialisation in the prominence now given to general physico-chemical problems, and to those broad questions concerning the relations of the elements which I would venture to group under the head of 'Comparative Chemistry.' Together these lines of inquiry afford promise of definite information about the real nature of the seventy or more entities we term 'elements,' and about the mechanism of that mysterious yet definite change in matter which we call 'chemical action.' Now and again one or other class of investigation enables us to get some glimpse beyond the known which stimulates the imaginative faculty.

For example, a curious side-light seems to be thrown on the nature of the elements by the chemico-physical discussion of the connection existing between the constitution of certain organic compounds and the colours they exhibit. Without attempting to intervene in the interesting controversy in which Armstrong and Hartly are engaged as to the nature of the connection, we may take it as an

established fact that a relation exists between the power which a dissolved chemical compound possesses of producing the colour impression within our comparatively small visual range, and the particular mode of grouping of its constituent radicals in its molecule. Further, the reality of this connection will be most freely admitted in the class of aromatic compounds ; that is, in derivatives of benzene, whose constituents are so closely linked together as to exhibit quasi-elemental persistence. If, then, the possession of what we call colour by a compound be connected with its constitution, may we not infer that 'elements' which exhibit distinct colour, such as gold and copper, in thin layers and in their soluble compounds, are at least complexes analogous to definitely decomposable substances ? This inference, while legitimate as it stands, would obviously acquire strength if we could show that anything like isomerism exists among the elements : for identity of atomic weight of any two chemically distinct elements must, by all analogy with compounds, imply dissimilarity in constitution, and, therefore, definite structure, independently of any argument derived from colour. Now, nickel and cobalt are perfectly distinct elements, as we all know, but, so far as existing evidence goes, the observed differences in their atomic weights (nickel 58·6, cobalt 58·7) are so small as to be within the range of the experimental errors to which the determinations were liable. Here, then, we seem to have the required example of something like isomerism among elements, and consequently some evidence that these substances are complexes of different orders ; but in the cases of cobalt and nickel we also know that in transparent solutions of their salts, if not in thin layers of the metals themselves, they exhibit strong and distinct colours—compare the beautiful rosy tint of cobalt sulphate with the brilliant green of the corresponding salt of nickel. Therefore, in exhibiting characteristically different colours, these substances afford us some further evidence of structural differences between the matter of which they consist, and support the conclusion to which their apparent identity in atomic weight would lead us. By means of such side-lights we may gradually acquire some idea of the nature of the elements, even if we are unable to get any clue to their origin other than such as may be found in Crookes' interesting speculations.

Again, while our knowledge of the genesis of the chemical elements is as small as astronomers possess of the origin of the heavenly bodies, much suggestive work has recently been accomplished in the attempt to apply the principle of gravitation, which simply explains the relative motions of the planets, to account for the interactions of the molecules of the elements. The first step in this direction was suggested by Mendeleef in his Royal Institution lecture (May 31, 1889), wherein he proposed to apply Newton's third law of motion to chemical molecules, regarded as systems of atoms analogous to double stars. The Rev. Dr. Haughton has followed up this idea with his well-known mathematical skill, and, in a series of papers just published, has shown that the three Newtonian laws are applicable to explain the interactions of chemical molecules, 'with this difference, that whereas the specific coefficient of gravity is the same for all bodies, independent of the particular kind of matter of which they are composed, the atoms have specific coefficients of attraction which vary with the nature of the atoms concerned.' The laws of gravitation, with this proviso, were found to apply to all the definite cases examined, and it was shown that a chemical change of combination is equivalent to a planetary catastrophe. So far the fundamental hypothesis of 'Newtonian Chemistry' has led to conclusions which are not at variance with the facts of the science, while it gives promise of help in obtaining a solution of the great problem of the nature of chemical action.

Passing from considerations of the kind to which I have just referred, permit me to occupy the rest of the time at my disposal with a short account of a line of study in what I have already termed 'comparative chemistry,' which is not only of inherent interest, but seems to give us the means of filling in some details of a hitherto rather neglected chapter in the early chemical history of this earth.

The most remarkable outcome of 'comparative chemistry' is the periodic law of the elements, which asserts that the properties of the elements are connected in the form of a periodic function with the masses of their atoms. Concurrently with the recognition of this principle, other investigations have been in progress, aiming

at more exact definitions of the characters of the relations of the elements, and ultimately of their respective offices in nature. Among inquiries of this kind the comparative study of the elements carbon and silicon appears to me to possess the highest interest. Carbon, whether combined with hydrogen, oxygen, or nitrogen, or with all three, is the great element of organic nature, while silicon, in union with oxygen and various metals, not only forms about one-third of the solid crust of the earth, but is unquestionably the most important element of inorganic nature. The chief functions of carbon are those which are performed at comparatively low temperatures ; hence carbon is essentially the element of the present epoch. On the other hand, the activities of silicon are most marked at very high temperatures ; hence it is the element whose chief work in nature was performed in the distant past, when the temperature of this earth was far beyond that at which the carbon compounds of organic life could exist. Yet between these dominant elements of widely different epochs remarkably close analogies are traceable, and the characteristic differences observed in their relations with other elements are just those which enable each to play its part effectively under the conditions which promote its greatest activity.

The chemical analogies of the two tetrad elements carbon and silicon are most easily recognised in compounds which either do not contain oxygen, or which are oxygen compounds of a very simple order, and the following table will recall a few of the most important of these, as well as some which have resulted from the fine researches of Friedel, Crafts, and Ladenburg :—

*Some Silicon Analogues of Carbon Compounds.*

| | | |
|---|---|---|
| $SiH_4$ . . . . Hydrides . . . . $CH_4$ |
| $SiCl_4$ . . . $\}$ Chlorides $\{$ . . . . $CCl_4$ |
| $Si_2Cl_6$ . . . $\}$ . . . . $C_2Cl_6$ |
| $SiO_2$ . . . . Oxides . . . . $CO_2$ |
| $H_2SiO_3$ . . . . Meta Acids . . . . $H_2CO_3$ |
| $HSiHO_2$ . . . . Formic Acids . . . . $HCHO_2$ |
| $(SiHO)_2O$ . . . . Formic Anhydrides . . . $(CHO)_2O$ ? |
| $H_2Si_2O_4$ . . . . Oxalic Acids . . . . $H_2C_2O_4$ |
| $HSi(CH_3)O_2$ . . . . Acetic Acids . . . . $HC(CH_3)O_2$ |
| $HSi(C_6H_5)O_2$ . . . Benzoic Acids . . . . $HC(C_6H_5)O_2$ |
| $SiC_8H_{19}H$ . . . . Nonyl Hydrides . . . . $C_9H_{19}H$ |
| $SiC_8H_{19}OH$ . . . . Nonyl Alcohols . . . . $C_9H_{19}OH$ |

But these silicon analogues of carbon compounds are, generally, very different from the latter in reactive power, especially in presence of oxygen and water. For example, hydride of silicon, even when pure, is very easily decomposed, and, if slightly warmed, is spontaneously inflammable in air ; whereas the analogous marsh gas does not take fire in air below a red heat. Again, the chlorides of silicon are rapidly attacked by water affording silicon hydroxides and hydrochloric acid ; but the analogous carbon chlorides are little affected by water even at comparatively high temperatures. Similarly, silicon-chloroform and water quickly produce silico-formic acid and anhydride along with hydrochloric acid, while ordinary chloroform can be kept in contact with water for a considerable time without material change.

Until recently no well-defined compounds of silicon were known including nitrogen ; but we are now acquainted with a number of significant substances of this class.

Chemists have long been familiar with the fact that a violent reaction takes place when silicon chloride and ammonia are allowed to interact. Persoz, in 1830, assumed that the resulting white powder was an addition compound, and assigned to it the formula $SiCl_4$, 6 $NH_3$, while Besson, as lately as 1892, gave $SiCl_4$, 5 $NH_3$. These formulæ only express the proportions in which ammonia reacts with the chloride under different conditions and give us no information as to the real nature of the product ; hence they are almost useless. Other chemists have, however, carefully examined the product of this reaction, but owing to peculiar difficulties

in the way have not obtained results of a very conclusive kind. It is known that the product when strongly heated in a current of ammonia gas affords ammonium chloride, which volatilises, and a residue, to which Schutzenberger and Colson have assigned the formula $Si_2N_3H$. This body they regard as a definite hydride of $Si_2N_3$, which latter they produced by acting on silicon at a white heat with pure nitrogen. Gattermann suggests that a nearer approach to the silicon analogue of cyanogen, $Si_2N_2$, should be obtained from the product of the action of ammonia on silicon-chloroform ; but it does not appear that this suggestion has yet borne fruit. It was scarcely probable that the above-mentioned rather indefinite compounds of silicon with nitrogen were the only ones of the class obtainable, since bodies including carbon combined with nitrogen are not only numerous but are among the most important carbon compounds known. Further investigation was therefore necessary in the interests of comparative chemistry, and for special reasons which will appear later on ; but it was evident that a new point of attack must be found.

A preliminary experimental survey proved the possibility of forming numerous compounds of silicon containing nitrogen, and enabled me to select those which seemed most likely to afford definite information. For much of this kind of work silicon chloride was rather too energetic, hence I had a considerable quantity of the more manageable silicon tetrabromide prepared by Serullas' method, viz. by passing the vapour of crude bromine (containing a little chlorine) over a strongly heated mixture of silica and charcoal. In purifying this product I obtained incidentally the chloro-bromide of silicon, $SiClBr_3$, which was required in order to complete the series of possible chlorobromides of silicon.[1]

Silicon bromide was found to produce addition compounds very readily with many feebly basic substances containing nitrogen. But one group of bromides of this class has yet been investigated in detail, namely, the products afforded by thioureas. The typical member of this group is the perfectly definite but uncrystalline substance

$$SiBr_2 \begin{cases} (CSN_2H_4)_4Br \\ (CSN_2H_4)_4Br \end{cases}$$

Substituted thioureas afford similar bodies, the most interesting of which is the allyl compound. This is a singularly viscid liquid, which requires several days at ordinary temperatures to regain its level, when a tube containing it is inverted. But these are essentially addition compounds, and are therefore comparatively unimportant.

In most cases, however, the silicon haloids enter into very definite reaction with nitrogen compounds, especially when the latter are distinctly basic, such as aniline or any of its homologues. One of the principal products of this class of change is the beautiful typical substance on the table, which is the first well-defined crystalline compound obtained in which silicon is exclusively combined with nitrogen. Its composition is $Si(NHC_6H_5)_4$.[2] Analogous compounds have been formed with the toluidines, naphthylamines, &c., and have been examined in considerable detail, but it suffices to mention them and proceed to point out the nature of the changes we can effect by the action of heat on the comparatively simple anilide.

When silicon anilide is heated carefully *in vacuo* it loses one molecule of aniline very easily and leaves triphenyl-guanidine, probably the *a* modification; if the action of heat be continued, but at ordinary pressure and in a current of dry hydrogen, another molecule of aniline can be expelled, and, just before the last trace of the latter is removed, the previously liquid substance solidifies and affords a silicon analogue of the insoluble modification of carbodiphenyldiimide, which may then be heated moderately without undergoing further material change. A comparison of the formulæ will make the relations of the products clear :—

Silicotetraphenylamide—$Si(NHPh)_4$
Silicotriphenylguanidine—$Si : NPh. (NHPh)_2$
Silicodiphenyldiimide—$Si : (NPh)_2$.

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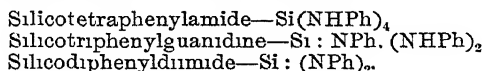
[1] Three years later Besson formed the same compound and described it as new.
[2] Harden has obtained an uncrystalline intermediate compound, $SiCl_2(NHC_6H_5)_2$.

Moreover, the diimide has been heated to full redness in a gas combustion furnace while dry hydrogen was still passed over it; even under these conditions little charring occurred, but some nitrogen and a phenyl radical were eliminated, and the purified residue was found to approximate in composition to $SiNPh$, which would represent the body as phenylsilicocyanide or a polymer of it. Even careful heating of the diimide in ammonia gas has not enabled me to remove all the phenyl from the compound, but rather to retain nitrogen, as the best residue obtained from such treatment consisted of $Si_2N_3Ph$, or the phenylic derivative of one of the substances produced by Schutzenberger and Colson from the ammonia reaction. It may be that both these substances are compounds of silicocyanogen with an imide group of the kind below indicated—

$$SiN{>}NH \quad : \quad SiN{>}NPh$$
$$SiN \qquad\qquad SiN$$
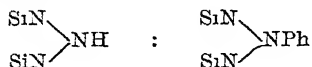
Further investigation must decide whether this is a real relationship; if it be, we should be able to remove the imidic group and obtain silicocyanogen in the free state. One other point only need be noticed, namely, that when the above silicon compounds are heated in oxygen they are slowly converted into $SiO_2$; but the last traces of nitrogen are removed with great difficulty, unless water-vapour is present, when ammonia and silica are quickly formed.

Much remains to be done in this department of comparative chemistry, but we may fairly claim to have established the fact that silicon, like carbon, can be made to form perfectly well-defined compounds in which it is exclusively united with the triad nitrogen of amidic and imidic groups.

Now, having proved the capacity of silicon for the formation of compounds of this order with a triad element, Nature very distinctively lets us understand that nitrogen is not the particular element which is best adapted to play the triad *rôle* towards silicon in its high-temperature changes, which are ultimately dominated by oxygen. We are not acquainted with any natural compounds which include silicon and nitrogen; but large numbers of the most important minerals contain the pseudo-triad element aluminum combined with silicon, and few include any other triad. Phosphorus follows silicon in the periodic system of the elements as nitrogen does carbon, but silicates containing more than traces of phosphorus are rare; on the other hand, silicates are not uncommon containing boron, the lower homologue of aluminum; for example, axinite, datholite, and tourmaline.

Moreover, it is well known that silicon dissolves freely in molten aluminum, though much of the former separates on cooling. Winkler has analysed the gangue of aluminum saturated with silicon, and found that its composition is approximately represented by the formula $SiAl$, or, perhaps, $Si_2Al_2$, if we are to regard this as analogous to $C_2N_2$ or cyanogen. Here aluminum at least resembles nitrogen in directly forming a compound with silicon at moderately high temperature. It would appear, then, that while silicon can combine with both the triads nitrogen and aluminum, the marked positive characters of the latter, and its extremely low volatility, suit it best for the production of permanent silicon compounds similar to those which nitrogen can afford.
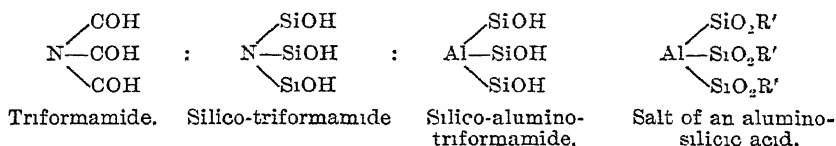
With these facts in mind we may carry our thoughts back to that period in the earth's history when our planet was at a higher temperature than the dissociation point of oxygen compounds. Under such conditions the least volatile elements were probably liquids, while silicides and carbides of various metals were formed in the fluid globe. We can imagine that the attraction of aluminum for the large excess of silicon would assert itself, and that, as the temperature fell below the point at which oxidation became possible, these silicides and carbides underwent some degree of oxidation, the carbides suffering most owing to the volatility of the oxides of carbon, while the fixity of the products of oxidation of silicides rendered the latter process a more gradual one. The oxidation of silicides of metals which had little attraction for silicon would lead to the formation of simple metallic silicates and to the separation of the large quantities of free silica we meet with in the solid crust of the earth, whereas oxidation of silicides of aluminum would not

break up the union of the two elements, but rather cause the ultimate formation of the alumino-silicates which are so abundant in most of our rocks.

Viewed in the light of the facts already cited and the inferences we have drawn from them as to the nitrogen-like relationship of aluminum to silicon, I am disposed to regard the natural alumino-silicates as products of final oxidation of sometime active silico-aluminum analogues of carbo-nitrogen compounds, rather than ordinary double salts. It is generally taken for granted that they are double salts, but recent work on the chromoxalates by E. A. Werner has shown that this view is not necessarily true of all such substances.
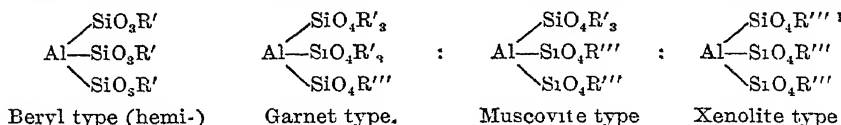
Without going into undue detail we can even form some conception of the general course of change from simple aluminum silicide to an alumino-silicate, if we allow the analogies already traced to lead us further.

We recognise the existence of silico-formyl in Friedel and Ladenburg's silico-formic anhydride; hence silico-triformamide is a compound whose probable formation we can admit, and, on the basis of our aluminum-nitrogen analogy, an aluminum representative also. Thus—

$$N\begin{cases} \diagup COH \\ {-}COH \\ \diagdown COH \end{cases} : \quad N\begin{cases} \diagup SiOH \\ {-}SiOH \\ \diagdown SiOH \end{cases} : \quad Al\begin{cases} \diagup SiOH \\ {-}SiOH \\ \diagdown SiOH \end{cases} \quad Al\begin{cases} \diagup SiO_2R' \\ {-}SiO_2R' \\ \diagdown SiO_2R' \end{cases}$$

Triformamide.    Silico-triformamide    Silico-alumino-triformamide.    Salt of an alumino-silicic acid.

Now, oxidation of triformamide would lead to complete resolution into nitrogen gas, carbondioxide gas and water rendering it an extremely unstable body; under similar conditions silico-triformamide would probably afford nitrogen gas and silicic acid (or silicon dioxide and water); while the third compound, instead of breaking up, would (owing to the fixity of aluminum as compared with nitrogen) be likely at first to afford a salt of an alumino-silicic acid, in presence of much basic material.

The frequent recurrence of the ratios $Si_3Al, Si_3Al_2$, &c., in the formulæ of natural alumino-silicates, suggests that some at least of these minerals are derived from oxidation products of the above triformic type. Without stopping to trace all the possible stages in the oxidation of the primary compound $Al(SiO_2R)_2$, or variations in basicity of the products, I may cite the four following representative examples out of many others which might be given of resulting representative mineral groups :—

$$Al\begin{cases} \diagup SiO_3R' \\ {-}SiO_3R' \\ \diagdown SiO_3R' \end{cases} \quad Al\begin{cases} \diagup SiO_4R'_3 \\ {-}SiO_4R'_3 \\ \diagdown SiO_4R''' \end{cases} : \quad Al\begin{cases} \diagup SiO_4R'_3 \\ {-}SiO_4R''' \\ \diagdown SiO_4R''' \end{cases} : \quad Al\begin{cases} \diagup SiO_4R'''^{1} \\ {-}SiO_4R''' \\ \diagdown SiO_4R''' \end{cases}$$

Beryl type (hemi-)    Garnet type.    Muscovite type    Xenolite type

Five years ago Professor F. W. Clarke, of the United States Geological Survey,' published a most interesting paper on the structure of the natural silicates. In this he adopts the view that the mineral xenolite, $Si_3Al_4O_{12}$, is the primary from which all other alumino-silicates may be supposed to arise by various substitutions. Nature, however, seems to teach us that such minerals as xenolite, fibrolite, and the related group of 'clays' are rather to be regarded as end-products of a series of hydrolytic changes of less aluminous silicates than primary substances themselves ; hence the sketch which I have ventured to give above of the probable genesis of alumino-silicates seems to provide a less arbitrary basis for Clarke's interesting work, without materially disturbing the general drift of his subsequent reasoning.

We may now consider for a moment in what direction evidence can be sought for the existence in nature of derivatives of the hypothetical intermediate products of oxidation between a primary silicide and its fully oxidised silicate.

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[1] In these cases where R''' = Al it is, of course, assumed that the latter is acting only as a basic radical.

In the absence of a working hypothesis of the kind which I have already suggested, it is not probable that direct evidence would yet be obtainable—this must be work for the future—but when we consider that the existence of compounds of the order in question would manifest themselves in ordinary mineral analyses by the analytical products exceeding the original weight of material, we seem to find some evidence on the point in recorded cases of the kind. A deficiency of a single atom of oxygen in compounds having the high molecular weights of those in question, would be indicated by very small excesses (from 2 to 3 per cent.) whose real meaning might be easily overlooked. Now, such results are not at all unusual in analyses of mineral alumino-silicates. For instance, *Amphiboles* containing a mere trace of iron have afforded 102 75 parts from 100, and almost all analyses of *Microsommite* are high, giving as much as 103 parts. In less degree *Vesuvianite* and members of the *Andalusite* group may be noted. All these cases may be capable of some other explanations, but I cite them to show that such excesses are commonly met with in published analyses. On the other hand, it is scarcely to be doubted that a good analyst, who obtained a really significant excess, would throw such a result aside as erroneous and never publish it. I therefore plead for much greater care in analyses of the kind in question, and closer scrutiny of results in the light of the suggestions I have ventured to offer. It is probable that silicates containing only partially oxidised aluminum are rare; nevertheless the search for them would introduce a new element of interest into mineralogical inquiries.

If the general considerations I have now endeavoured to lay before you are allowed their full weight, some of the alumino-silicates of our primary rocks reveal to us more than we hitherto supposed. Regarded from this newer standpoint, they are teleoxidised representatives of substances which foreshadowed in terms of silicon, aluminum, and oxygen the compounds of carbon, nitrogen, and hydrogen required at a later stage of the earth's history for living organisms. Thus, while the sedimentary strata contain remains which come down to us from the very dawn of life on this globe, the rocks from whose partial disintegration the preserving strata resulted contain mineral records which carry us still further back, even to Nature's earliest efforts in building up compounds similar to those suited for the purposes of organic development.

# ADDRESS

## TO THE

# GEOLOGICAL SECTION

### OF THE

# BRITISH ASSOCIATION,

### BY

## J. J. H. TEALL, M.A., F.R.S., Sec. G.S.,

#### PRESIDENT OF THE SECTION.

IT is a striking and remarkable fact that, although enormous progress has been made in petrographical science during the last hundred years, there has been comparatively little advance so far as broad, general theories relating to the origin of rocks are concerned. In Hutton's 'Theory of the Earth,' the outlines of which were published in 1788, the following operations are clearly recognised:—The degradation of the earth's surface by aqueous and atmospheric agencies, the deposition of the *débris* beneath the waters of the ocean; the consolidation and metamorphosis of the sedimentary deposits by the internal heat and by the injection of molten mineral matter; the disturbance and upheaval of the oceanic deposits, and, lastly, the formation of rocks by the consolidation of molten material both at the surface and in the interior of the earth.

Hutton regarded these operations as efficient causes ordained for the purpose of producing an earth adapted to sustain animal and vegetable life. His writings are saturated with the teleological philosophy of the age to which they belong, and some of his arguments strike us, therefore, as strange and inconclusive; moreover, the imperfect state of the sciences of chemistry and physics occasionally led him into serious error. Notwithstanding these imperfections, we are compelled to admit, when viewing his work in the light of modern knowledge, that we can find the traces, and sometimes far more than the traces, of those broad general theories relating to dynamical geology which are current at the present day.

If Hutton had contented himself with proving the reality of the agencies to which reference has been made, it is probable that his views would have been generally accepted. But he went much further than this, and boldly maintained that one or other of these agencies, or several combined, would account for all the phenomena with which the geologist has to deal. It was this that gave rise to the controversial fire which blazed up with such fury during the early years of this century, and whose dying embers have not yet been extinguished.

The views of Hutton were in strong contrast to those of Werner, the celebrated professor of mineralogy at Freiberg, to whom science owes a debt of gratitude as great as that due to the Scottish physician. The value of a man's work must not simply be judged by the truth of the theory which he holds. I consider that the Wernerian theory—by which I understand a reference to the early stages of planetary evolution for the purpose of explaining certain geological facts—has been on the wane from the time it was propounded down to the present day; but I claim to be second to none in my admiration for the knowledge, genius, and enthusiasm of the illustrious Saxon professor. The uniformitarian doctrines of Hutton gave a very decided character to the theoretical views of British geologists during the middle of the century, in consequence of the eloquent support of

Lyell; but of late there has been a tendency to hark back to a modified form of Wernerism. This tendency can, I think, be largely traced to the recognition of evolution as a factor in biology and physical astronomy. The discoveries in these sciences may necessitate a modification of the views held by some of the extreme advocates of uniformitarianism. This admission, however, by no means carries with it the conclusion that the methods based on the doctrine of uniformitarianism must be discarded. If I read the history of geology aright, every important advance in the theoretical interpretation of observed facts relating to physical geology has been made by the application of these methods. It does not, of course, follow that the progress in the future will be exactly along the same lines as that in the past, but, if I am right in the opinion I have expressed, it is a strong reason for adhering to the old methods until they have been proved to be inapplicable to at least some of the facts with which the physical geologist has to deal. Let us consider for a moment whether the recognition of evolution as a factor in biology and physical astronomy gives an *à priori* probability to some form of Wernerism.

The period of time represented by our fossiliferous records is perhaps equivalent to that occupied by the evolution of the vertebrata, but all the great subdivisions of the invertebrata were living in the Cambrian period, and must have been differentiated in still earlier times. Is it not probable, therefore, that the fossiliferous records at present known represent a period insignificant in comparison with that during which life has existed upon the earth? Again, is it not probable that the period during which life has existed is a still smaller fraction of that which has elapsed since the formation of the primitive crust? And if so, what *à priori* reason have we for believing that the rocks accessible to observation contain the records of the early stages of the planet's history? But the advocates of the diluted forms of Wernerism which find expression in geological writings at the present day almost invariably refer to recent speculations in cosmical physics. The views of astronomers have always had a powerful influence on those of geologists. Hutton wrote at a time when the astronomical world had been profoundly affected by Lagrange's discovery, in 1776, of the periodicity of the secular changes in the forms of the planetary orbits. The doubts as to the stability of the solar system which the recognition of these changes had inspired were thus removed, and astronomers could then see in the physical system of the universe ' no vestige of a beginning,—no prospect of an end.' Now it is otherwise. Tidal friction and the dissipation of energy by the earth and by the sun are each referred to as fixing a limit to the existing conditions. I have not the knowledge necessary to enable me to discuss these questions, and I will therefore admit, for the sake of argument, that the phenomena referred to indicate the lines along which the physical evolution of our planet has taken place; but does it follow that geologists should desert a working hypothesis which has led to brilliant results in the past for one which has been tried again and again and always found wanting?

If there were absolute unanimity amongst mathematical physicists, it might be necessary for us to reconsider our position. This, however, is not the case. After referring to the argument from tidal friction, Professor Darwin, in his address to the Mathematical and Physical Section for 1886, says:—' On the whole, then, I can neither feel the cogency of the argument from tidal friction itself, nor, accepting it, can I place any reliance on the limits which it assigns to geological history.' In reviewing the argument from the secular cooling of the earth, he points out that the possibility of the generation of heat in the interior by tidal friction has been ignored, and that the thermal data on which the calculations are based are not sufficiently complete to remove all reasonable doubt. He regards the case depending on the secular cooling of the sun as the strongest; but it is evident that, in view of undreamt-of possibilities, he would not allow it to have much weight in the face of adverse geological evidence. In conclusion he says·— ' Although speculations as to the future course of science are usually of little avail, yet it seems as likely that meteorology and geology will pass the word of command to cosmical physics as the converse. At present our knowledge of a definite limit to geological time has so little precision that we should do wrong to summarily reject any theories which appear to demand longer periods of time than those which now

appear allowable. In each branch of science hypothesis forms the nucleus for the aggregation of observation, and as long as facts are assimilated and co-ordinated we ought to follow our theory.' Now, my point is that the uniformitarian hypothesis, as applied to the rocks we can examine, has assimilated and co-ordinated so many facts in the past, and is assimilating and co-ordinating so many new discoveries, that we should continue to follow it, rather than plunge into the trackless waste of cosmogonical speculation in pursuit of what may after all prove to be a will-o'-the-wisp.

As an additional illustration of the want of agreement amongst mathematical physicists on questions relating to the earth, I may refer to certain papers by Mr. Chree.[1] This author maintains that the modern theory of elasticity points to the conclusion that if a spherical globe, composed of a nearly incompressible elastic solid of the size of the earth, were set rotating as the earth is rotating, it would take the form which the earth actually possesses. How is the question of the fixity of the earth's axis affected by Mr Chree's researches, and by the recent observations which prove a simultaneous change of latitude, in opposite directions, in Europe and at Honolulu? If geological facts point to a shifting of the position of the axis, is there any dynamical reason why they should not receive due consideration? Geologists want as much freedom as possible. We do not object to any limitations which are necessary in the interest of science, and we cordially welcome, and as a matter of fact are largely dependent upon, assistance from other departments of knowledge; but those who would help us should bear in mind that the problems we have still to solve are extremely difficult and complex, so that if certain avenues of thought are closed on insufficient grounds by arguments of the validity of which we are unable to judge, but which we are naturally disposed to take on trust, the difficulties of our task may be greatly augmented and the progress of science seriously retarded. So far as I can judge, there is no *à priori* reason why we should believe that any of the rocks we now see were formed during the earlier stages of planetary evolution. We are free to examine them in our own way, and to draw on the bank of time to any extent that may seem necessary.

For some years past the greater part of my time has been devoted to a study of the composition and structure of rocks, and it has occurred to me that I might, on the present occasion, give expression to my views on the question as to whether the present position of petrographical science necessitates any important modification in the theoretical views introduced by the uniformitarian geologists. Must we supplement the ideas of Hutton and Lyell by any reference to primordial conditions when we endeavour to realise the manner in which the rocks we can see and handle were produced? The question I propose to consider is not whether some of these rocks *may* have been formed under physical conditions different from those which now exist—life is too short to make a discussion of geological possibilities a profitable pursuit—but whether the present state of petrographical science renders uniformitarianism untenable as a working hypothesis, and, if so, to what extent. There is nothing original in what I am about to lay before you. All that I propose to do is to select from the numerous facts and more or less conflicting views, bearing on the question I have stated, a few of those which appear to me to be of considerable importance.

The sedimentary rocks contain the history of life upon the earth, and on this account, as well as on account of their extensive development at the surface, they have necessarily received an amount of attention which is out of all proportion to their importance as constituent portions of the planet. They are, after all, only skin deep. If they were totally removed from our globe its importance as a member of the solar system would not be appreciably diminished. The general laws governing the formation and deposition of these sediments have been fairly well

[1] C. Chree, 'On Some Applications of Physics and Mathematics to Geology,' *Phil. Mag.*, vol. xxxii (1891), pp 233, 342.

understood for a long time.  Hutton, as we have already seen, clearly realised that the land is always wasting away, and that the materials are accumulating on the beds of rivers, lakes, and seas.  The chemical effects of denudation are mainly seen in the breaking up of certain silicates and the separation of their constituents into those which are soluble and those which are insoluble under surface conditions. The mechanical effects are seen in the disintegration of rocks, and this may, under certain circumstances, take place without the decomposition of their component minerals.[1]  Quartz and the aluminous silicates, which enter largely into the composition of shales and clays, are two of the most important insoluble constituents.  It must be remembered, however, that felspars often possess considerable powers of resistance, and rocks which contain them may be broken up without complete or anything like complete decomposition of these minerals.  Orthoclase, microcline, and oligoclase are the varieties which most successfully resist decomposition ; and, as a natural consequence, occur most abundantly in sedimentary deposits.  It is commonly stated that when felspars are attacked the general effect is to reduce them to a fine powder, composed of a hydrated silicate of alumina, and to remove the alkalies, lime, and a portion of the silica.  But, as Dr. Sterry Hunt has so frequently urged, the removal of alkalies is imperfect, for they are almost invariably present in argillaceous deposits.  Three, four, and even five per cent., consisting mainly of potash, may frequently be found.  This alkali appears to be present in micaceous minerals, which are often produced, as very minute scales, during the decomposition of felspars.  White mica, whether formed in this way or as a product of igneous or metamorphic action, possesses great powers of resistance to the ordinary surface agencies of decomposition, and so may be used over and over again in the making of sedimentary deposits.  Brown mica is also frequently separated from granite and other rocks, and deposited as a constituent of sediments ; but it is far more liable to decomposition than the common white varieties, and its geological life is, therefore, comparatively short.[2]  Small crystals and grains of zircon, rutile, ilmenite, cyanite, and tourmaline are nearly indestructible, and occur as accessory constituents in the finer-grained sandstones.[3]  Garnet and staurolite also possess considerable powers of resistance, and are not unfrequently present in the same deposits.  If we except the last two minerals and a few others, such as epidote, the silicates containing lime, iron, and magnesia are, as a rule, decomposed by surface agencies and the bases removed in solution ; augite, enstatite, hornblende, and lime-felspars are extremely rare as constituents of ordinary sediments.

The insoluble constituents resulting from the waste of land surfaces are deposited as gravel, sand, and mud ; the soluble constituents become separated as solid bodies by evaporation of the water in inland seas and lagoons, by chemical action, and by organic life.  They are deposited as carbonates, sulphates, chlorides, and sometimes, as in the case of iron and manganese, as oxides.  The soluble silica may be deposited in the opaline condition by the action of sponges, radiolaria, and diatoms, or as sinter.

The question that we have now to consider is whether there is any marked difference between ancient and modern sediments.  One of the oldest deposits in the British Isles is the Torridon sandstone of the North-West of Scotland.  The recent discovery of *Olenellus* high up in the stratified rocks which unconformably overlie this deposit has placed its pre-Cambrian age beyond all doubt.  Now this formation is mainly composed of quartz and felspar, at least in its upper part, and the latter mineral is both abundant and very slightly altered.  One is naturally tempted, at first sight, to associate the freshness of the felspar with the great age of the rock—to assume either that the sand was formed at a time when the chemical agents of decomposition did not act with the same force as now, or that they had

[1] J W. Judd, 'Deposits of the Nile Delta,' *Proc Royal Soc* vol xxxix 1886, p. 213.

[2] 'Notes on the Probable Origin of some Slates,' by W  Maynard Hutchings, *Geol. Mag* , 1890, p 264

[3] 'Ueber das Vorkommen mikroskopischer Zirkone und Titan-Mineralien,' von Dr Hans Thurach, *Verhandl d  phys.-medic  Gesellschaft zu Wurzburg*, N F. xviii. ' On Zircons and other Minerals contained in Sand,' Allan B. Dick, *Nature*, vol. xxxvi. (1887), p  91.   See also ' Mem Geol. Survey,' *Geology of London*, vol. i. p  523.

not been in operation for a sufficient length of time to eliminate the felspar. A pure quartzose sand is probably never formed by the direct denudation of a granitic or gneissose area. The coarser sediments thus produced contain in most, if not in all, cases a considerable amount of felspar. But felspar is more liable to decomposition by percolating waters when it occurs as a constituent of grit than when present in the parent rock. Silica may thus be liberated in a soluble form, and subsequently deposited on the grains of quartz so as to give rise to secondary crystalline faces, and kaolin may be produced as beautiful six-sided tablets in the interstices of the grit. When the grit is in its turn denuded the felspar is still further reduced in amount, and a purer quartz-sand is formed. As the coarser detrital material is used over and over again, thus measuring different periods of time like the sand in an hour-glass, the felspar and other decomposable minerals are gradually eliminated. The occurrence of a large amount of fresh felspar in the Torridon sandstone might, I say, at first sight be thought to be due to the great age of the rock. Any tendency to accept a view of this kind is, however, at once checked when attention is paid to the pebbles in the coarser conglomeratic beds of the same deposit. These consist largely of quartzite—a rock formed by the consolidation of as pure a quartz-sand as any known to exist in the later formations. We are therefore led to the conclusion that the special features of the Torridon sandstone are not a function of time, but of the local conditions under which the rock was produced.

A similar conclusion may be reached by considering other types of sediment. When the stratified rocks of the different geological periods represented in any limited area are compared with each other certain marked differences may be observed, but the different types formed in any one area at different times can often be paralleled with the different types formed in different areas at the same time, and also with those now forming beneath the waters of rivers, lakes, and seas. Deep sea, shallow water, littoral and terrestrial deposits can be recognised in the formations belonging to many geological periods, from the most ancient to the most recent; and there is no evidence that any of our sedimentary rocks carry us back to a time when the physical conditions of the planet were materially different from those which now exist. After reviewing all the evidence at my disposal, I must, however, admit that the coarser as well as the finer deposits of the earlier periods appear to be more complex in composition than those of the later. The grits of the Palæozoic formations, taken as a whole, contain more felspar than the sandstones of the Mesozoic and Tertiary formations, and the slates and shales of the former contain more alkalies than the clays of the latter. This statement will hold good for the British Isles, even when allowance is made for the enormous amount of volcanic material amongst the older rocks—a phenomenon which I hold to be of purely local significance—but I strongly suspect that it will not be found to apply universally. In any case, it is not of much importance from our present point of view. All geologists will admit that denudation and deposition were taking place in pre-Cambrian times, under chemical and physical conditions very similar to, if not identical with, those of the present day.

There is, however, one general consideration of more serious import. Additions to the total amount of detrital material are now being made by the decomposition of igneous rocks, and there is no doubt that this has been going on during the whole period of time represented by our stratified deposits. It follows, therefore, as a necessary consequence that strict uniformitarianism is untenable, unless we suppose that igneous magmas are formed by the melting of sediments.

So far we have been dealing with the characters of sedimentary rocks as seen in hand-specimens rather than with those which depend on their distribution over large areas. Thanks to Delesse [1] and the officers of the 'Challenger' Expedition, [2] an attempt has now been made to construct maps on which the distribution of the sediments in course of formation at the present time is laid down. It is impossible to exaggerate the importance of such maps from a geological point of view, for on the facts which they express rests the correct interpretation of our stratigraphical

[1] *Lithologie du Fond des Mers.* Paris, 1871.
[2] *Report on Deep-sea Deposits,* 1891.

records.   Imperfect as is our knowledge of the sea-beds of former geological
periods, it is in many respects more complete than that of the sea-beds of the
present day.   The former we can often examine at our leisure, and follow from
point to point in innumerable exposures; the latter are known only from a few
soundings, often taken at great distances apart.[1]   An examination of such imper-
fect maps as we have raises many questions of great interest and importance, to
one of which I wish to direct special attention—not because it is new, but because
it is often overlooked.   The boundary lines separating the distinct types of deposit
on these maps are not, of course, chronological lines.   They do not separate sedi-
ments produced at different times, but different sediments simultaneously forming
in different places.   Now, the lines on our geological maps are usually drawn by
tracing the boundary between two distinct lithological types, and, as a natural
consequence, such lines will not always be chronological lines.   It is only when
the existing outcrop runs parallel with the margin of the original area of deposit
that this is the fact.   Consider the case of a subsiding area—or, to avoid theory,
let us say an area in which the water-level rises relatively to the land—and, for
the sake of illustration, let us suppose that the boundary separating the districts
over which sand and mud are accumulating remains parallel to the old coast-line
during the period of deposition.   This line will follow the retreating coast, so that
if, after the consolidation, emergence, and denudation of the deposits, the outcrop
happens to be oblique to the old shore. then the line on the geological map sepa-
rating clay and sand will not be of chronological value   That portion of it which
lies nearer to the position of the vanished land will represent a later period than
that which lies further away.   If such organisms as ammonites leave their remains
in the different deposits, and thus define different chronological horizons with
approximate accuracy, the imperfection of the lithological boundary as a chrono-
logical horizon will become manifest.   It is not that the geological map is wrong.
Such maps have necessarily to be constructed with reference to economic considera-
tions, and from this point of view the lithological boundaries are of paramount
importance.   They are, moreover, in many cases the only boundaries that can be
actually traced.[2]   The geological millennium will be near at hand when we can
construct maps which shall represent the distribution of the different varieties of
sediment for each of the different geological periods.   All we can say at present is
that increase of knowledge in this direction tends greatly to strengthen the uni-
formitarian hypothesis.   We can see, for example, that during Triassic times
marine conditions prevailed over a large part of what is now the great mountain-
belt of the Euro-Asiatic continent, whilst littoral and terrestrial conditions existed
in the north of Europe ; and we can catch glimpses of the onward sweep of the
sedimentary zones during the great Cretaceous transgression, culminating in the
widespread deep-sea[3] conditions under which the Chalk was deposited.

We turn now to the igneous rocks.   It is no part of my purpose to treat in
detail of the growth of knowledge from an historical point of view and to attempt
to allot to each observer the credit due to him ; but there is one name that I
desire to mention in this connection, because it is that of a man who clearly
proved the essential identity of ancient and modern volcanic rocks by the appli-
cation of precise petrographical methods at a time when there was a very general
belief that the Tertiary and pre-Tertiary rocks were radically distinct.   I need
hardly say that I refer to Mr. Samuel Allport.[4]   He wrote at a time when ob-

[1] Suess, *Das Antlitz der Erde*, Bd. II., s. 267
[2] See S S Buckman, ' On the Cotteswold, Midford, and Yeovil Sands,' *Quart.
Journ. Geol. Soc* , vol. xlv (1889), p 440 ; and the same author, ' On the So-called
Upper Lias Clay of Down Cliffs,' *Quart Journ Geol Soc* , vol xlvi (1890), p 518.
Also J. Starkie Gardner, ' On the Relative Ages of the American and the English
Cretaceous and Eocene Series,' *Geol Mag* , 1884, p. 492.
[3] Theodor Fuchs, ' Welche Ablagerungen haben wir als Tiefseebildungen zu
betrachten ? ' *Neues Jahrbuch f Miner* , &c , Beilage, Band II., p. 487.
[4] ' Tertiary and Palæozoic Trap-rocks,' *Geol Mag* , 1873, p 196   ' British Car-
boniferous Dolerites,' *Quart. Journ Geol. Soc* , vol xxx (1874), p 529 ; ' Ancient
Devitrified Pitchstones,' &c , *Quart. Journ. Geol. Soc.*, vol xxxiii. (1877), p. 449.

servers in this country had to prepare their own sections, and those who, like myself, have had the privilege of examining many of his slides scarcely know which to admire most—the skill and patience of which they are the evidence, or the conciseness and accuracy of his petrographical descriptions. His papers do not occupy a large number of pages, but they are based on an amount of observation which is truly surprising. The general conclusions at which he arrived as to the essential identity of ancient and modern igneous rocks are expressed with the utmost confidence, and one feels, after going over his material, that this confidence was thoroughly justified. It is curious now to note that the one British champion of the distinctness of the Tertiary and pre-Tertiary rocks pointed to the difference between the Antrim and Limerick traps. These traps differ in exactly the same way as do the corresponding Tertiary and pre-Tertiary continental rocks, with this important difference. On the Continent the ophitic structure is characteristic of the pre-Tertiary rocks, whereas in the north of Ireland it is a marked feature of those of Tertiary age. We see, therefore, that the arguments for the distinctness of the two sets of rocks derived from the two areas, based in both cases on perfectly accurate observations, neutralise each other, and the case hopelessly breaks down as regards the basalts and dolerites.

In this country it is now generally recognised that, when allowance is made for alterations which are necessarily more marked in the earlier than in the later rocks, there is no important difference either in structure or composition between the rhyolites, andesites, and basalts of the Palæozoic and Tertiary periods. But identity of structure and composition may in this case be taken to imply identity as to the physical conditions under which the rocks were produced. We are thus led to picture in our minds long lines of volcanoes fringing the borders of Palæozoic continents and rising as islands in the Palæozoic seas. Then, as now, there issued from the craters of these volcanoes enormous masses of fragmental material, a large portion of which was blown to dust by the explosive escape of steam and other gases from the midst of molten rock; and then, as now, there issued from fissures on their flanks vast masses of lava which consolidated as rhyolite, andesite, and basalt. We may sum up the case as regards the volcanic rocks by saying that, so long as observations are confined to a limited area, doubts may arise as to the truth of the uniformitarian view, but these doubts gradually fade away as the area of observation is extended. There are still some outstanding difficulties, such as the apparent absence of leucite lavas amongst the Palæozoic formations; but as many similar difficulties have been overcome in the past, it is improbable that those which remain are of a very formidable character.

So far we have been referring to rocks formed at the surface of the earth under conditions similar to those now in operation. But there are others, such as granite, gneiss, and mica-schist, which are obviously unlike any of the products of surface agencies. If these rocks are forming now, it must be beneath the surface. This point was clearly realised by Hutton. Granite was proved by him to be an igneous rock of subterranean origin. His conclusions as to the formation of the schists are expressed in a passage so remarkable when viewed in connection with what I regard as the tendency of modern research that I make no apology for quoting it at length. ' If, in examining our land, we shall find a mass of matter which had been evidently formed originally in the ordinary manner of stratification, but which is now extremely distorted in its structure and displaced in its position,—which is also extremely consolidated in its mass, and variously changed in its composition,—which therefore has the marks of its original or marine composition extremely obliterated, and many subsequent veins of melted mineral matter interjected; we should then have reason to suppose that here were masses of matter which, though not different in their origin from those that are gradually deposited at the bottom of the ocean, have been more acted upon by subterranean heat and the expanding power, that is to say, have been changed in a greater degree by the operations of the mineral region. If this conclusion shall be thought reasonable, then here is an explanation of all the peculiar appearances of the Alpine schistus masses of our land, those parts which have been erroneously con-

sidered as primitive in the constitution of the earth.'[1]  Surely it is not claiming too much for our author to say that we have there, sketched in broad outline, the theories of thermal and dynamic metamorphism which are attracting so much attention at the present day.

The hypogene origin of the normal plutonic rocks and their formation at different periods, even as late as the Tertiary, are facts which are now so generally recognised that we may leave these rocks without further comment and pass on to the consideration of the crystalline schists.

Everyone knows that the statement, 'He who runs may read,' is untrue when the stratigraphical interpretation of an intensely folded and faulted district is concerned.  The complexity produced by the earth-movements in such regions can only be unravelled by detailed work after definite palæontological and lithological horizons have been established.  But if the statement be untrue when applied to districts composed of ordinary stratified rocks, still less can it be true of regions of crystalline schist where the movements have often been much more intense; where the original characters of the rocks have been profoundly modified; and where all distinct traces of fossils have in most cases been obliterated.  If detailed work like that of Professor Lapworth at Dobb's Linn was required to solve the stratigraphical difficulties of the Southern Uplands, is it not probable that even more detailed work will be required to solve the structural problems of such a district as the Highlands of Scotland, where the earth-stresses, though somewhat similar, have operated with greater intensity, and where the injection of molten mineral matter has taken place more than once both on a large and on a small scale?  With these few general remarks by way of introduction, I will now call attention to what appear to me to be the most promising lines of investigation in this department of geology.

The crystalline schists certainly do not form a natural group.  Some are undoubtedly plutonic igneous rocks showing original fluxion; others are igneous rocks which have been deformed by earth-stresses subsequent to consolidation; others, again, are sedimentary rocks metamorphosed by dynamic and thermal agencies, and more or less injected with 'molten mineral matter'; and lastly, some cannot be classified with certainty under any of these heads.  So much being granted, it is obvious that we must deal with this petrographical complex by separating from it those rocks about the origin of which there can be no reasonable doubt.  Until this separation has been effected, it is quite impossible to discuss with profit the question as to whether any portions of the primitive crust remain.  In order to carry out this work it is necessary to establish some criterion by which the rocks of igneous may be separated from those of sedimentary origin.  Such a criterion may, I think, be found, at any rate in many cases, by combining chemical with field evidence.[2]  If associated rocks possess the composition of grits, sandstones, shales and limestones, and contain also traces of stratification, it seems perfectly justifiable to conclude that they must have been originally formed by processes of denudation and deposition.  That we have such rocks in the Alps and in the Central Highlands of Scotland, to mention only two localities, will be admitted by all who are familiar with those regions.  Again, if the associated rocks possess the composition of igneous products, it seems equally reasonable to conclude that they are of igneous origin.  Such a series we find in the North-West of Scotland, in the Malvern Hills, and at the Lizard.  In applying the test of chemical composition it is very necessary to remember that it must be based, not on a comparison of individual specimens, but of groups of specimens.  A granite and an arkose, a granitic gneiss and a gneiss formed by the metamorphosis of a grit, may agree in chemical and even in mineralogical composition.  The chemical test would therefore utterly fail if employed for the purpose of discriminating between these rocks.  But when we introduce the principle of paragenesis it enables us as in many cases to distinguish between them.  The granitic gneiss will be associated with rocks having the composition of diorites, gabbros, and peridotites; the sedimentary gneiss with rocks

[1] *Theory of the Earth*, vol. i. p 375
[2] H Rosenbusch, 'Zur Auffassung der chemischen Natur des Grundgebirges,' *Min. und petro. Mitth*, xii. (1891), p. 49.

answering to sandstones, shales, and limestones. Apply this test to the gneisses of Scotland, and I believe it will be found in many cases to furnish a solution of the problem. Caution, however, is necessary; for crystal-building and the formation of segregation veins and patches in the sedimentary schists clearly prove that a migration of constituents takes place under certain circumstances.

Recent work on the gneisses and schists of igneous composition has shown that the parallel structure, by no means invariably present, is sometimes the result of fluxion during the final stages of consolidation, and sometimes due to the plastic deformation of solid rocks. When compared with masses of ordinary plutonic rock, the principal points of difference, apart from those due to secondary dynamic causes, depend on what may be called their extreme petrographical differentiation. Indications of differentiation may, however, be seen in the contemporaneous veins and basic patches so common in ordinary irruptive bosses, but they are never so marked as in gneissic regions, like those of the North-West of Scotland, where specimens answering in composition to granites, diorites, and even peridotites, may be collected repeatedly in very limited areas. The nearest approach to the conditions of gneissose regions is to be found in connected masses of diverse plutonic rocks, such as those which are sometimes found on the borders of great granitic intrusions.

The tectonic relations of those gneisses which resemble igneous rocks in composition fully bear out the plutonic theory as to their origin. Thus, the intrusive character of granitic gneiss in a portion of the Himalayas has been demonstrated by General McMahon.[1] The protogine of Mont Blanc has been investigated by M. Lévy[2] with the same result. Most significant of all are the discoveries in the vast Archæan region of Canada. Professor Lawson[3] has shown that immense areas of the so-called Laurentian gneiss in the district north-west of Lake Superior are intrusive in the surrounding rocks, and therefore newer, not older, than these. Professor Adams[4] has quite recently established a similar fact as regards the anorthosite rocks—the so-called Norian—of the Saguenay River and other districts lying near the eastern margin of the 'Canadian shield.' Now that the intrusive character of so many gneisses is being recognised, one wonders where the tide of discovery will stop. How long will it be before the existence of gneisses of Tertiary age will be generally admitted? At any rate, the discoveries of recent years have compelled the followers of Wernerian methods to evacuate large slices of territory.

Turning now to the gneisses and schists which resemble sedimentary rocks in composition, we note that the parallel structure may be due to original stratification, to subsequent deformation, or to both of these agencies combined. It must also be remembered that they have often been injected with igneous material, as Hutton pointed out. Where this has followed parallel planes of weakness, we have a banding due to alternations of igneous and sedimentary material. This injection *lit par lit* has been shown by M. Lévy to be a potent cause in the formation of certain banded gneisses.

Will the various agencies to which reference has been made explain all the phenomena of the crystalline schists and gneisses? I do not think that the present state of our knowledge justifies us in answering this question in the affirmative? Those who are working on these rocks frequently have brought under their notice specimens about the origin of which they are not able to speak with any degree of confidence. Sometimes a flood of light is suddenly thrown on a group of doubtful rocks by the recognition of a character which gives unmistakable indications of their mode of origin. Thus, some of the fine-grained quartz-felspathic rocks associated with the crystalline schists of the Central Highlands are proved to have

[1] 'The Geology of Dalhousie,' *Records of Geol. Survey of India*, vol. xv. part 1 (1882), p 34. See also vol xvi. part 3 (1883). p 129

[2] 'Les Roches Crystallines et Eruptives des Environs du Mont-Blanc,' *Bull. des Services de la Carte Géologique de la France*, No 9 (1890).

[3] 'On the Geology of the Rainy Lake Region,' *Annua Report Geol. Survey of Canada* for 1887

[4] 'Ueber das Norian oder Ober-Laurentian von Canada,' *Neues Jahrbuch f. Mineralogie*, &c , Beilage, Band viii p. 419.

been originally sands like those of Hampstead Heath by the presence in them of narrow bands rich in zircon, rutile, and the other heavy minerals which are so constantly present in the finer-grained arenaceous deposits of all ages. Such pleasant surprises as the recognition of a character like this increase our confidence in the theory which endeavours to explain the past by reference to the present, and refuses to admit the necessity of believing in the existence of rocks formed under physical conditions different from those which now prevail simply because there are some whose origin is still involved in mystery.

A crystalline schist has been aptly compared to a palimpsest. Historical records of priceless value have often been obscured by the superposition of later writings; so it is with the records of the rocks. In the case of the schists, the original characters have been so modified by folding, faulting, deformation, crystallisation, and segregation that they have often become unrecognisable. But when the associated rocks have the composition of sediments we need have no hesitation in attributing the banded structure in some way to stratification, provided we clearly recognise that the order of succession and the relative thicknesses of the original beds cannot be ascertained by applying the principles which are valid in comparatively undisturbed regions

In studying the crystalline schists nothing, perhaps, strikes one more forcibly than the evidence of crystal-building in solid rocks. Chiastolite, staurolite, andalusite, garnet, albite, cordierite, micas of various kinds, and many other minerals have clearly been developed without anything like fusion having taken place. Traces of previous movements may not unfrequently be found in the arrangement of the inclusions, while the minerals themselves show no signs of deformation. Facts of this kind, when they occur, clearly indicate that the crystallisation was subsequent to the mechanical action. Nevertheless, it is probable that both phenomena were closely related, though not in all cases as cause and effect. The intrusion of large masses of plutonic rock often marks the close of a period of folding. This is well illustrated by the relation of granite to the surrounding rocks in the Lake District, the Southern Uplands of Scotland, and the West of England. Those of the two first-mentioned localities are post-Silurian and pre-Carboniferous, those of the last-mentioned locality are post-Carboniferous and pre-Permian : one set followed the Caledonian [1] folding, the other set followed the Hercynian folding. That the intrusion of these granites was subsequent to the main movements which produced the folding and cleavage is proved by the fact that the mechanical structures may often be recognised in the crystalline contact-rocks, although the individual minerals have not been strained or broken. In many other respects the rocks produced by so-called contact-metamorphism resemble those found in certain areas of crystalline schist. Many of the most characteristic minerals are common to the two sets of rocks, and so also are many structures. The cipolins and associated rocks of schistose regions have many points of resemblance to the crystalline limestones and 'kalksilicathornfels' produced by contact-metamorphism.[2]

These facts make it highly probable that, by studying the metamorphic action surrounding plutonic masses, we may gain an insight into the causes which have produced the crystalline schists of sedimentary origin ; just as, by studying the intrusive masses themselves and noting the tendency to petrographical differentiation, especially at the margins, we may gain an insight into the causes which have produced the gneisses of igneous origin.[3] In the districts to which reference has been made the igneous material came from below into a region where the rocks had been rendered tolerably rigid. Differential movement was not taking place in these rocks when the intrusion occurred. Consider what must happen if the folding stresses operate on the zone separating the sedimentary rocks from the

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[1] This term is employed in the sense in which it is used by Suess and Bertrand.

[2] H. Rosenbusch, 'Zur Auffassung des Grundgebirges,' *Neues Jahr. f. Miner.*, Bd. II. 1889, p 8

[3] G. Barrow, 'On an Intrusion of Muscovite-biotite-gneiss in the South-Eastern Highlands of Scotland, &c.,' *Quart. Journ. Geol. Soc.*, vol. xlix. (1893), p 330.

underlying source of igneous material.  Intrusion must then take place during
interstitial movement, fluxion structures will be produced in the more or less
differentiated igneous magmas, the sediments will be injected and impregnated
with igneous material, and thermo-metamorphism will be produced on a regional
scale.    The origin of gneisses and schists, in my opinion, is to be sought for in a
combination of the thermal and dynamic agencies which may be reasonably
supposed to operate in the deeper zones of the earth's crust.  If this view be
correct, it is not improbable that we may have crystalline schists and gneisses of
post-Silurian age in the North-West of Europe formed during the Caledonian fold-
ing, others in Central Europe of post-Devonian age due to the Hercynian folding,
and yet others in Southern Europe of post-Cretaceous age produced in connection
with the Alpine folding [1]   But if the existence of such schists should ultimately
be established it will still probably remain true that rocks of this character are in
most cases of pre-Cambrian age.   May not this be due to the fact, suggested by a
consideration of the biological evidence, that the time covered by our fossiliferous
records is but a small fraction of that during which the present physical conditions
have remained practically constant ?
     The good old British ship 'Uniformity,' built by Hutton and refitted by Lyell,
has won so many glorious victories in the past, and appears still to be in such
excellent fighting trim, that I see no reason why she should haul down her colours
either to 'Catastrophe' or 'Evolution.'  Instead, therefore, of acceding to the
request to 'hurry up' we make a demand for more time.   The early stages of the
planet's history may form a legitimate subject for the speculations of mathematical
physicists, but there seems good reason to believe that they lie beyond the ken of
those geologists who concern themselves only with the records of the rocks.
     In this address I have ventured to express my views on certain disputed
theoretical questions, and  I must not conclude without a word of caution.   The
fact is, I attach very little importance to my own opinions, at least on doubtful
questions connected with the origin of the crystalline schists, but, as you have
done me the honour to accept me as your President, I thought you might like to
know my present attitude of mind towards some of the unsolved problems of
geology.   There is still room for legitimate difference of opinion on many of the
subjects to which I have referred.  Meanwhile, we cannot do better than remember
the words with which one of our great living masters recently concluded an article
on a controversial subject: 'Let us continue our work and remain friends.'

     [1] Some geologists maintain that this is the case, others deny it    See H Reusch,
'Die fossilienfuhrenden krystallinischen Schiefer von Bergen in Norwegen,' Leipzig,
1883 ; J. Lehmann, 'Uebei die Entstehung der altkrystallinischen Schiefergesteine,
mit besonderer Bezugnahme auf des sachsische Granulitgebirge, Erzgebirge, Fich-
telgebirge, und bairisch-bohmische Grenzgebirge,' Bonn, 1884 ;  T  G   Bonney,
several papers on the Alps, and especially ' On the Crystalline Schists and their
Relation to the Mesozoic Rocks of the Lepontine Alps,' *Quart. Journ Geol Soc*,
vol. xlvi (1890), p. 188 , A Heim, contribution to the discussion on the last paper;
C. W. Gumbel, ' Geognostische Beschreibung des K. Bayern' and ' Grundzuge der
Geologie,' Kassel, 1888–1892.
     Although it is convenient to speak of the three types of folding which have so
largely influenced the structure of the European continent as if each belonged to a
definite period, it is important to remember that this is not strictly true    The
movements were prolonged ; they probably crept slowly over the surface of the
lithosphere, as did the zones of sedimentation, so that those of the same type are
not in all places strictly contemporaneous

# ADDRESS

TO THE

# BIOLOGICAL SECTION

OF THE

# BRITISH ASSOCIATION,

BY

## Rev. H. B. TRISTRAM, M.A., LL.D., D D., F.R.S.,

PRESIDENT OF THE SECTION

IT is difficult for the mind to grasp the advance in biological science (I use the term biology in its wide etymological, not its recently restricted sense) which has taken place since I first attended the meetings of the British Association, some forty years ago. In those days, the now familiar expressions of 'natural selection,' 'isolation,' 'the struggle for existence,' 'the survival of the fittest,' were unheard of and unknown, though many an observer was busied in culling the facts which were being poured into the lap of the philosopher who should mould the first great epoch in natural science since the days of Linnæus.

It is to the importance and value of field observation that I would venture in the first place to direct your attention.

My predecessors in this chair have been, of recent years, distinguished men who have searched deeply into the abstrusest mysteries of physiology. Thither I do not presume to follow them. I rather come before you as a survivor of the old-world naturalist, as one whose researches have been, not in the laboratory or with the microscope, but on the wide desert, the mountain side, and the isles of the sea.

This year is the centenary of the death of Gilbert White, whom we may look upon as the father of field naturalists. It is true that Sir T. Browne, Willughby, and Ray had each, in the middle of the seventeenth century, committed various observations to print; but though Willughby, at least, recognised the importance of the soft parts in affording a key to classification, as well as the osteology, as may be seen from his observation of the peculiar formations, in the Divers (*Colymbidæ*) of the tibia, with its prolonged procnemial process, of which he has given a figure, or his description of the elongation of the posterior branches of the woodpecker's tongue, as well as by his careful description of the intestines of all specimens which came under his notice in the flesh, none of these systematically noted the habits of birds, apart from an occasional mention of their nidification, and very rarely do they even describe the eggs. But White was the first observer to recognise how much may be learnt from the life habits of birds. He is generally content with recording his observations, leaving to others to speculate. Fond of Virgilian quotations (he was a fellow of Oriel of the last century), his quotations are often made with a view to prove the scrupulous accuracy of the Roman poet, as tested by his (White's) own observations.

In an age, incredulous as to that which appears to break the uniformity of nature, but quick to recognise all the phenomena of life, a contrast arises before the mind's eye between the abiding strength of the objective method, which brings Gilbert White in touch with the great writers whose works are for all time, and the transient feebleness of the modern introspective philosophies, vexed with the problems of psychology. The modern psychologist propounds his theory of man

and the universe, and we read him, and go on our way, and straightway forget. Herodotus and Thucydides tell a plain tale in plain language, or the Curate of Selborne shows us the hawk on the wing, or the snake in the grass, as he saw them day by day, and, somehow, the simple story lives and moves him who reads it long after the subtleties of this or that philosophical theory have had their day and passed into the limbo of oblivion. But, invaluable as has been the example of Gilbert White in teaching us how to observe, his field was a very narrow one, circumscribed for the most part by the boundaries of a single parish, and on the subject of geographical distribution (as we know it now) he could contribute nothing, a subject on which even the best explorers of that day were strangely inobservant and inexact. A century and a half ago, it had not come to be recognised that distribution is, along, of course, with morphology and physiology, a most important factor in determining the facts of biology. It is difficult to estimate what might have been gained in the case of many species, now irreparably lost, had Forster and the other companions of Captain Cook, to say nothing of many previous voyagers, had the slightest conception of the importance of noting the exact locality of each specimen they collected. They seem scarcely to have recognised the specific distinctions of the characteristic genera of the Pacific Islands at all, or, if they did, to have dismissed them with the remark, ' On this island was found a flycatcher, a pigeon, or a parrot similar to those found in New Holland, but with white tail-feathers instead of black, an orange instead of a scarlet breast, or red shoulders instead of yellow.' As we turn over the pages of Latham or Shaw, how often do we find for locality one of the islands of the South Sea, and, even where the locality is given, subsequent research has proved it erroneous, as though the specimens had been subsequently ticketed ; Le Vaillant described many of his South African birds from memory. Thus Latham, after describing very accurately *Rhipidura flabellifera*, from the south island of New Zealand, remarks, apparently on Forster's authority, that it is subject to variation ; that in the island of Tanna another was met with, with a different tail, etc., and that there was another variety in the collection of Sir Joseph Banks. Endless perplexity has been caused by the *Psittacus pygmæus* of Gmelin (of which Latham's type is at Vienna) being stated in the inventory as from Botany Bay, by Latham from Otaheite, and in his book as inhabiting several of the islands of the South Seas, and now it proves to be the female *Psittacus palmarum* from the New Hebrides. These are but samples of the confusion caused by the inaccuracies of the old voyagers. Had there been in the first crew who landed on the Island of Bourbon, I will not say a naturalist, but even a simple-hearted Leguat, to tell the artless tale of what he saw, or had there been among the Portuguese discoverers of Mauritius one who could note and describe the habits of its birds with the accuracy with which a Poulton could record the ways and doings of our Lepidoptera, how vastly would our knowledge of a perished fauna have been enriched ! It is only since we learned from Darwin and Wallace the power of isolation in the differentiation of species, that special attention has been paid to the peculiarities of insular forms. Here the field naturalist comes in as the helpful servant of the philosopher and the systematist, by illustrating the operation of isolation in the differentiation of species. I may take the typical examples of two groups of oceanic islands, differing as widely as possible in their position on the globe, the Sandwich Islands in the centre of the Pacific, thousands of miles from the nearest Continent, and the Canaries, within sight of the African coast; but agreeing in this, that both are truly oceanic groups, of purely volcanic origin, the ocean depths close to the Canaries, and between the different islands, varying from 1,500 to 2,000 fathoms. In the one we may study the expiring relics of an avifauna completely differentiated by isolation , in the other we have the opportunity of tracing the incipient stages of the same process.

The Sandwich Islands have long been known as possessing an avifauna not surpassed in interesting peculiarity by that of New Zealand or Madagascar ; in fact, it seems as though their vast distance from the continent had intensified the influences of isolation. There is scarcely a passerine bird in its indigenous fauna which can be referred to any genus known elsewhere. But, until the very recent

researches of Mr. Scott Wilson, and the explorations of the Honourable W. Roth-schild's collectors, it was not known that almost every island of the group possessed one or more representatives of each of these peculiar genera. Thus, every island which has been thoroughly explored, and in which any extent of the primeval forest remains, possesses, or has possessed, its own peculiar species of *Hemignathus, Himatione, Phæornis, Acrulocercus, Loxops, Drepanis*, as well as of the massive-beaked finches, which emulate the *Geospiza* of the Galapagos. Professor Newton has shown that while the greater number of these are, probably, of American origin, yet the South Pacific has contributed its quota to this museum of ornithological rarities, which Mr. Clarke very justly proposes to make a distinct biological sub-region.

That each of the islands of this group, however small, should possess a flora specifically distinct, suggests thoughts of the vast periods occupied in their differentiation.

In the Canary Islands, either because they are geologically more recent, or because of their proximity to the African coast, which has facilitated frequent immigrations from the continent, the process of differentiation is only partially accomplished Yet there is scarcely a resident species which is not more or less modified, and this modification is yet further advanced in the westernmost islands than in those nearest to Africa In Fuertaventura and Lanzarote, waterless and treeless, there is little change, and the fauna is almost identical with that of the neighbouring Sahara. There is a whin-chat, *Pratincola dacotiæ*, discovered by my companion, Mr. Meade-Waldo, peculiar to Fuertaventura, which may possibly be found on the opposite coast, though it has not yet been met with by any collectors there. Now, our whin-chat is a common winter visitant all down the West African coast, and it seems probable that isolation has produced the very marked characters of the Canaries form, while the continental individuals have been restrained from variation by their frequent association with their migratory relations. A similar cause may explain why the blackbird, an extremely common resident in all the Canary Islands, has not been modified in the least, since many migratory individuals of the same species sojourn every winter in the islands. Or take the blue titmouse. Our familiar resident is replaced along the coast of North Africa by a representative species, *Parus ultramarinus*, differentiated chiefly by a black instead of a blue cap, and a slate-coloured instead of a green back. The titmouse of Lanzarote and Fuertaventura is barely separable from that of Algeria, but is much smaller and paler, probably owing to scarcity of food and a dry desert climate Passing, 100 miles further to sea, to Grand Canary, we find in the woods and forests a bird in all respects similar to the Algerian in colour and dimensions, with one exception—the greater wing coverts of the Algerian are tipped with white, forming a broad bar when the wing is closed. This, present in the Fuertaventura form, is represented in the Canarian by the faintest white tips, and in the birds from the next islands, Tenerife and Gomera, this is altogether absent. This form has been recognised as *Parus teneriffæ*. Proceeding to the north-west outermost island, Palma, we find a very distinct species, with different proportions, a longer tail, and white abdomen instead of yellow. In the Ultima Thule, Hierro, we find a second very distinct species, resembling that of Tenerife in the absence of the wing bar and in all other respects, except that the back is green like the European, instead of slate as in all the other species. Thus we find in this group a uniform graduation of variation as we proceed further from the cradle of the race.

A similar series of modifications may be traced in the chaffinch (*Fringilla*), which has been in like manner derived from the North African *F. spodiogena*, and in which the extreme variation is to be found in the westernmost islands of Palma and Hierro. The willow wren (*Phylloscopus trochilus*), extremely numerous and *resident*, has entirely changed its habits, though not its plumage, and I have felt justified in distinguishing it as *Ph. fortunatus*. In note and habits it is entirely different from our bird, and though it builds a domed nest it is always near the top of lofty trees, most frequently in palm-trees. The only external difference from our bird consists in its paler tarsi and more rounded wing, so that its power of flight is weaker, but, were it not for the marked difference in its habits and voice,

I should have hesitated to differentiate it. In the kestrel and the great spotted woodpecker there are differences which suggest incipient species, while the forests of the wooded western islands yield two very peculiar pigeons, differing entirely from each other in their habits, both probably derived from our wood-pigeon, but even further removed from it than the *Columba trocaz* of Madeira, and, by their dark chestnut coloration, suggesting that peculiar food, in this case the berries of the tree laurel, has its full share in the differentiation of isolated forms. If we remember the variability of the pigments in the food of birds, and the amount absorbed and transferred to the skin and plumage, the variability in the tints and patterns of many animals can be more readily understood.

One other bird deserves notice, the *Caccabis*, or red-legged partridge, for here, and here alone, we have chronological data. The Spaniards introduced *Caccabis rufa* into Canary, and *C petrosa* into Tenerife and Gomera, and they have never spread from their respective localities. Now, both species, after a residence of only 400 years, have become distinctly modified. *C. rufa* was introduced into the Azores also, and changed exactly in the same manner, so much so that Mr Godman, some years ago, would have described it as distinct, but that the only specimen he procured was in moult and mutilated, and his specimen proves identical with the Canarian bird. Besides minor differences, the back is one-fourth stouter and longer than in the European bird, and the tarsus very much stouter and longer, and the back is grey rather than russet. The grey back harmonises with the volcanic dark soil of the rocks of the Canaries, as the russet does with the clay of the plains of England and France. In the Canaries the bird lives under different conditions from those of Europe. It is on the mountain sides and among rocks that the stouter beak and stronger legs are indispensable to its vigorous existence. It is needless to go into the details of many other species. We have here the effect of changed conditions of life in 400 years. What may they not have been in 400 centuries? We have the result of peculiar food in the pigeons, and of isolation in all the cases I have mentioned Such facts can only be supplied to the generaliser and the systematist through the accurate and minute observations of the field naturalist.

The character of the avifauna of the Comoro Islands, to take another insular group, seems to stand midway in the differentiating process between the Canaries and the Sandwich Islands. From the researches of M. Humblot, worked out by MM. Milne-Edwards and Oustalet, we find that there are twenty-nine species acknowledged as peculiar; two species from South Africa and twenty-two from Madagascar in process of specification, called by M. Milne-Edwards secondary or derived species.

The little Christmas Island, an isolated rock 200 miles south of Java, only 12 miles in length, has been shown by Mr. Lister to produce distinct and peculiar forms of every class of life, vegetable and animal. Though the species are few in number, yet every mammal and land bird is endemic; but, as Darwin remarks, to ascertain whether a small isolated area, or a large open area like a continent, has been more favourable for the production of new organic forms, we ought to make the comparison between equal times, and this we are incapable of doing. My own attention was first directed to this subject when, in the year 1857-58, I spent many months in the Algerian Sahara, and noticed the remarkable variations in different groups, according to elevation from the sea, and the difference of soil and vegetation. The 'Origin of Species' had not then appeared; but on my return my attention was called to the communication of Darwin and Wallace to the Linnean Society on the tendencies of species to form varieties, and on the perpetuation of varieties and species by means of natural selection. I then wrote: [1] 'It is hardly possible, I should think, to illustrate this theory better than by the larks and chats of North Africa. In all these, in the congeners of the wheatear, of the rock chat, of the crested lark, we trace gradual modifications of coloration and of anatomical structure, deflecting by very gentle gradations from the ordinary type, but, when we take the extremes, presenting the most marked differences . . . In the desert, where neither trees, brushwood, nor even undulations of surface

[1] *Ibis* 1859, pp 429-433.

afford the slightest protection to an animal from its foes, a modification of colour, which shall be assimilated to that of the surrounding country, is absolutely necessary. Hence, without exception, the upper plumage of every bird—whether lark, chat, sylvan or land grouse—and also the fur of all the small mammals, and the skin of all the snakes and lizards, is of the uniform isabelline or sand-colour. It is very possible that some further purpose may be served by the prevailing colours, but this appears of itself a sufficient explanation. There are individual varieties of depth of hue among all creatures. In the struggle for life which we know to be going on among all species, a very slight change for the better, such as improved means of escape from its natural enemies (which would be the effect of an alteration from a conspicuous colour to one resembling the hue of the surrounding objects), would give the variety that possessed it a decided advantage over the typical or other forms of the species. . . . To apply the theory to the case of the Sahara If the Algerian Desert were colonised by a few pairs of crested larks—putting aside the ascertained fact of the tendency of an arid, hot climate to bleach all dark colours—we know that the probability is that one or two pairs would be likely to be of a darker complexion than the others. These, and such of their offspring as most resembled them, would become more liable to capture by their natural enemies, hawks and carnivorous beasts. The lighter-coloured ones would enjoy more or less immunity from such attacks. Let this state of things continue for a few hundred years and the dark-coloured individuals would be exterminated, the light-coloured remain and inherit the land This process, aided by the above-mentioned tendency of the climate to bleach the coloration still more, would in a few centuries produce the *Galerida abyssinica* as the typical form ; and it must be noted that between it and the European *G. cristata* there is no distinction but that of colour.

'But when we turn to *Galerida isabellina*, *G. arenicola*, and *G. macrorhyncha*, we have differences, not only of colour, but of structure. These differences are most marked in the form of the bill. Now, to take the two former first, *G. arenicola* has a very long bill, *G. isabellina* a very short one ; the former resorts exclusively to the deep, loose, sandy tracts, the latter haunts the hard and rocky districts. It is manifest that a bird whose food has to be sought for in deep sand derives a great advantage from any elongation, however slight, of its bill. The other, who feeds among stones and rocks, requires strength rather than length. We know that even in the type species the size of the bill varies in individuals—in the lark as well as in the snipe. Now, in the desert, the shorter-billed varieties would undergo comparative difficulty in finding food where it was not abundant, and, consequently would not be in such vigorous condition as their longer-billed relations. In the breeding season, therefore, they would have fewer eggs and a weaker progeny. Often, as we know, a weakly bird will abstain from matrimony altogether. The natural result of these causes would be that in course of time the longest-billed variety would steadily predominate over the shorter, and, in a few centuries, they would be the sole existing race ; their shorter-billed fellows dying out until that race was extinct. The converse will still hold good of the stout-billed and weaker-billed varieties in a rocky district.

'Here are only two causes enumerated which might serve to create, as it were, a new species from an old one Yet they are perfectly natural causes, and such as I think must have occurred, and are possibly occurring still. We know so very little of the causes which, in the majority of cases, make species rare or common that there may be hundreds of others at work, some even more powerful than these, which go to perpetuate and eliminate certain forms " according to natural means of selection." '

It would appear that those species in continental areas are equally liable to variation with those which are isolated in limited areas, yet that there are many counteracting influences which operate to check this tendency. It is often assumed, where we find closely allied species apparently inter-breeding at the centre of their area, that the blending of forms is caused by the two races commingling. Judging from insular experience I should be inclined to believe that the theory of inter-breeding is beginning at the wrong end, but rather that while the generalised forms

remain in the centre of distribution, we find the more decidedly distinct species at the extremes of the range, caused not by inter-breeding, but by differentiation. To illustrate this by the group of the blue titmouse. We find in Central Russia, in the centre of distribution of the family, the most generalised form, *Parus pleskii*, partaking of the characters of the various species east, west, and south. In the north-east and north it becomes differentiated as *P. cyaneus;* to the south-west and south into *P. cæruleus* and its various sub-species, while a branch extending due east has assumed the form of *P. flavipectus*, bearing traces of affinity to its neighbour *P. cyaneus* in the north, which seems evidently to have been derived from it.

But the scope of field observation does not cease with geographical distribution and modification of form. The closet systematist is very apt to overlook or to take no count of habits, voice, modification, and other features of life which have an important bearing on the modification of species. To take one instance, the short-toed lark (*Calandrella brachydactyla*) is spread over the countries bordering on the Mediterranean; but, along with it, in Andalusia alone is found another species, *Cal. bætica*, of a rather darker colour, and with the secondaries generally somewhat shorter. Without further knowledge than that obtained from a comparison of skins, it might be put down as an accidental variety. But the field naturalist soon recognises it as a most distinct species. It has a different voice, a differently shaped nest; and, while the common species breeds in the plains, this one always resorts to the hills. The Spanish shepherds on the spot recognise their distinctness, and have a name for each species Take, again, the eastern form of the common song-thrush. The bird of North China, *Turdus auritus*, closely resembles our familiar species, but is slightly larger, and there is a minute difference in the wing formula. But the field naturalist has ascertained that it lays eggs like those of the missel-thrush, and it is the only species closely allied to our bird which does not lay eggs of a blue ground colour. The hedge accentor of Japan (*Accentor rubidus*) is distinguished from our most familiar friend, *Accentor modularis*, by delicate differences of hue. But, though in gait and manner it closely resembles it, I was surprised to find the Japanese bird strikingly distinct in habits and life, being found only in forest and brushwood several thousand feet above the sea. I met with it first at Chinsenze—6,000 feet—before the snow had left the ground, and in summer it goes higher still, but never descends to the cultivated land. If both species are derived, as seems probable, from *Accentor immaculatus* of the Himalayas, then the contrast in habits is easily explained. The lofty mountain ranges of Japan have enabled the settlers there to retain their original habits, for which our humbler elevations have afforded no scope.

On the solution of the problem of the migration of birds, the most remarkable of all the phenomena of animal life, much less aid has been contributed by the observations of field naturalists than might reasonably have been expected. The facts of migration have, of course, been recognised from the earliest times, and have afforded a theme for Hebrew and Greek poets 3,000 years ago. Theories which would explain it are rife enough, but it is only of late years that any systematic effort has been made to classify and summarise the thousands of data and notes which are needed in order to draw any satisfactory conclusion. The observable facts may be classified as to their bearing on the whither, when, and how of migration, and after this we may possibly arrive at a true answer to the Why? Observation has sufficiently answered the first question, Whither?

There are scarcely any feathered denizens of earth or sea to the summer and winter ranges of which we cannot now point. Of almost all the birds of the holo-arctic fauna, we have ascertained the breeding-places and the winter resorts. Now that the knot and the sanderling have been successfully pursued even to Grinnell Land, there remains but the curlew sandpiper (*Tringa subarquata*), of all the known European birds, whose breeding ground is a virgin soil, to be trodden, let us hope, in a successful exploration by Nansen, on one side or other of the North Pole. Equally clearly ascertained are the winter quarters of all the migrants. The most casual observer cannot fail to notice in any part of Africa, north or south, west coast or interior, the myriads of familiar species which winter there.

As to the time of migration, the earliest notes of field naturalists have been the records of the dates of arrival of the feathered visitors. We possess them for some localities, as for Norfolk by the Marsham family, so far back as 1736. In recent years these observations have been carried out on a larger and more systematic scale, by Middendorff, who, forty years ago, devoted himself to the study of the lines of migration in the Russian Empire, tracing what he called the *isopipteses*, the lines of simultaneous arrival of particular species, and by Professor Palmén, of Finland, who, twenty years later, pursued a similar course of investigation; and by Professor Baird on the migration of North American birds; and subsequently by Severtzoff as regards Central Asia, and Menzbier as regards Eastern Europe. As respects our own coasts, a vast mass of statistics has been collected by the labours of the Migration Committee appointed by the British Association in 1880, for which our thanks are due to the indefatigable zeal of Mr. John Cordeaux, and his colleague Mr. John Harvie Brown, the originators of the scheme by which the lighthouses were for nine years used as posts of observation on migration. The reports of that Committee are familiar to us, but the inferences are not yet worked out. I cannot but regret that the Committee has been allowed to drop. Professor W. W. Cooke has been carrying on similar observations in the Mississippi valley, and others, too numerous to mention, have done the same elsewhere. But, as Professor Newton has truly said, All these efforts may be said to pale before the stupendous amount of information amassed during more than fifty years by the venerable Herr Gätke of Heligoland, whose work we earnestly desire may soon appear in an English version.

We have, through the labours of the writers I have named, and many others, arrived at a fair knowledge of the When? of migration. Of the How? we have ascertained a little, but very little. The lines of migration vary widely in different species, and in different longitudes. The theory of migration being directed towards the magnetic pole, first started by Middendorff, seems to be refuted by Baird, who has shown that in North America the theory will not hold. Yet, in some instances, there is evidently a converging tendency in northward migrations. The line, according to Middendorff, in Middle Siberia is due north, in Eastern Siberia S E. to N.W., and in Western Siberia from S.W. to N.E. In European Russia Menzbier traces four northward routes: (1) A coast line coming up from Norway round the North Cape to Nova Zembla. (2) The Baltic line with bifurcation, one proceeding by the Gulf of Bothnia, and the other by the Gulf of Finland, which is afterwards again subdivided. (3) A Black Sea line, reaching nearly as far north as the valley of the Petchora, and (4) the Caspian line, passing up the Volga and reaching as far east as the valley of the Obi by other anastomosing streams

Palmén has endeavoured to trace the lines of migration on the return autumnal journey in the eastern hemisphere, and has arranged them in nine routes: (1) From Nova Zembla, round the West of Norway, to the British Isles. (2) From Spitzbergen, by Norway, to Britain, France, Portugal, and West Africa. (3) From North Russia, by the Gulf of Finland, Holstein, and Holland, and then bifurcating to the West Coast of France on the one side, and on the other up the Rhine to Italy and North Africa. (4 *a*) Down the Volga by the Sea of Azof, Asia Minor, and Egypt, while the other portion (4 *b*), trending east, passes by the Caspian and Tigris to the Persian Gulf. (5) By the Yenesei to Lake Baikal and Mongolia. (6) By the Lena on to the Amoor and Japan (7) From East Siberia to the Corea and Japan. (8) Kamschatka to Japan and the Chinese Coast. (9) From Greenland, Iceland, and the Faroes, to Britain, where it joins line 2.

All courses of rivers of importance form minor routes, and consideration of these lines of migration might serve to explain the fact of North American stragglers, the waifs and strays which have fallen in with great flights of the regular migrants and been more frequently shot on the east coast of England and Scotland than on the west coast or in Ireland. They have not crossed the Atlantic, but have come from the far north, where a very slight deflection east or west might alter their whole course, and in that case they would naturally

strike either Iceland or the west coast of Norway, and in either case would reach the east coast of Britain. But, if by storms, and the prevailing winds of the North Atlantic coming from the west, they had been driven out of their usual course, they would strike the coast of Norway, and so find their way hither in the company of their congeners.

As to the elevation at which migratory flights are carried on, Herr Gätke, as well as many American observers, holds that it is generally far above our ken, at least in normal conditions of the atmosphere, and that the opportunities of observation, apart from seasons and unusual atmospheric disturbance, are confined chiefly to unsuccessful and abortive attempts. It is maintained that the height of flight is some 1,500 to 15,000 feet, and if this be so, as there seems every reason to admit, the aid of land bridges and river valleys becomes of very slight importance. A trivial instance will illustrate this. There are two species of blue-throat, *Cyanecula suecica* and *C. leucocyana*: the former with its red breast patch is abundant in Sweden in summer, but is never found in Germany, except most accidentally, as the other is the common form of Central Europe. Yet both are abundant in Egypt and Syria, where they winter, and I have, on several occasions, obtained both species out of the same flock. Hence we infer that the Swedish bird makes its journey from its winter quarters with scarcely a halt, while the other proceeds leisurely to its nearer summer quarters. On the other hand, I have more than once seen myriads of swallows, martins, sand-martins, and, later in the season, swifts, passing up the Jordan Valley and along the Bukoa of Central Syria, at so slight an elevation that I was able to distinguish at once that the flight consisted of swallows or house-martins. This was in perfectly calm clear weather. One stream of swallows, certainly not less than a quarter of a mile wide, occupied more than half an hour in passing over one spot, and flights of house-martins, and then of sand-martins, the next day, were scarcely less numerous. These flights must have been straight up from the Red Sea, and may have been the general assembly of all those which had wintered in East Africa. I cannot think that these flights were more than 1,000 feet high. On the other hand, when standing on the highest peak in the Island of Palma, 6,500 feet, with a dense mass of clouds beneath us, leaving nothing of land or sea visible, save the distant Peak of Tenerife, 13,000 feet, I have watched a flock of Cornish choughs soaring above us, till at length they were absolutely undistinguishable by us except with field-glasses.

As to the speed with which the migration flights are accomplished, they require much further observation. Herr Gätke maintains that godwits and plovers can fly at the rate of 240 miles an hour (!), and the late Dr. Jerdon stated that the spine-tailed swift (*Acanthyllis caudacutus*), roosting in Ceylon, would reach the Himalayas (1,200 miles) before sunset. Certainly in their ordinary flight the swift is the only bird I have ever noticed to outstrip an express train on the Great Northern Railway.

Observation has shown us that, while there is a regular and uniform migration in the case of some species, yet that, beyond these, there comes a partial migration of some species, immigrants and emigrants simultaneously, and this, besides the familiar vertical emigration from higher to lower altitudes and *vice versâ*, as in the familiar instance of the lapwing and golden plover. There is still much scope for the field naturalist in observation of these partial migrations. There are also species in which some individuals migrate and some are sedentary, *e g*, in the few primeval forests which still remain in the Canary Islands, and which are enshrouded in almost perpetual mist, the woodcock is sedentary, and not uncommon. I have often put up the bird and seen the eggs; but in winter the number is vastly increased, and the visitors are easily to be distinguished from the residents by their lighter colour and larger size. The resident never leaves the cover of the dense forest, where the growth of ferns and shrubs is perpetual, and fosters a moist, rich, semi-peaty soil, in which the woodcock finds abundant food all the year, and has thus lost its migratory instincts.

But why do birds migrate? Observation has brought to light many facts which seem to increase the difficulties of a satisfactory answer to the question. The

autumnal retreat from the breeding quarters might be explained by a want of sufficient sustenance as winter approaches in the higher latitudes, but this will not account for the return migration in spring, since there is no perceptible diminution of supplies in the winter quarters. A friend of mine, who was for some time stationed at an infirmary at Kikombo, on the high plateau south-east of Victoria Nyanza Lake, almost under the equator, where there is no variation in the seasons, wrote to me that from November to March the country swarmed with swallows and martins, which seemed to the casual observer to consist almost wholly of our three species, though occasionally a few birds of different type might be noticed in the larger flocks. Towards the end of March, without any observable change in climatic or atmospheric conditions, nine-tenths of the birds suddenly disappeared, and only a sprinkling remained. These, which had previously been lost amid the myriad of winter visitants, seemed to consist of four species, of which I received specimens of two, *Hirundo puella* and *H. senegalensis*. One, described as white underneath, is probably *H. æthiopica :* and the fourth, very small, and quite black, must be a *Psalidoprocne.* All these remained through spring and summer. The northward movement of all the others must be through some impulse not yet ascertained In many other instances observation has shown that the impulse of movement is not dependent on the weather at the moment. This is especially the case with sea birds. Professor Newton observes they can be trusted as the almanack itself. Foul weather or fair, heat or cold, the puffins, *Fratercula arctica*, repair to some of their stations punctually on a given day, as if their movements were regulated by clockwork. In like manner, whether the summer be cold or hot, the swifts leave their summer home in England about the first week in August, only occasional stragglers ever being seen after that date. So in three different years I noticed the appearance of the common swift (*Cypselus apus*) in myriads on one day in the first week in April. In the case of almost all the land birds, it has been ascertained by repeated observations that the male birds arrive some days before the hens. I do not think it is proved that they start earlier ; but, being generally stronger than the females, it is very natural that they should outstrip their weaker mates. I think, too, that there is evidence that those species which have the most extended southerly, have also the most extended northerly range. The same may hold good of individuals of the same species, and may be accounted for by, or account for, the fact that, *e g.*, the individuals of the wheatear or of the willow wren which penetrate furthest north have longer and stronger wings than those individuals which terminate their journey in more southern latitudes. The length of wing of two specimens of *Saxicola œnanthe* in my collection from Greenland and Labrador exceeds by ·6 inch the length of British and Syrian specimens, and the next longest, exceeding them by ·5 inch, is from the Gambia. So the sedentary *Phylloscopus trochilus* of the Canaries has a perceptibly shorter wing than European specimens.

To say that migration is performed by instinct is no explanation of the marvellous faculty, it is an evasion of the difficulty. Professor Mobius holds that birds crossing the ocean may be guided by observing the rolling of the waves, but this will not hold good in the varying storms of the Atlantic, still less in the vast stretch of stormy and landless ocean crossed by the bronze cuckoo (*Chrysococcyx lucidus*) in its passage from New Guinea to New Zealand. Professor Palmén ascribes the due performance of the flight to experience, but this is not confirmed by field observers. He assumes that the flights are led by the oldest and strongest, but observation by Herr Gatke has shown that among migrants, as the young and old journey apart and by different routes, the former can have had no experience. All ornithologists are aware that the parent cuckoos leave this country long before their young ones are hatched by their foster-parents. The sense of sight cannot guide birds which travel by night, or span oceans or continents in a single flight. In noticing all the phenomena of migration, there yet remains a vast untilled region for the field naturalist.

What Professor Newton terms the sense of direction, unconsciously exercised, is the nearest approach yet made to a solution of the problem. He remarks

how vastly the sense of direction varies in human beings, contrasting its absence in the dwellers in towns compared with the power of the shepherd and the countryman, and, infinitely more, with the power of the savage or the Arab He adduces the experience of Middendorff among the Samojeds, who know how to reach their goal by the shortest way through places wholly strange to them. He had known it among dogs and horses (as we may constantly perceive), but was surprised to find the same incomprehensible animal faculty unweakened among uncivilised men. Nor could the Samojeds understand his enquiry how they did it ? They disarmed him by the question, How now does the arctic fox find its way aright on the Tundra, and never go astray ? and Middendorff adds : ' I was thrown back on the unconscious performance of an inherited animal faculty;' and so are we !

There is one more kind of migration. on which we know nothing, and where the field naturalist has still abundant scope for the exercise of observation. I mean what is called exceptional migration, not the mere wanderings of waifs and strays, nor yet the uncertain travels of some species, as the crossbill in search of food, but the colonising parties of many gregarious species, which generally, so far as we know in our own hemisphere, travel from east to west, or from south-east to north-west. Such are the waxwing (*Ampelis garrula*), the pastor starling (*Pastor roseus*) and Pallas's sandgrouse, after intervals sometimes of many years, or sometimes for two or three years in succession. The waxwing will overspread Western Europe in winter for a short time. It appears to be equally inconstant in its choice of summer quarters, as was shown by J. Wolley in Lapland. The rose pastor regularly winters in India, but never remains to breed. For this purpose the whole race seems to collect and travel north-west, but rarely, or after intervals of many years, returns to the same quarters. Verona, Broussa, Smyrna, Odessa, the Dobrudscha have all during the last half-century been visited for one summer by tens of thousands, who are attracted by the visitations of locusts, on which they feed, rear their young, and go. These irruptions, however, cannot be classed under the laws of ordinary migration. Not less inexplicable are such migrations as those of the African darter, which, though never yet observed to the north of the African lakes, contrives to pass, every spring, unobserved to the lake of Antioch in North Syria, where I found a large colony rearing their young, which, so soon as their progeny was able to fly, disappeared to the south-east as suddenly as they had arrived.

There is one possible explanation of the sense of direction unconsciously exercised, which I submit as a working hypothesis. We are all aware of the instinct, strong both in mammals and birds without exception, which attracts them to the place of their nativity. When the increasing cold of the northern regions, in which they all had their origin, drove the mammals southward, they could not retrace their steps, because the increasing polar sea, as the arctic continent sank, barred their way. The birds reluctantly left their homes as winter came on, and followed the supply of food. But as the season in their new residence became hotter in summer, they instinctively returned to their birthplaces, and there reared their young, retiring with them when the recurring winter impelled them to seek a warmer climate Those species which, unfitted for a greater amount of heat by their more protracted sojourn in the northern regions, persisted in revisiting their ancestral homes, or getting as near to them as they could, retained a capacity for enjoying a temperate climate, which, very gradually, was lost by the species which settled down more permanently in their new quarters, and thus a law of migration became established on the one side, and sedentary habits on the other.

If there be one question on which the field naturalist may contribute, as lion's provider to the philosopher, more than another, it is on the now much disputed topic of ' mimicry,' whether protective or aggressive. As Mr. Beddard has remarked on this subject, ' The field of hypothesis has no limits, and what we need is more study '—and, may we not add, more accurate observation of facts. The theory of protective mimicry was first propounded by Mr. H. W. Bates, from his observations on the Amazon. He found that the group of butterflies, *Heliconidæ*, conspicuously banded with yellow and black, were provided with certain glands

which secrete a nauseating fluid, supposed to render them unpalatable to birds   In the sand districts he found also similarly coloured butterflies, belonging to the family *Pieridæ*, which so closely resembled the others in shape and markings as to be easily mistaken for them, but which, unprovided with such secreting glands, were unprotected from the attacks of birds.   This resemblance, he thought, was brought about by natural selection for the protection of the edible butterflies, through the birds mistaking them for the inedible kinds.   Other cases of mimicry among a great variety of insects have since been pointed out, and the theory of protective mimicry has gained many adherents.   Among birds, many instances have been adduced.   Mr. Wallace has described the extraordinary similarity between birds of very different families, *Oriolus bouruensis* and *Philemon moluccensis*, both peculiar to the island of Bouru.   Mr. H. O. Forbes has discovered a similar brown oriole, *Oriolus decipiens,* as closely imitating the appearance of the *Philemon timorlaoensis* of Timor-laut.   A similar instance occurs in Ceram.   But Mr. Wallace observes that, while usually the mimicking species is less numerous than the mimicked, the contrary appears to be the case in Bouru, and it is difficult to see what advantage has been gained by the mimicry.   Now, all the species of *Philemon* are remarkably sombre-coloured birds, and the mimicry cannot be on their side.   But there are other brown orioles, more closely resembling those named, in other Moluccan islands, and yet having no resemblance to the *Philemon* of the same island, as may be seen in the case of the *Oriolus phæochromus* and *Philemon gilolensis* from Gilolo.   Yet the oriole has adopted the same livery which elsewhere is a perfect mimicry.   May it not therefore be that we have, in this group of brown orioles, the original type of the family, undifferentiated?   As they spread east and south we may trace the gradation, through the brown striation of the New Guinea bird, to the brighter, green-tinged form of the West Australian and the green plumage of the Southern Australian, while westward the brilliant yellows of the numerous Indian and African species were developed, and another group, preferring high elevations, passing through the mountain ranges of Java, Sumatra, and Borneo, intensified the aboriginal brown into black, and hence were evolved the deep reds of the various species which culminate in the crimson of Formosa, *Oriolus ardens,* and the still deeper crimsons of *O. trailli* of the Himalayas.

It is possible that there may be similarity without mimicry, and, by the five laws of mimicry as laid down by Wallace, very many suggested cases must be eliminated.   We all know that it is quite possible to find between species of very different genera extraordinary similarity which is not mimetic.   Take, for instance, the remarkable identity of coloration in the case of some of the African species *Macronyx* and the American *Sturnella,* or, again, of some of the African *Campophagæ* and the American *Agelæus.*   The outward resemblance occurs in both cases in the red as well as in the yellow-coloured species of all four groups. But we find that the *Macronyx* of America and the *Campophagæ* of Africa, in acquiring this coloration, have departed widely from the plain colour found in their immediate relatives.   If we applied Mr. Scudder's theory on insects, we must imagine that the prototype form has become extinct, while the mimicker has established its position.   This is an hypothesis which is easier to suggest than either to prove or to disprove.   Similar cases may frequently be found in botany. The strawberry is not indigenous in Japan, but in the mountains there I found a potentilla in fruit which absolutely mimicked the Alpine strawberry in the minutest particulars, in its runners, its blossoms, and fruit ; but the fruit was simply dry pith, supporting the seeds and retaining its colour without shrinking or falling from the stalk for weeks—a remarkable case, we cannot say of unconscious mimicry, but of unconscious resemblance.   Mimicry in birds is comparatively rare, and still rarer in mammals, which is not surprising when we consider how small is the total number of the mammalia, and even of birds, compared with the countless species of invertebrates.   Out of the vast assemblage of insects, with their varied colours and patterns, it would be strange if there were not many cases of accidental resemblance.   A strict application of Wallace's five laws would, perhaps, if all the circumstances were known, eliminate many accepted instances.

As to cases of edible insects mimicking inedible, Mr. Poulton admits that even unpalatable animals have their special enemies, and that the enemies of palatable animals are not indefinitely numerous.

Mr. Beddard gives tables of the results obtained by Weismann, Poulton, and others, which show that it is impossible to lay down any definite law upon the subject, and that the likes and dislikes of insect-eating animals are purely relative.

One of the most interesting cases of mimicry is that of the *Volucella*, a genus of *Diptera*, whose larvæ live on the larvæ of *Hymenoptera*, and of which the perfect insect closely resembles some species of humble-bee. Though this fact is unquestioned, yet it has recently given rise to a controversy, which, so far as one who has no claim to be an entomologist can judge, proves that, while there is much that can be explained by mimicry, there is, nevertheless, a danger of its advocates pressing it too far. *Volucella bombylans* occurs in two varieties, which prey upon the humble-bees, *Bombus muscorum* and *B. lapidarius*, which they respectively resemble. Mr. Bateson does not question the behaviour of the *Volucella*, but states that neither variety specially represents *B. muscorum*, and yet that they deposit their eggs more frequently in their nests than in the nests of other species which they resemble more closely. He also states that in a show-case in the Royal College of Surgeons, to illustrate mining, two specimens of another species, *B. sylvarum*, were placed alongside of the *Volucella*, which they do resemble, but were labelled *B. muscorum*.

But Mr. Hart explains the parasitism in another way. He states that a nest of *B. muscorum* is made on the surface, without much attempt at concealment, and that the bee is a peculiarly gentle species, with a very feeble sting; but that the species which the *Volucella* most resemble are irascible, and therefore more dangerous to intruders. If this be so, it is difficult to see why the *Volucella* should mimic the bee, which it does not affect, more closely than the one which is generally its victim. I do not presume to express any opinion further than this, that the instances I have cited show that there is much reason for further careful observation by the field naturalist, and much yet to be discovered by the physiologist and the chemist, as to the composition and nature of animal pigments.

I had proposed to occupy a considerable portion of my address with a statement of the present position of the controversy on heredity, by far the most difficult and important of all those subjects which at present attract the attention of the biologist; but an attack of illness has compelled me to abandon my purpose. Not that I proposed to venture to express any opinions of my own, for, with such protagonists in the field as Weismann, Wallace, Romanes, and Poulton on the one side, and Herbert Spencer and Hartog on the other, '*Non nostrum inter vos tantas componere lites.*'

So far as I can understand Weismann's theory, he assumes the separation of germ cells and somatic cells, and that each germ cell contains in its nucleus a number of 'ids,' each 'id' representing the personality of an ancestral member of the species, or of an antecedent species. 'The first multicellular organism was probably a cluster of similar cells, but these units soon lost their original homogeneity. As the result of mere relative position, some of the cells were especially fitted to provide for the nutrition of the colony, while others undertook the work of reproduction.' The latter, or germ-plasm, he assumes to possess an unlimited power of continuance, and that life is endowed with a fixed duration, not because it is contrary to its nature to be unlimited, but because the unlimited existence of individuals would be a luxury without any corresponding advantage.

Herbert Spencer remarks upon this: 'The changes of every aggregate, no matter of what kind, inevitably end in a state of equilibrium. Suns and planets die, as well as organisms.' But has the theory been proved, either by the histologist, the microscopist, or the chemist? Spencer presses the point that the immortality of the protozoa has not been proved. And, after all, when Weismann makes the continuity of the germ-plasm the foundation of a theory of heredity, he is building upon a pure hypothesis.

From the continuity of the germ-plasm, and its relative segregation from the body at large, save with respect to nutrition, he deduces, *à priori*, the impossibility

of characters acquired by the body being transmitted through the germ-plasm to the offspring  From this he implies that where we find no intelligible mechanism to convey an imprint from the body to the germ, there no imprint can be conveyed. Romanes has brought forward many instances which seem to contradict this theory, and Herbert Spencer remarks that 'a recognised principle of reasoning—" the law of parsimony "—forbids the assumption of more causes than are needful for the explanation of phenomena  We have evident causes which arrest the cell multiplication, therefore it is illegitimate to ascribe this arrest to some property inherent in the cells.'

With regard to the reduction or disappearance of an organ, he states 'that when natural selection, either direct or reversed, is set aside, why the mere cessation of selection should cause decrease of an organ, irrespective of the direct effects of disease, I am unable to see. Beyond the production of changes in the size of parts, by the selection of fortuitously arising variation, I can see but one other cause for the production of them—the competition among the parts for nutriment. . . . The active parts are well supplied, while the inactive parts are ill supplied and dwindle, as does the arm of the Hindu fakir. This competition is the cause of economy of growth—this is the cause of decrease from disease.'

I may illustrate Mr. Herbert Spencer's remarks by the familiar instance of the pinions of the Kakapo (*Stringops*)—still remaining, but powerless for flight.

As for acquired habits, such as the modification of bird architecture by the same species under changed circumstances, how they can be better accounted for than by hereditary transmitted instinct, I do not see  I mean such cases as the ground-nesting *Didunculus* in Samoa having saved itself from extinction since the introduction of cats, by roosting and nesting in trees; or the extraordinary acquired habit of the black-cap in the Canaries, observed by Dr. Lowe, of piercing the calyx of *Hibiscus rosa-sinensis*—an introduced plant—to attract insects, for which he quietly sits waiting. So the lying low of a covey of partridges under an artificial kite would seem to be a transmitted instinct from a far-off ancestry not yet lost; for many generations of partridges, I fear, must have passed since the last kite hovered over the forefathers of an English partridge, save in very few parts of the island.

I cannot conclude without recalling that the past year has witnessed the severance of the last link with the pre-Darwinian naturalists in the death of Sir Richard Owen. Though never himself a field-worker, or the discoverer of a single animal living or extinct, his career extends over the whole history of palæontology.  I say palæontology, for he was not a geologist in the sense of studying the order, succession, area, structure, and disturbance of strata.  But he accumulated facts on the fossil remains that came to his hands, till he won the fame of being the greatest comparative anatomist of the age.  To him we owe the building up of the skeletons of the giant *Dinornithidæ* and many other of the perished forms of the gigantic sloths, armadilloes, and mastodons of South America, Australia, and Europe.  He was himself a colossal worker, and he never worked for popularity.  He had lived and worked too long before the Victorian age to accept readily the doctrines which have revolutionised that science, though none has had a larger share in accumulating the facts, the combination of which of necessity produced that transformation.  But, though he clung fondly to his old idea of the archetype, no man did more than Owen to explode the rival theories of both Wernerians and Huttonians, till the controversies of Plutonians and Neptunians come to us from the far past with as little to move our interest as the blue and green controversies of Constantinople.

Nor can we forget that it is to Sir Richard's indomitable perseverance that we owe the magnificent palace which contains the national collections in Cromwell Road  For many years he fought the battle almost alone. His demand for a building of two stories, covering five acres, was denounced as audacious.  The scheme was pronounced foolish, crazy, and extravagant; but, after twenty years' struggle, he was victorious, and in 1872 the Act was passed which gave not five, but more than seven acres for the purpose.  Owen retired from its direction in 1883, having achieved the crowning victory of his life.  Looking back in his old age on the

scientific achievements of the past, he fully recognised the prospects of still further advances, and observed, 'The known is very small compared with the knowable, and we may trust in the Author of all truth, who, I think, will not let that truth remain for ever hidden.'

I have endeavoured to show that there is still room for all workers, that the naturalist has his place, though the morphologist and the physiologist have rightly come into far greater prominence, and we need not yet abandon the field-glass and the lens for the microscope and the scalpel. The studies of the laboratory still leave room for the observations of the field. The investigation of muscles, the analysis of brain tissue, the research into the chemical properties of pigment, have not rendered worthless the study and observation of life and habits. As you cannot diagnose the Red Indian and the Anglo-Saxon by a comparison of their respective skeletons or researches into their muscular structure, but require to know the habits, the language, the modes of thought of each; so the mammal, the bird, and even the invertebrate, has his character, his voice, his impulses, aye, I will add, his ideas, to be taken into account in order to discriminate him. There is something beyond matter in life, even in its lowest forms I may quote on this the caution uttered by a predecessor of mine in this chair (Professor Milnes Marshall): 'One thing above all is apparent, that embryologists must not work single-handed; must not be satisfied with an acquaintance, however exact, with animals from the side of development only; for embryos have this in common with maps, that too close and too exclusive a study of them is apt to disturb a man's reasoning power.'

The ancient Greek philosopher gives us a threefold division of the intellectual faculties—φρόνησις, ἐπιστήμη, σύνεσις—and I think we may apply it to the subdivision of labour in natural science: φρόνησις, ἡ τὰ καθ' ἕκαστα γνωρίζουσα, is the power that divides, discerns, distinguishes—i.e. the naturalist; σύνεσις, the operation of the closet zoologist, who investigates and experiments; and ἐπιστήμη, the faculty of the philosopher, who draws his conclusions from facts and observations.

The older naturalists lost much from lack of the records of previous observations, their difficulties were not ours, but they went to nature for their teachings rather than to books. Now we find it hard to avoid being smothered with the literature on the subject, and being choked with the dust of libraries. The danger against which Professor Marshall warns the embryologist is not confined to him alone; the observer of facts is equally exposed to it, and he must beware of the danger, else he may become a mere materialist. The poetic, the imaginative, the emotional, the spiritual, all go to make up the man; and if one of these is missing, he is incomplete.

I cannot but feel that the danger of this concentration upon one side only of nature is painfully illustrated in the life of our great master, Darwin. In his early days he was a lover of literature, he delighted in Shakespeare and other poets; but after years of scientific activity and interest, he found on taking them up again that he had not only grown indifferent to them, but that they were even distasteful to him. He had suffered a sort of atrophy on that side of his nature, as the disused pinions of the Kakapo have become powerless—the spiritual, the imaginative, the emotional, we may call it.

The case of Darwin illustrates a law—a principle we may call it—namely, that the spiritual faculty lives or dies by exercise or the want of it even as does the bodily. Yet the atrophy was unconscious. Far was it from Darwin to ignore or depreciate studies not his own. He has shown us this when he prefixed to the title-page of his great work the following extract from Lord Chancellor Bacon:—'To conclude, therefore, let no man, out of a weak conceit of sobriety, or an ill-applied moderation, think or maintain that a man can search too far, or be too well studied in the book of God's word, or in the book of God's works, divinity or philosophy, but rather let men endeavour an endless progress or proficience in both.' In true harmony this with the spirit of the father of natural history, concluding with the words, 'O Lord, how manifold are Thy works, in wisdom hast Thou made them all, the earth is full of Thy riches.'

# ADDRESS

TO THE

# GEOGRAPHICAL SECTION

OF THE

# BRITISH ASSOCIATION.

BY

## HENRY SEEBOHM, Sec.R.G S., F.L.S., F Z S.

PRESIDENT OF THE SECTION.

GEOGRAPHY, the child of Mathematics and Astronomy, stands in the relation of mother to half a dozen other sciences, which have long ago left the parental roof to establish sections of their own. Like every other science, geography is so closely connected with, and dependent on, its allied sciences that it is impossible to treat of the one without invading the province of the others. No one supposes that the making of maps is the whole duty of the geographer. The accurate delineation of the trend of coast-lines, the courses of rivers, the heights of mountains, the depths of seas, or the position of towns is only the skeleton which underlies the real science of geography.

The study of geography may be divided into various sections, but it must always be remembered that they dovetail into each other, as well as into the allied sciences, to such an extent that no hard-and-fast line can be drawn between them. The object of dividing so comprehensive a section as that of geography into sub-sections is more practical than scientific. The classification of facts is an important aid to memory, and introduces order into what might otherwise seem to be a chaos of knowledge.

The foundation of all geography is EXPLORATION ; but before the traveller can do good geographical work he must acquire the necessary knowledge embraced in the science of CARTOGRAPHY. This includes a practical acquaintance with the various instruments used in making a survey, the necessary mathematical and astronomical knowledge required for their use, and a familiarity with the accepted mode of expressing the geographical facts that may be acquired on a chart or map. Exploration may then be undertaken with some chance of ultimate success, but the object of exploration must be something more than the filling up of blanks in our maps. Many other subjects must receive attention, subjects which are collectively included in the term physical geography, but which require treatment under different heads. Of these the most obvious is the geographical distribution of light and heat, as well as the more fitful alternations of wind and rain with calm and drought; in other words, the numerous causes which combine to produce climate. Meteorology or CLIMATOLOGY, the geography of the air, is a most important branch of geography in general; and when we come to inquire into the changes which have taken place in the climate of different parts of the earth's surface, especially those which have affected the Polar Basin, we enter upon a subject which has claimed a large share of the attention of geologists, who have also made a profound study of the geographical distribution of the various kinds of rock which are found on the crust of the earth. Another sub-section of great importance is the geographical distribution of organic life. The geographical ranges of the species and genera, both of plants and animals, have become a subject of vastly increased importance since so much attention has been directed to the

theory of evolution, and the paramount importance of the human race is so great that ethnological geography may fairly claim to be treated as a sub-section apart from the study of the rest of the fauna of a country. Inasmuch as a map with the towns left out is only half a map, the geographer cannot afford to neglect the races of men with which he comes in contact, nor the remains (architectural or otherwise) which existing nations have produced or past races have left behind them.

I propose on the present occasion to elaborate these subjects at greater detail, and, with your permission, to take the Polar Basin as an example.

There is only one Polar Basin; the relative distribution of land and water and the geographical distribution of light and heat in the Arctic region is absolutely unique. In no other part of the world is a similar climate to be found. The distribution of land and water round the South Pole is almost the converse of that round the North Pole. In the one we have a mountain of snow and ice covering—it may be a continent, it may be an archipelago, but in any case a lofty mass of congealed water surrounded by an ocean stretching away with very little interruption from land to the confines of the tropics. In the other we have a basin of water surrounding a comparatively flat plain of pack ice, some of which is probably permanent (the so-called palæocrystic sea), but most of which is driven hither and thither in summer by winds and currents, and is walled in by continental and island barriers broken only by the narrow outlets of Behring Strait and Baffin's Bay and the broader gulf which leads to the Atlantic Ocean, and even that interrupted by Iceland, Spitzbergen, and Franz Josef Land. When we further remember that this gulf is constantly conveying the hot water of the tropics to the Arctic Ocean, and that every summer gigantic rivers are pouring volumes of comparatively warm water into this ocean, we cannot but admit that the climatic conditions near the two poles differ widely from each other.

In looking at a map of the Polar Basin one cannot help remarking the curious fact that the North Pole is so very nearly central, and a glance at the southern hemisphere also shows a rough sort of symmetry in the distribution of land and water round the South Pole. It is a curious coincidence if this be only accident.

The history of the

### EXPLORATION

of the Polar Basin is a very long and a very tragic story. Much has been done, but much remains to do. The unexplored regions of the Polar Basin may be estimated at a million square miles. No part of the world presents greater difficulties to the explorer. Many brave men have perished in the enterprise, and more have only just succeeded in passing through the ordeal of hunger and cold with their lives. For the most part the heroic endurance of the tortures of famine has shown a marvel of discipline, though occasionally the commanders of the expeditions have had to enforce obedience to the verge of cruelty, both in the case of men and of dogs. There are, indeed, a few ghastly stories of mutinous men who have been shot, and of cases where it has been found necessary to resort to human food to save the lives of the survivors, but the records of Arctic exploration are records of which any nation might be proud.

Of recent years there has been but little done to explore the unknown parts of the Polar Basin. Adventurous journeys in Central Africa and Central Asia have somewhat eclipsed the exploration of the Arctic regions. Two visits to Greenland cannot, however, be entirely passed by in silence In the summer of last year an expedition went to the north of Greenland under the command of Lieutenant Peary, succeeded in reaching latitude 82°, and added material evidence to prove that Greenland is an island. The expedition sailed on June 6, 1891, steamed up Baffin's Bay and Smith's Sound, and on July 25 dismissed the ship and established themselves in winter quarters in McCormick Bay, on the north side of Murchison Sound, in latitude 78°. They laid in a stock of game for the winter, guillemots and reindeer. A most interesting proof of the successful organisation of the expedition is the fact that Mrs. Peary was one of the party, and was able to accompany her husband on his sledge trip which started on the 18th of the following April.

It took them a week in their dog sledges to round Inglefield Gulf, during which they discovered thirty glaciers, ten of them of the first magnitude  During the next three months they explored the north coast of Greenland, as far east as longitude 34° W , when a great bay was reached which they named Independence Bay, as they discovered it on July 4.  The northern shore of this bay was free from snow and ice  On August 6 they regained their winter quarters in McCormick Bay.  On the 8th the steamer arrived, and on the 24th they started for home, reaching Philadelphia on September 23.  During the sledge journey they travelled for a fortnight at an average elevation of 8,000 feet above the sea.  Besides their important additions to the map of Greenland, the suggestive fact that the thermometer can rise to 41° F., and torrents of rain can fall in the middle of February as far north as latitude 78°, must be regarded as a valuable discovery.

It was hardly to be expected that so successful a journey should not be followed by a second attempt in order to follow up the discoveries of the first.  Peary has already started for the north of Greenland with a more carefully organised staff for a longer expedition.  They expect to be absent two years or more.  It has been arranged to spend the coming winter not far from their previous quarters. In March they hope to start for Independence Bay, which was discovered on the previous expedition, and there the party will divide, with the object of completing the survey of the coast line of Greenland by reaching Cape Bismarck if possible, and at the same time to explore the northern coast line of Independence Bay, hoping that it may land them further north than the highest point yet reached by any Arctic traveller.

In the summer of 1888 Dr. Nansen was bold enough to cross the continent of Greenland about latitude 64°, reaching an altitude of 9,000 feet, and he told his story to this Section in his own simple words on his return.  The distance across was about ten degrees, and the highest point was about one-third of the way across from the east coast.  If the scientific results were necessarily somewhat meagre, Dr. Nansen established a reputation for bravery and physical endurance which he hopes to increase by an attempt to reach the North Pole.  The ' Fram ' has already started from Hammerfest, and was telegraphed a few weeks ago from the east coast of Norway.  The intention is to enter the Kara Sea and to push northwards and eastwards, hoping that the warm currents caused by the great Siberian rivers will enable them to get well into the ice before winter begins.  Once frozen into the pack ice, Nansen hopes to be carried by the currents somewhere near the North Pole, and, after drifting for two or three years, he hopes finally to emerge from his ice prison somewhere on the east coast of Greenland.  Foolhardy as the expedition appears, it is nevertheless planned with great skill, and its chances of success are supposed to be based upon a sufficiently accurate knowledge of the ocean currents of the Polar Basin.

These currents, so far as they are known, are very interesting  The Mackenzie and the great Siberian rivers flow into the Polar Basin, and the current through Behring Strait is supposed to do the same; but both these sources of supply can only be regarded as of minor importance.  Between Spitzbergen and Finmark, however, the Gulf Stream enters the Polar Basin 300 or 400 miles wide.  To compensate for these inward currents there are two outward currents, one on each side of Greenland, which, coming from the centre of cold, do their best to intensify the rigours of that mountainous island.

Nansen hopes that the current which carried the ' Jeannette ' from Herald Island, north of Behring Strait, in a north-westerly direction, for 500 or 600 miles is the same current that flows down the east coast of Greenland, and he bases his hopes upon three facts  First, that many articles from the wreck of the ' Jeannette' were found on an ice floe off the south coast of Greenland three years afterwards , second, that a harpoon-thrower of a pattern unknown except in Alaska was picked up on the south-west coast of Greenland , and, third, that drift-wood supposed to be of Siberian origin is stranded regularly in considerable quantity on the coasts of Greenland  The Norwegian at Hammerfest, about latitude 70°, is dependent for his firewood upon the Gulf Stream, which brings him an ample supply from the Gulf of Mexico, whilst the Eskimo on the Greenland coast, in the same latitude, trusts

to a current from the opposite direction to bring him his necessary store of wood from the Siberian forests.

We can only hope that Nansen will find the currents as favourable to his needs, and that so much bravery may be supported by good luck.

By no means the least important physical feature of the Polar Basin is its gigantic

### RIVER SYSTEMS.

The rivers which flow into the Arctic Ocean are some of them amongst the greatest in the world.

Some idea of the relative sizes of the drainage areas of a few of the best known rivers may be learnt from the following table, in which the Thames, with a drainage area of 6,000 square miles, is the unit :—

| | | | |
|---|---|---|---|
| 9 | Thames | equal | 1 Elbe (54,000). |
| 2 | Elbes | ,, | 1 Pechora (108,000). |
| 2½ | Pechoras | ,, | 1 Danube (270,000) |
| 2 | Danubes | | 1 Mackenzie (540,000). |
| 2 | Mackenzies | | 1 Yenisei (1,080,000). |
| 2 | Yeniseis | | 1 Amazon (2,160,000). |

Perhaps a more scientific classification of rivers would be to call those with a drainage area (between 2,560,000 and) over 1,280,000 square miles rivers of the first magnitude, a category which contains the Amazon alone. There are ten rivers of the second magnitude, with drainage areas between 1,280,000 and 640,000 square miles (Ob, Congo, Mississippi, La Plata, Yenisei, Nile, Lena, Niger, Amur, Yangtse). There are twelve rivers of the third magnitude, with drainage areas between 640,000 and 320,000 square miles (Mackenzie, Volga, Murray, Zambesi, Saskatchewan, Ganges, St. Lawrence, Orange, Orinoco, Hoang Ho, Indus, and Bramaputra). There are more than a dozen rivers of the fourth magnitude, with drainage areas between 320,000 and 160,000 square miles, but none of them empties itself into the Arctic Ocean. They include the Danube, Euphrates, and several of the African and South American rivers. Of the numerous rivers which are of the fifth magnitude, with drainage areas between 160,000 and 80,000 square miles, the Pechora belongs to the Polar Basin. The number of rivers of lesser magnitude is legion, and it is only necessary to quote one of each as an example.

| | | | |
|---|---|---|---|
| 6th magnitude | (80,000 to 40,000), | Rhine. |
| 7th | ,, | (40,000 to 20,000), | Rhone. |
| 8th | , | (20,000 to 10,000), | Garonne. |
| 9th | ,, | (10,000 to 5,000), | Thames. |

There is nothing that makes a greater impression upon the Arctic traveller than the enormous width of the rivers. The Pechora is only a river of the fifth magnitude, but it is more than a mile wide for several hundred miles of its course. The Yenisei is more than three miles wide for at least a thousand miles, and a mile wide for nearly another thousand. Whymper describes the Yukon as varying from one to four miles in width for three or four hundred miles of its length. The Mackenzie is described as averaging a mile in width for more than a thousand miles, with occasional expansions for long distances to twice that size.

The drainage area does not measure the size of the Arctic rivers at all adequately. Though the rainfall of many of them is comparatively small, the size of the rivers is relatively very large, owing to the sudden melting of the winter's accumulation of snow, which causes an annual flood of great magnitude, like the rising of the Nile. Even on the Amur in Eastern Siberia, and on the Yukon in Alaska, the annual flood is important enough, but on the rivers which flow north into the Polar Sea the damming up of the mouths by the accumulations of ice produces an annual deluge, frequently extending over thousands of square miles, a catastrophe the effects of which have been much underrated and never adequately described.

If we assume that the unknown regions are principally sea, then the Polar Basin, including the area drained by all rivers flowing into the Arctic Sea, may be roughly estimated to contain about 14,000,000 square miles, of which half is land and half water. In the coldest part of the basin the land is either glacier or tundra, and in the warmer parts it is either forest or steppe.

Greenland, the home of the

### GLACIER

and the mother of the icebergs of the Northern Atlantic, rises 9,000 or 10,000 feet above sea level, whilst the sea between that lofty plateau and Scandinavia is the deepest known in the Polar Basin, though it is separated from the rest of the Atlantic by a broad belt or submarine plateau connecting Greenland across Iceland and the Faroes with the British Islands and Europe. Iceland, Spitzbergen, and Novaya-Zemlia, the latter a continuation of the Urals, are all mountainous and full of glaciers. The glaciers of Southern Alaska are some of the largest in the world. The glaciers and the icebergs have a literature of their own, and we must pass them by to say a word or two about

### THE TUNDRA.

The Arctic Sea, which lies at the bottom of the Polar Basin, is fringed with a belt of bare country, sometimes steep and rocky, abruptly descending in more or less abrupt cliffs and piles of precipices to the sea, but more often sloping gently down in mud banks and sand hills representing the accumulated spoils of countless ages of annual floods, which tear up the banks of the rivers and deposit shoals of detritus at their mouths, compelling them to make deltas in their efforts to force a passage to the sea. In Norway this belt of bare country is called the Fjeld, in Russia it is known as the Tundra, and in America its technical name is the Barren Grounds. In the language of science it is the country beyond the limit of forest growth.

In exposed situations, especially in the higher latitudes, the tundra does really merit its American name of Barren Ground, being little else than gravel beds interspersed with bare patches of peat or clay, and with scarcely a rush or a sedge to break the monotony. In Siberia at least this is very exceptional. By far the greater part of the tundra, both east and west of the Ural Mountains, is a gently undulating plain, full of lakes, rivers, swamps, and bogs. The lakes are diversified with patches of green water plants, amongst which ducks and swans float and dive ; the little rivers flow between banks of rush and sedge ; the swamps are masses of tall rushes and sedges of various species, where phalaropes and ruffs breed, and the bogs are brilliant with the white fluffy seeds of the cotton grass. The groundwork of all this variegated scenery is more beautiful and varied still—lichens and moss of almost every conceivable colour, from the cream-coloured reindeer moss to the scarlet-cupped trumpet moss, interspersed with a brilliant alpine flora, gentians, anemones, saxifrages, and hundreds of plants, each a picture in itself, the tall aconites, both the blue and yellow species, the beautiful cloudberry, with its gay white blossom and amber fruit, the fragrant *Ledum palustre* and the delicate pink *Andromeda polifolia*. In the sheltered valleys and deep watercourses a few stunted birches, and sometimes large patches of willow scrub, survive the long severe winter, and serve as cover for willow grouse or ptarmigan. The Lapland bunting and red-throated pipit are everywhere to be seen, and certain favoured places are the breeding grounds of snipe, plover, and sandpipers of many species. So far from meriting the name of Barren Ground, the tundra is for the most part a veritable paradise in summer. But it has one almost fatal drawback—it swarms with millions of mosquitoes.

The tundra melts away insensibly into the

### FOREST,

but isolated trees are rare, and in Siberia there is an absence of young wood on the confines of the tundra. The limit of forest growth appears to be retiring south-

ward, if we may judge from the number of dead and dying stumps; but this may be a temporary or local variation caused by exceptionally severe winters. The limit of forest growth does not coincide with the isotherms of mean annual temperature, nor with the mean temperature for January nearly so closely as it does with the mean temperature for July. It may be said to approximate very nearly to the July isotherm of 53° F. We may therefore assume that a six-foot blanket of snow prevents the winter frosts from killing the trees so long as they can be revivified by a couple of months of summer heat above 50° F.

The limit of forest growth is thus directly determined by geographical causes. In Alaska and in the Mackenzie Basin it extends about 300 miles above the Arctic circle, but in Eastern Canada the depression of Hudson's Bay acts as a vast ice-house and the forest line falls 500 miles below the Arctic circle, whilst on the east coast of Labrador the Arctic current from Baffin's Bay sends it down nearly as far again. On the other side of the Atlantic the limit of forest growth begins on the Norwegian coast on the Arctic Circle, gradually rises until it reaches 200 miles farther north in Lapland, is depressed again by the ice-house of the White Sea, but has recovered its position in the valley of the Pechora, which is rather more than maintained until a second vast ice-house, the Sea of Okotsk, combined with Arctic currents, repeats the depression of Labrador in Chuski Land and Kamchatka.

There are no trees on Novaya-Zemlia. Two or three species of willow grow there, but they are dwarfs, seldom attaining a height of three inches. Novaya-Zemlia enjoys a comparatively mild winter, the mean temperature of January, thanks to the influence of the Gulf Stream, being 15° F. above zero in the south, and only 5° F. below zero in the north. The absence of trees is due to the cold summers, the mean temperature of July not reaching higher than 45° F. in the south, whilst in the north it only reaches 38° F.

The Indians of Canada have discovered that when they want to find water in winter it is easiest reached under thick snow, the thinnest ice on the river or lake being found under the thickest blanket of snow. On the same principle the tree roots defy the severe winters protected by their snow shields, but they must have a certain temperature (above 50° F.) to hold their own in summer.

The influence of the snow blanket is very marked in determining the depths to which the frost penetrates beneath it. Thus we find that a Norwegian writer, alluding to latitude 62°, remarks ' that the ground is frozen from one to two and a half feet in winter, but this depends upon how soon the snow falls. Higher up the mountains the ground is scarcely frozen at all, owing to the snow falling sooner, and, in fact, if the snow falls very early lower down it is scarcely frozen to any depth.' Similar facts have been recorded from Canada in latitude 53°. ' On this prairie land, when there is a good fall of snow when the winter sets in, the frost does not penetrate so deep as when there is no snow till late ' Another writer a little further south, in latitude 51°, says: ' I am safe in saying that the frost penetrates here to an average of five feet, except when we have had a great depth of snow in the beginning of winter, in which case it does not penetrate nearly so far.'

It is not so easy to explain the boundary line between the forest and the

## STEPPE.

There are two great steppe regions in the Polar Basin, one in Asia and the other in America. The great Barabinski steppe in South-west Siberia stretches with but slight interruptions across Southern Russia into Bulgaria. The great prairie region of Minnesota and Manitoba reaches the Mackenzie Basin, and outlying plains are found almost up to the Great Slave Lake The cause of the treeless condition of the steppes or prairies has given rise to much controversy My own experience in Siberia convinced me that the forests were rocky, and the steppes covered with a deep layer of loose earth, and I came to the conclusion that on the rocky ground the roots of the trees were able to establish themselves firmly so as to defy the strongest gales, which tore them up when they were planted in light soil. Other travellers have formed other opinions. Some suppose that the prairies were

once covered with trees which have been gradually destroyed by fires. Others suggest that the earth on the treeless plains contains too much salt or too little organic matter to be favourable to the growth of trees. No one, so far as I know, has suggested a climatic explanation of the circumstance. Want of drainage may produce a swamp and the deficiency of rainfall may cause a desert, both conditions being fatal to forest growth, but no one can mistake either of these treeless districts for a steppe or prairie. The

### ANTHROPOLOGY

of the Polar Basin presents many points of interest. On the American coasts of the Arctic Ocean the Eskimo lives a very similar life to the Lapp in Norway and the Samoyede in the tundras of Siberia. These races of men resemble each other very much in their personal appearance, and still more so in their habits. Their straight black hair, with little or no beard, their dark and obliquely set eyes, their high cheek bones and flat noses, and their small hands and feet, testify to their Mongoloid origin. They are all indebted to the reindeer for their winter dress and for much of their food, and they all have dogs; but the Eskimo travel only with dogs, and the Lapp only with reindeer, whilst the Samoyede uses both dog sledges and reindeer sledges. They all lead a nomadic life, trapping fur-bearing animals in winter and fishing in summer; they resemble each other in many other customs and beliefs, but they are nevertheless supposed to have emigrated to the Arctic regions from independent sources, and many characters in which they resemble each other are supposed to have been independently acquired.

The various races which inhabit the Polar Basin below the limit of forest growth are too numerous to be considered in detail.

Most zoologists divide the Polar Basin into two zoological regions, or, to be strictly accurate, they include the Old World half of the Polar Basin in what they call the Palæarctic region, and the New World half in the Nearctic region; but recent investigations have shown that these divisions are unnatural and cannot be maintained. Some writers unite the two regions together under the name of the Holarctic region, whilst others recognise a circumpolar Arctic region above the limit of forest growth, and unite in a second region the temperate portions of the northern hemisphere. In the opinion of the last-mentioned writers the circumpolar Arctic region differs more from the temperate regions of the northern hemisphere than the American portion of the latter does from the Eurasian portion. The fact is that

### LIFE AREAS,

or zoo-geographical regions, are more or less fanciful generalisations. The geographical distribution of animals, and probably also that of plants, is almost entirely dependent upon two factors, *climate* and *isolation,* the one playing quite as important a part as the other. The climate varies in respect of rainfall and temperature, and species are isolated from each other by seas and mountain ranges. The geographical facts which govern the zoological provinces consequently range themselves under these four heads. It is at once obvious that the influences which determine the geographical distribution of fishes must be quite different from those which determine the distribution of mammals, since the geographical features which isolate the species in the one case are totally different from those which form impassable barriers in the other. It is equally obvious that the climatic conditions which influence the geographical range of mammals must include the winter cold as well as the summer heat, whilst those which determine the geographical distribution of birds, most of which are migratory in the Arctic regions, is entirely independent of any amount of cold which may descend upon their breeding grounds during the months which they spend in their tropic or sub-tropic winter quarters.

Hence all attempts to divide the Polar Basin into zoological regions or provinces are futile. Nearly every group of animals has zoological regions of its own, determined by geographical features peculiar to itself, and any generalisations from these different regions can be little more than a curiosity of science. The mean temperature or distribution of heat can be easily ascertained. It is easy to generalise so as to arrive at an average between the summer heat and the winter

cold, because they can be both expressed in the same terms. When, however, it may
seek to generalise upon the distribution of animal or vegetable life, how is The
possible to arrive at a mean geographical distribution of animals? How many em-
genera of mollusks are equal to a genus of mammals, or how many butterflies are es
equal to a bird?                                                                ly

If there be any region of the world with any claim to be a life area, it is that et
part of the Polar Basin which lies between the July isotherm of 50° or 53° F. e
and the northern limit of organic life. The former corresponds very nearly with
the northern limit of forest growth, and they comprise between them the barren
grounds of America and the tundras of Arctic Europe and Siberia.

The fauna and flora of this circumpolar belt is practically homogeneous; many
species of both plants and animals range throughout its whole extent. It consti-
tutes a circumpolar Arctic region, and cannot consistently be separated at Behring
Strait into two parts of sufficient importance to rank even as sub-regions.

Animals recognise facts, and are governed by them in the extension of their
ranges; they care little or nothing about generalisations. The mean temperature
of a province is a matter of indifference to some plants and to most animals. The
facts which govern their distribution are various, and vary according to the needs
of the plant or animal concerned. To a migratory bird the mean annual temper-
ature is a matter of supreme indifference. To a resident bird the question is
equally beside the mark. The facts which govern the geographical distribution of
birds are the extremes of temperature, not the means. Arctic birds are nearly
all migratory. Their distribution during the breeding season depends primarily
on the temperature of July, which must range between 53° and 35° F. It is very
important, however, to remember that it is actual temperature that governs them,
not isotherms corrected to sea level. If an Arctic bird can find a correct isotherm
below the Arctic circle by ascending to an elevation of 5,000 or 6,000 feet
above the level of the sea, it avails itself of the opportunity. Then the region of
the Dovrefeld above the limit of forest growth is the breeding plan of many abso-
lutely Arctic birds; but this is not nearly so much the case on the Alps, because
the cold nights vary too much from the hot days to come within the range of the
birds' breeding grounds. Here, again, the mean daily temperature is of no import-
ance. It is the extreme of cold which is the most potent factor in this case, and
no extreme of heat can counterbalance its effect.

In estimating the influence of elevation upon temperature it has been ascer-
tained that it is necessary to deduct about 3° F. for every thousand feet. The

### ISOTHERMAL LINES

are very eccentric in the Polar Basin. The mean temperature of summer is quite
independent of that of winter. The isothermal lines of July are regulated by geo-
graphical causes which do not affect those of December, or operate in a contrary
direction. The Gulf Stream raises the mean temperature of Iceland during winter
to the highest point which it reaches in the Polar Basin, viz., 30° to 35° F., whilst
in summer it prevents it from rising above 45° and 50° F., a range of only 15°. In
the valley of the Lena in the same latitude the mean temperature of January is 55°
to 50° F. below zero, whilst that of July is 60° to 65° F. above zero, a range of 115°.

The close proximity of the Pacific Ocean has a much less effect on the mean
temperature at Behring Strait, which is in the same latitude as the north of Iceland.
The mean temperature for January is zero, whilst that for July is 40° F. The
mean temperature for January in the same latitude in the valley of the Mackenzie
is 25° below zero, whilst that for July is 55° F. In this case the contrast of the
ranges is 40 and 80, which compared with 15 and 115 is small, but the geo-
graphical conditions are not the same. Behring Sea is so protected by the Aleutian
chain of islands that very little of the warm current from Japan reaches the
straits. It is deflected southwards, so the Aleutian Islands form a better basis for
comparison. Their mean temperature for January is 35° F., whilst that for July
is 50° F., precisely the same difference as that to be found in Iceland.

The influence of geographical causes upon climate being at present so great, it
is easy to imagine that changes in the distribution of land and water may have
had an equally important influence upon the climate of the Polar Basin during the

recent cold age, which geologists call the Pleistocene period. It is impossible for the traveller to overlook the evidences of this so-called Glacial period in the Polar Basin, and whether we seek an explanation of the geographical phenomena from the astronomer or the geologist, or both, it is impossible to ignore the geographical interest of the subject.

No sciences can be more intimately connected than geography and

## GEOLOGY.

A knowledge of geography is absolutely essential to the geologist. To discriminate between one kind of rock and another is a comparatively small part of the work of the geologist. To ascertain the geographical distribution of the various rocks is a study of profound interest. If the geologist owes much to the geographer, the latter is also largely indebted to the labours of the former. The geology of a mountain range or an extended plain is as important to the physical geographer as the knowledge of anatomy is to the figure painter.

The geology of the Polar Basin is not very accurately known, and the subject is one too vast to be more than mentioned on an occasion like the present ; but the evidences of a comparatively recent ice age in eastern America and western Europe are too important to be passed by without a word

In the sub-arctic regions of the world there is much evidence to show that the climate has in comparatively recent times been Arctic. The present glaciers of Central Europe were once much greater than they are now, and even in the British Islands glaciers existed during what has been called the ice age, and the evidence of their existence in the form of rocks, upon which they have left their scratches and heaps of stones, which they have deposited in their retreat, are so obvious that he who runs may read. Similar evidence of an ice age is found in North America, and to a limited extent in the Himalayas, but in the alluvial plains of Siberia and North Alaska, as might be expected, no trace of an ice age can be found.

Croll's hypothesis that an ice age is produced when the eccentricity of the earth's orbit is unusually great has been generally accepted as the most plausible explanation of the facts. It is assumed that during the months of perihelion evaporation is extreme, and that during the months of aphelion the snowfall is considerably increased. The effect of the last period of high eccentricity is supposed to have been much increased by geographical changes. The elevation of the shallow sea which connects Iceland with Greenland on the one hand, and the south of Norway and the British Islands on the other, would greatly increase the accumulation of snow and ice in those parts of the Polar Basin where evidence of a recent ice age is now to be found ; whilst the depression of the lowlands on either side of the Ural Mountains, so as to admit the waters of the Mediterranean through the Black and Caspian Seas, might prevent any glaciation in those parts of the Polar Basin where no evidence of such a condition is now discoverable. But this is a question that must be left to the geologist to decide.

The extreme views of the early advocates of the theory of an ice age have been to a large extent abandoned No one now believes in the former existence of a Polar ice cap, and possibly when the irresistible force of ice-dammed rivers has been fully realised, the estimated area of glaciation may be considerably reduced. The so-called great ice age may have been a great snow age, with local centres of glaciation on the higher grounds.

The zoological evidence as to the nature, extent, and duration of the ice age has never been carefully collected. The attention of zoologists has unfortunately been too exclusively devoted to the almost hopeless task of theorising upon the causes of evolution, instead of patiently cataloguing its effects.

There is a mass of evidence bearing directly upon the recent changes in the climate of the Polar Basin to be found in the study of the present geographical distribution of birds. The absence of certain common British forest birds (some of them of circumpolar range sub-generically, if not specifically) from Ireland and the North of Scotland is strong confirmation of the theory that the latter countries were not very long ago outside the limit of forest growth.

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The presence of species belonging to Arctic and sub-Arctic genera on mar, r
the South Pacific Islands is strong evidence that they were compelled to emig.
in search of food by some great catastrophe, such as an abnormally heavy snowfa
and the fact that no island contains more than one species is strong evidence th
this great catastrophe has only occurred once in recent times.   The occurrence c
a well-recognised line of migration from Greenland across Iceland, the Faroes
and the British Islands to Europe is strongly suggestive of a recent elevation ot
the land where the more shallow sea now extends in this locality.   The extraordi-
nary similarity of the fauna and flora of the Arctic regions of the Old and New
Worlds can only be found elsewhere in continuous areas, and, had it not been for
the unfortunate division of the Arctic region into two halves, Palæarctic and
Nearctic, would have attracted much more attention than it has hitherto received.


### THE RAINFALL

of the Polar Basin is small compared to that with which we are familiar, but its
visible effects are enormous.   In Arctic Europe and Siberia it is supposed to
average about thirteen inches per annum ; in Arctic America not more than nine
inches.   The secret of its power is that about a third of the rainfall descends in
the form of snow, which melts with great suddenness.

The stealthy approach of winter on the confines of the Polar Basin is in strong
contrast to the catastrophe which accompanies the sudden onrush of summer.
One by one the flowers fade, and go to seed if they have been fortunate enough to
attract by their brilliancy a bee or other suitable pollen-bearing visitor.   The
birds gradually collect into flocks, and prepare to wing their way to southern
climes.   Strange to say. it is the young birds of each species that set the example.
They are not many weeks old.   They have no personal experience of migration,
but Nature has endowed them with an inherited impulse to leave the land of their
birth before their parents.   Probably they inherit the impulse to migrate without
inheriting any knowledge of where their winter quarters are to be found, and by
what route they are to be sought.   They are sometimes, if not always, accom-
panied by one or two adults, it may be barren birds, or birds whose eggs or young
have been destroyed, and who may therefore get over their autumn moult earlier
than usual, or moult slowly as they travel southwards   Of most species the adult
males are the next to leave, to be followed perhaps a week later by the adult
females.   One by one the various migratory species disappear until only the few
resident birds are left, and the Arctic forest and tundra resume the silence so con-
spicuous in winter.   As the nights get longer the frosts bring down the leaves
from the birch and the larch trees.   Summer gently falls asleep, and winter as
gently steals a march upon her, with no wind and no snow, until the frost silently
lays its iron grip upon the river, which, after a few impotent struggles, yields to
its fate.   The first and mayhap the second ice is broken up, and when the starrester
of the village sallies forth to peg out with rows of birch trees the winter road
down the river to the next village for which he is responsible, he has frequently to
deviate widely from the direct course in his efforts to choose the smoothest ice
and find a channel between the hummocks that continually block the way.

The date upon which winter resumes his sway varies greatly in different
localities, and probably the margin between an early and a late season is consider-
able.   In 1876 Captain Wiggins was frozen up in winter quarters on the Yenisei
in latitude, 66½° on October 17.   In 1878 Captain Palander was frozen up on the
coast 120 miles west of Behring Strait, in latitude 67¾°, on September 28.

The sudden arrival of summer on the Arctic Circle appears to occur nearly at
the same date in all the great river basins, but the number of recorded observations
is so small that the slight variation may possibly be seasonal and not local.   The
ice on the Mackenzie River is stated by one authority to have broken up on May 13
in latitude 62°, and by another on May 9 in latitude 67°.   If the Mackenzie breaks
up as fast as the Yenisei—that is to say, at the rate of a degree a day—-an assump-
tion which is supported by what little evidence can be found—then the difference
between these two seasons would be nine days.   My own experience has been that

the ice of the Pechora breaks up ten days before that of the Yenisei, but as I have only witnessed one such event in each valley too much importance must not be attached to the dates.

According to the 'Challenger' tables of isothermal lines the mean temperatures of January and July on the Arctic Circle in the valleys of the Mackenzie and the Yenisei scarcely differ, the summer temperature in each case being about 55° F., and that of winter −25° F., a difference of 80° F.

On the American side of the Polar Basin summer comes almost as suddenly as it does on the Asiatic side, but the change appears to be less of the nature of a catastrophe. The geographical causes which produce this result are the smaller area of the river basins and the less amount of rainfall. There is only one large river which empties itself into the Arctic Ocean on the American side, the Mackenzie, with which may be associated the Saskatchewan, which discharges into Hudson Bay far away to the south. The basin of the Mackenzie is estimated at 590,000 square miles, whilst that of the Yenisei is supposed to be exactly twice that area. The comparative dimensions of the two summer floods are still more diminished by the difference in the quantity of snow.

The snow in the Mackenzie basin is said to be from 2 to 3 feet deep, whilst that in the Yenisei basin is from 5 to 6 feet deep, so that the spring flood in the latter river must be about five times as large as that of the former.

Another feature in which the basin of the Mackenzie differs from those of the rivers in the Arctic regions of the Old World is the number of rapids and lakes contained in it. The ice in the large lakes attains a thickness at least twice as great as that of the rapid stream, and consequently breaks up much later. In the Great Slave Lake the ice attains a depth of 6 to 7 feet, and even in the Athabaska Lake, in latitude 58°, it reaches 4 feet. The rapids between these two lakes extend for 15 miles. The ice on the river breaks up a month before that on the lakes, so that the drainage area of the first summer flood is much restricted.

The arrival of summer in the Arctic regions happens so late that the inexperienced traveller may be excused for sometimes doubting whether it really is going to come at all. When continuous night has become continuous day without any perceptible approach to spring an alpine traveller naturally asks whether he has not reached the limit of perpetual snow. It is true that here and there a few bare patches are to be found on the steepest slopes, where most of the snow has been blown away by the wind, especially if these slopes face the south, where even an Arctic sun has more potency than it has elsewhere. It is also true that small flocks of little birds—at first snow-buntings and mealy redpoles, and later shore larks and Lapland buntings—may be observed to flit from one of these bare places to another looking for seeds or some other kind of food, but after all evidently finding most of it in the droppings of the peasants' horses on the hard snow-covered roads. The appearance of these little birds does not, however, give the same confidence in the eventual coming of summer to the Arctic naturalist as the arrival of the swallow or the cuckoo does to his brethren in sub-Arctic and sub-tropic climates. The four little birds just mentioned are only gipsy migrants that are perpetually flitting to and fro on the confines of the frost, continually being driven south by snowstorms, but ever ready to take advantage of the slightest thaw to press northwards again to their favourite Arctic home. They are all circumpolar in their distributions, are as common in Siberia as in Lapland, and range across Canada to Alaska as well as to Greenland. In sub-Arctic climates we only see them in winter, so that their appearance does not in the least degree suggest the arrival of summer to the traveller from the south.

The gradual rise in the level of the river inspires no more confidence in the final melting away of the snow and the disruption of the ice which supports it. In Siberia the rivers are so enormous that a rise of 5 or 6 feet is scarcely perceptible. The Yenisei is three miles wide at the Arctic Circle, and as fast as it rises the open water at the margin freezes up again and is soon covered with the drifting snow. During the summer which I spent in the valley of the Yenisei we had 6 feet of snow on the ground until the first of June. To all intents and purposes it was mid-winter, illuminated for the nonce with what amounted

to continuous daylight.   The light was a little duller at midnight, but not so much
so as during the occasional snowstorms that swept through the forest and drifted
up the broad river bed   During the month of May there were a few signs of the
possibility of some mitigation of the rigours of winter.   Now and then there
was a little rain, but it was always followed by frost.   If it thawed one day
it froze the next, and little or no impression was made on the snow.   The
most tangible signs of coming summer was an increase in the number of birds,
but they were nearly all forest birds, which could enjoy the sunshine in the
pines and birches, and which were by no means dependent on the melting
away of the snow for their supply of food.   Between May 16 and 30 we had
more definite evidence of our being within bird flight of bare grass or open
water.   Migratory flocks of wild geese passed over our winter quarters, but if
they were flying north one day they were flying south the next, proving beyond
all doubt that their migration was premature.   The geese evidently agreed with
us that it ought to be summer, but it was as clear to the geese as to us that
it really was winter.

W̄e afterwards learnt that during the last ten days of May a tremendous
battle had been raging 600 miles as the crow flies to the southward of our
position on the Arctic Circle.   Summer in league with the sun had been fighting
winter and the north wind all along the line, and had been as hopelessly beaten
everywhere as we were witnesses that it had been in our part of the river.   At
length, when the final victory of summer looked the most hopeless, a change was
made in the command of the forces.   Summer entered into an alliance with the
south wind.   The sun retired in dudgeon to his tent behind the clouds, mists
obscured the landscape, a soft south wind played gently on the snow, which melted
under its all-powerful influence like butter upon hot toast, the tide of battle was
suddenly turned, the armies of winter soon vanished into thin water and beat a
hasty retreat towards the pole.   The effect on the great river was magical.   Its
thick armour of ice cracked with a loud noise like the rattling of thunder, every
twenty-four hours it was lifted up a fathom above its former level, broken up, first
into ice floes and then into pack ice, and marched down stream at least a hundred
miles.   Even at this great speed it was more than a fortnight before the last strag-
gling ice-blocks passed our post of observation on the Arctic Circle, but during
that time the river had risen 70 feet above its winter level, although it was three
miles wide, and we were in the middle of a blazing hot summer, picking flowers of
a hundred different kinds, and feasting upon wild ducks' eggs of various species.
Birds abounded to an incredible extent.   Between May 29 and June 18 I identified
sixty-four species which I had not seen before the break up of the ice.   Some of
them stopped to breed and already had eggs, but many of them followed the
retreating ice to the tundra, and we saw them no more until, many weeks after-
wards, we had sailed down the river beyond the limit of forest growth.

The victory of the south wind was absolute, but not entirely uninterrupted.
Occasionally the winter made a desperate stand against the sudden onrush of
summer.   The north wind rallied its beaten forces for days together, the clouds
and the rain were driven back, and the half-melted snow frozen on the surface.
But it was too late ; there were many large patches of dark ground which rapidly
absorbed the sun's heat ; the snow melted under the frozen crust, and its final
collapse was as rapid as it was complete.

In the basin of the Yenisei the average thickness of the snow at the end of
winter is about 5 feet.   The sudden transformation of this immense continent of
snow, which lies as gently on the earth as an eider-down quilt upon a bed, into an
ocean of water rushing madly down to the sea, tearing everything up that comes
into its way, is a gigantic display of power compared with which an earthquake
sinks into insignificance.   It is difficult to imagine the chaos of water which must
have deluged the country before the river beds were worn wide enough and deep
enough to carry the water away as quickly as is the case now.   If we take the
Lower Yenisei as an example it may be possible to form some conception of the
work which has already been done.   At Yeniseisk the channel is about a mile
wide, 800 miles lower down (measuring the windings of the river), at the

village of Kureika, it is about 3 miles wide, and, following the mighty stream for about another 800 miles down to the Brekoffsky Islands, it is nearly 6 miles wide. The depth of the channel varies from 50 to 100 feet above the winter level of the ice. This ice is about 3 feet thick, covered with 6 feet of snow, which becomes flooded shortly before the break up and converted into about 3 feet of ice, white as marble, which lies above the winter blue ice. When the final crash comes this field of thick ice is shatttered like glass. The irresistible force of the flood behind tears it up at an average rate of 4 miles an hour, or about a hundred miles a day, and drives it down to the sea in the form of ice floes and pack ice. Occasionally a narrow part of the channel or a sharp bend of the river causes a temporary check; but the pressure from behind is irresistible, the pack ice is piled into heaps, and the ice floes are doubled up into little mountains, which rapidly freeze together into icebergs, which float off the banks as the water rises. Meanwhile, other ice floes come up behind: some are driven into the forests, where the largest trees are mown down by them like glass, whilst others press on until the barrier gives way, and the waters, suddenly let loose, rush along at double speed, carrying the icebergs with them with irresistible force, the pent-up dam which has accumulated in the rear often covering hundreds of square miles. In very little more than a week the ice on the 800 miles from Yeniseisk to the Kureika is completely broken up, and in little more than another week the second 800 miles from the Kureika to the Brekoffsky Islands is in the same condition.

During the glacial epoch the annual fight between winter and the sun nearly always ended in the victory of the former. Even now the fight is a very desperate one within the Polar Circle, and is subject to much geographical variation. The sun alone has little or no chance. The armies of winter are clad in white armour, absolutely proof against the sun's darts, which glance harmlessly on six feet of snow. In these high latitudes the angle of incidence is very small, even at mid-day in midsummer. The sun's rays are reflected back into the dry air with as little effect as a shell which strikes obliquely against an armour plate. But the sun does not fight his battle alone. He has allies which, like the arrival of the Prussians on the field of Waterloo, finally determine the issue of the battle in his favour. The tide of victory turns earliest in Norway, although the Scandinavian Fjeld forms a magnificent fortress in which the forces of winter entrench themselves in vain. This fortress looks as impregnable as that on the opposite coast, and would doubtless prove so were it not for the fact that in this part of the Polar Basin the sun has a most potent ally in the Gulf Stream, which soon routs the armies of winter and compels the fortress to capitulate.

The suddenness of the arrival of summer in Siberia is probably largely due to the geographical features of the country. In consequence of the vastness of the area which is drained by the great rivers, and the immense volume of water which they have to carry to the sea, the break up of the ice in their lower valleys precedes, instead of being caused by, the melting of the snow towards the limit of forest growth. The ice on the affluents either breaks up after that on the main river, or is broken up by irresistible currents from it which flow up stream; an anomaly for which the pioneer voyager is seldom prepared; and when the captain has escaped the danger of battling against an attack of pack ice and ice floes from a quarter whence it was entirely unexpected, he may be suddenly called upon to face a second army of more formidable ice floes and pack ice from the great river itself, and if his ship survive the second attack a third danger awaits him in the alternate rise and fall of the tributary as each successive barrier where the ice gets jammed in its march down the main stream below the junction of the river accumulates until the pressure from behind becomes irresistible, when it suddenly gives way. This alternate advance and retreat of the beaten armies of winter continued for about ten days during the battle between summer and winter of which I was a witness in the valley of the Yenisei On one occasion I calculated that at least 50,000 acres of pack ice and ice floes had been marched up the Kureika. The marvel is what became of it. To all appearance half of it never came back. Some of it no doubt melted away during the ten days' marches and counter-marches,

some drifted away from the river on the flooded places, which are often many square miles in extent, some got lost in the adjoining forests, and was doubtless stranded amongst the trees when the flood subsided, and some was piled up in layers one upon the top of the other, which more or less imperfectly froze together and formed icebergs of various shapes and sizes. Some of the icebergs which we saw going down the main stream were of great size, and as nearly as we could estimate stood from 20 to 30 feet above the surface of the water. These immense blocks appeared to be moving at the rate of from 10 to 20 miles an hour. The grinding together of the sharp edges of the innumerable masses of ice as they were driven down stream by the irresistible pressure from behind produced a shrill rustling sound that could be heard a mile from the river.

The alternate marching of this immense quantity of ice up and down the Kureika was a most curious phenomenon. To see a strong current up stream for many hours is so contrary to all previous experience of the behaviour of rivers that one cannot help feeling continuous astonishment at the novel sight. The monotony which might otherwise have intervened in a ten-days march-past of ice was continually broken by complete changes in the scene. Sometimes the current was up stream, sometimes it was down, and occasionally there was no current at all. Frequently the pack ice and ice floes were so closely jammed together that there was no apparent difficulty in scrambling across them, and occasionally the river was free from ice for a short time. At other times the river was thinly sprinkled over with ice blocks and little icebergs, which occasionally ' calved ' as they travelled on, with much commotion and splashing. The phenomenon technically called ' calving ' is curious, and sometimes quite startling. It takes place when a number of scattered ice blocks are quietly floating down stream. All at once a loud splash is heard as a huge lump of ice rises out of the water, evidently from a considerable depth, like a young whale coming up to breathe, noisily beats back the waves that the sudden upheaval has caused, and rocks to and fro for some time before it finally settles down to its floating level. There can be little doubt that what looks like a comparatively small ice block floating innocently along is really the top of a formidable iceberg, the greater part of which is a submerged mass of layers of ice piled one on the top of the other, and in many places very imperfectly frozen together. By some accident, perhaps by grounding on a hidden sandbank, perhaps by the water getting between the layers and thawing the few places where they are frozen together, the bottom layer becomes detached, escapes to the surface, and loudly asserts its commencement of an independent existence with the commotion in the water which generally proclaims the fact that an iceberg has calved.

Finally comes the last march-past of the beaten forces of winter, the ragtag and bobtail of the great Arctic army that comes straggling down the river when the campaign is all over—worn and weather-beaten little icebergs, dirty ice floes that look like floating sandbanks, and straggling pack ice in the last stages of consumption that looks strangely out of place under a burning sun between banks gay with the gayest flowers, amidst the buzz of mosquitoes, the music of song birds, and the harsh cries of gulls, divers, ducks, and sandpipers of various species.

I have been thus diffuse in describing these scenes, in the first place, because they are very grand; in the second place, because they have so important a bearing upon climate, one of the great factors which determine the geographical distribution of animals and plants; and in the third place, because they have never been sufficiently emphasised.

# ADDRESS

## TO THE

# ECONOMIC SCIENCE AND STATISTICS SECTION

### OF THE

# BRITISH ASSOCIATION,

### BY

## Professor J. SHIELD NICHOLSON,

### PRESIDENT OF THE SECTION.

*The Re-action in favour of the Classical Political Economy.*

It may naturally be expected in the address which, as president of this section, I have the honour to deliver, that some attempt should be made at originality, or at any rate at novelty. Accordingly, I hope that I shall fall in with the traditions of my office by defending a series of paradoxes and by running counter to a variety of popular opinions. I will only premise that however paradoxical I may appear, and however much I may seem to strain at singularity, I shall speak always to the best of my ability with the utmost good faith, and I shall endeavour to give only the results of my most deliberate convictions.

The central paradox which I propose to defend—the root of the whole series—is that the so-called orthodox, or classical, political economy, so far from being dead, is in full vigour, and that there is every sign of a marked re-action in favour of its principles and methods. The singularity of my position may be indicated by a word and a phrase. The word is Saturn, the phrase ' we are all socialists now.' I shall try to show that the traditional English political economy has neither been banished to Saturn nor stifled by socialism, and that in fact it is stronger than ever. This renewed vigour is no doubt largely due to the attacks made upon it on all sides in increasing force for the last twenty years. The dogmatic slumber induced by popular approval has been rudely shattered, and although some of the more timid followers of the orthodox camp thought they had been killed when they were only frightened and awakened, the central positions are more secure than before.

Consider, in the first place, the question of scientific method and the closely allied question of the relation of political economy to allied sciences. The method practically adopted by Adam Smith and Ricardo, and reduced to scientific form by Mill and Cairnes, and quite recently and still more effectively by Dr. Keynes, must still be regarded as fundamental. It has survived and been strengthened by two distinct attacks. In the first place, the extreme advocates of the historical method attempted to reduce political economy to a branch of history and statistics. They were concerned to pile up facts and add up figures, and they seemed to think that no guiding principles were necessary. But compilations of this kind are, properly speaking, not even history, still less are they political economy. History does not consist simply in collecting facts; the facts must be grouped, arranged and connected in an orderly manner. A room-full of old newspapers is not history, though it may contain much material for history. There was really nothing new in this extreme form of the historical method. It was a reversion to a primitive type. The plan had been adopted by chroniclers time out of mind; they embedded facts, signs,

wonders and traditions, as the mud of a river embeds what happens to fall in it. The facts are the fossils of the historian, and he has to make a very few go a long way. In economic literature we have an example of this method in the 'Annals of Commerce' of Anderson and Macpherson. The simple device is to collect all the facts and opinions about Commerce all the world over, and arrange them under the year in which they happened. The basis of classification is time pure and simple, and at the best we have an imperfect collection of materials which must be sifted and weighed to be of any service.

Now compare this method of simple accumulation—this attempt to write a biography of Father Time as a man of business—with the historical method adopted by Adam Smith; at least two-thirds of the 'Wealth of Nations' is history, and it is history of the first rank, and it is so because it is history that is introduced for the illustration, confirmation, or qualification, as the case may be, of principles. It does not follow because the principles are fundamental that the facts are warped and distorted; it simply means that the facts are made intelligible. Take, for example, his account of the economic aspects of the feudal system. He brushes away the technicalities and looks into the inner life as easily as William the Conqueror at the Council of Salisbury. Or, to take a modern instance, he is like a naturalist who puts aside the parts of the creature he does not want in order that he may see what he does want more clearly. This is a very different matter from suppressing truth and warping facts to suit preconceived opinions. It is needless to say that Adam Smith made some mistakes, e.g. in the treatment of the mercantilists; it ought to be equally needless to say that he made some remarkable discoveries of the processes of economic development. Adam Smith also made large use of the comparative method; he literally ranged from China to Peru in his survey of mankind. What is the underlying assumption in this procedure? It is simply that in economic affairs, in matters of buying and selling in the widest sense of the terms, in satisfying wants by labour, in the accumulation of wealth, there are certain characteristics of human nature that may be regarded as fundamental. These are no doubt subject to modifications by other influences, but modification is not total suppression or eradication. How long would it take the Ethiopian to change his skin under a different climate? And is it not proverbial that human nature is more than skin-deep? I think the Ethiopian might become very pale in complexion long before he would learn to prefer low wages to high wages, and much labour to little labour. Economists may learn something from the poets. Why do the creations of the greatest poets live and move? Why do we assent at once to their reality? Simply because they are like ourselves, and we feel with Goethe that we ourselves could commit the same crimes in debasement, and achieve the same glory in exaltation, of spirit. The gods and goddesses, the sylphs and fairies, are only shadows. Can any man read Shakespeare or Homer—to say nothing of undoubted historical records—and deny that a large part of human nature, especially that part with which economists have to deal, is subject to but little variation? Knowledge grows and is handed on from age to age, and the power of man over nature steadily increases, but the feelings are renewed with every generation. The children of the nineteenth century may be precocious and priggish, but they are not nineteen centuries old. Let me remind you, though I am anticipating my argument, that the latest and most advanced scientific economics—that which the Austrian mathematicians have evolved out of the conception of utility—in reality lay more stress than Adam Smith did on the universality of the feelings of mankind. The only difference is that he knew that he was speaking plain prose, and they sometimes think they are only speaking subjective philosophy. In consequence, Adam Smith's men and women are more real and less uniform than the offspring of the new analysis. But the point of importance is the recognition of certain characteristics of human nature as fundamental; there is no other justification for the use of the comparative and historical methods in the broad manner of Adam Smith.

There are, however, still evidences in recent writers of the influence of that narrow view of history which tries to avoid principles, in order to make an impressionist record of facts. Impressionism may be good art, but it is bad science. Too much stress, for example, is laid on the mere enumeration of statutes and preambles,

and too little attention is given to the far more difficult question, How far was the law operative, and how far was the preamble a just description? But signs are not wanting that the broader method of Adam Smith is gaining ground. The work of Mr. Seebohm on the 'English Village Community' is a splendid example, worthy to be placed on a level with the best chapters of the 'Wealth of Nations'; and Dr. Cunningham throughout his excellent history has informed facts with principles.

But it is time to observe that the traditional method of English political economy was more recently attacked, or rather warped, in another direction. The hypothetical or deductive side was pushed to an extreme by the adoption of mathematical devices. I have nothing to say against the use of mathematics, provided always that the essential character of mathematics is borne in mind. Mathematics is a formal science that must get its materials from other sciences. It is essentially as formal as formal logic   The mathematician is an architect who must be provided with stones and wood and labour by the contractor. It is one thing to draw a plan, another to erect a building. In economics there are certain relations which are most easily expressed in mathematical form. One of my greatest obligations to Professor Marshall is that when I began the study of political economy at Cambridge some twenty years ago, he advised me to read Cournot. And before going further I should like to say that I think one of the greatest signs of power in Professor Marshall's 'Principles' is that he has transferred his mathematical researches and illustrations to appendices and foot-notes, and in his preface also he has admirably stated the limits and functions of mathematics in economic reasoning.   But less able mathematicians have had less restraint and less insight; they have mistaken form for substance, and the expansion of a series of hypotheses for the linking together of a series of facts. This appears to me to be especially true of the mathematical theory of utility. I venture to think that a large part of it will have to be abandoned. It savours too much of the domestic hearth and the desert island. I announced my intention at the beginning of running counter to some popular opinions. I ask for your patience and forbearance when I say that in my opinion the value of the work of Jevons as regards the main body of economic doctrine has been much exaggerated. I am ready to admit that much of his work in finance and currency and in many special problems is excellent. But he was, I think, too deficient in philosophical grasp and intellectual sympathy to give the proper place to a new conception: witness his treatment of Mill and Ricardo. Again, Jevons was not a mathematician of the first rank; he struggles with the differential calculus as a good man struggles with adversity. The older economists maintained that price was the measure, not of utility, but of value, and value could not be reduced simply to utility.   Things, they said, might have a high value in use and but little value in exchange.   Jevons, by making the distinction between final and total utility, thought that he had discovered a method by which utility might be measured by price. No doubt, if we make adequate hypotheses, qualifications, and explanations this may be done , and, in the same way, if we introduce enough cycles and epicycles we may explain or describe the motions of the stars. But price is essentially the expression of objective and not of subjective relations—that is the older view in modern phraseology; the attempt to make a kind of pre-established harmony between the two leads to unreality. Price depends upon demand and supply, and the degree of utility is one element affecting demand   In my view the distinction between final and total utility is of qualitative importance; it is of service in explaining the real advantage of exchange; although the essential character of this advantage has been explained by Adam Smith and his successors. The precision of the new phraseology, especially when translated into curves, gives definiteness and sharpness to the conceptions.   The subject is too intricate for more detailed consideration in this place.   I will only add that in my view Professor Marshall's criticism of Jevons may be carried much further, with a still further rehabilitation of Ricardo.

There is another direction in which I think the mathematical economists have wandered far from reality. I allude to the stress laid upon what are called mar-

ginal increments    There is a tendency to magnify the effects of the last portion of supply or the last expression of demand.  I will only say that this doctrine is very apt to run into the fallacy which may be popularly described as the tail of the dog fallacy—the idea being that the tail wags the dog and the tip of the tail wags the tail.

To resume in a sentence: the method of the so-called orthodox English economists has only been modified and supplemented, not revolutionised and supplanted, by the historical and mathematical methods of recent writers, and this, in my opinion, is being recognised more and more.

I pass on to consider a closely allied question—the question, namely, of the limitation of the boundaries of the subject-matter of political economy.  In my view one of the greatest merits of the orthodox economists was the careful distinction they drew between economic and other social sciences.  They refused to merge it in the misty regions of general sociology, and they excluded from its borders the rocks and quicksands as well as the green pastures of ethics and religions.  This specialisation, they argued, was necessary if any real advance was to be made beyond the expression of platitudes and sentiments.  They allowed that in practical social problems there were in general other considerations besides the purely economic ; but these they left to the jurist, the moralist, or the politician  For a time, however, especially under German influences, attempts were made to break down these boundaries, and the economist was elevated to the position of universal philanthropist and general provider of panaceas.  Mill himself was partly to blame for the excursions which he made into the applications of social philosophy to practice.  It is to these excursions we are indebted for the fantastical notion of the unearned increment, and the curious idea that it is the duty of people to leave the bulk of their money to the State, or rather the duty of the State to take it.  Fortunately, however, for the progress of economics, this ideal of breadth without depth has not become dominant, and any force it had is already spent.  The advances made in other social or less vaguely human sciences have been so great that the economist is obliged to exclude them from his domain.

Still to some extent the view prevails, especially in Germany, that it is the business of the economist to discover the general conditions of social well-being, and to show how they may be realised.  If such an attempt were seriously made it could only end in the projection of the personality of the writer into an ideal, and one ideal would succeed another like a set of dissolving views.  Suppose, for example, that I personally were to attempt to set up an ideal, and, not having imagination enough to create a new one, I were to turn to ancient Greece.  There is something very fascinating about the life of the typical Athenian in the best days of Athens.  Physical beauty and vigour were considered as essential as keenness of intellect, appreciation of the fine arts, and skill in oratory ; and this intense self-realisation was tempered by ardent patriotism and a strong sense of the duties of citizenship.  The principal blot, from the modern point of view, was the institution of slavery and the relegation of most industrial functions to slaves.  I might as an economist, if this breadth of view were justified, take it on myself to show how modern life might be Hellenised, and by leaving out slavery and introducing a little Christian charity a very pleasing ideal  might be made, and then I might go on to show what steps Government should take to realise this ideal.

In the meantime, however, my friend Dr. Cunningham might take as his type one of the equally fascinating religious communities of the Middle Ages, and by leaving out some of the superstitions and inserting a few Hegelian contradictions, he might construct an equally attractive ideal and proceed to direct the statesmen how it might be carried into practice.  But when all the other economists had worked out similar projects—Professor Sidgwick, for example, on the lines of Bentham, and Professor Edgeworth with his love of measurements on the lines of Pythagoras—the difficulty would arise, Who was to be the ultimate arbiter ? And to this question no one would accept the answer of the rest.

Perhaps it may seem that my illustration goes beyond the argument ; let me, then, state the position in general terms.  According to the traditional English view it is not the business of the economist to decide all the disputes that may

arise even regarding fundamental questions in ethics, religion, fine art, education, public law, administration—to decide, in a word, the first duty of man and the last duty of governments  His sphere is much more limited, and the limits have been indicated with tolerable precision by the classical English economists. Even in England, however, there has been a tendency in recent years to remove the old land-marks, and I do not mean simply on the part of socialists, but by those who in the main profess to accept the English traditions.

Just as the German idealists think it is the business of the economist to discover the way to the perfectibility of the species, the English realists impose upon him the duty of finding the road to the greatest happiness of the greatest number. In technical language political economy is the economy of utility. No doubt, at first sight, this aim seems to be both definite and practical. From the old inquiry, 'How nations are made wealthy,' to the new inquiry, 'How nations are made happy,' it seems a natural and easy transition. For the essence of wealth is to possess utility, to satisfy desires, to create happiness. It is obvious also that the happiness of a people depends largely on its economic conditions in the narrowest sense of the term , it depends, that is to say, on the amount and distribution of its material wealth. Accordingly it seems plausible to maintain that the economist ought to discover by his calculus of utility those principles of production and distribution that will lead to most happiness

Plausible and natural, however, as this transition from wealth to happiness may seem, it may readily lead to the abandonment of the central position of the classical economists. The steps are worth tracing. The first deduction made from the general principle of utility is that it obeys a law of diminishing return. Every additional portion consumed or acquired of any commodity gives a decreasing satisfaction, and passing through the point of satiety we reach the negative utility of being a nuisance  Illustrated by the usual curve this law assumes the character of a mathematical axiom.

The next step is to show that the rich man derives very little utility (or happiness) from his superfluity, whilst if his abundance were divided amongst the poor a great amount of happiness would be created. It seems to follow at once that, assuming an average capacity for happiness, the more equal the distribution of wealth the greater will be the happiness of the people. Never did any theory of equality assume such a simple and scientific form; it is like the advent of primitive Christianity in the guise of a new philosophy.

The practical question remains, 'How is this ideal to be carried out ? '  Obviously it is too much to expect that the principle of natural liberty and the policy of *laisser-faire* may be left to work out this latter-day salvation. Competition may be well enough for the strong, but is the destruction of the poor and weak. Accordingly it seems easy to prove, or at least to presume, that great powers must be given to the State. It only remains to bring in the principle which Mill flattered himself was his chief contribution to economic theory, viz., that the distribution of wealth depends entirely on the opinions of mankind, that these opinions are indefinitely pliable, and that, therefore, no schemes of distribution can be called impracticable, and we arrive at the conclusion of the whole matter. And practically that conclusion is nothing less than State Socialism.

It needs no demonstration, however, that nothing could be more opposed to the traditional English political economy. What, then, becomes of my contention that it remains unshaken, and that there are signs of a strong reaction in its favour? The truth is that this conclusion has again brought into prominence other portions of the old doctrine that had been allowed to fall into the background. We are confronted with the limited power of the State and the infinite variety of individual enterprise. To the older economists the difference seemed so great that they considered the presumption against State interference to be established  The rule, it is true, was never absolute and unqualified. Adam Smith himself indicated some of the most important of these exceptions, and the list has been extended by his successors. But these exceptions were all based upon reasoned principles, such as the incapacity of the persons concerned, *e.g.*, children to make fair contracts, the lack of individual interest in public works, *e.g.*, the maintenance of roads, and

the importance of the highest security, as in the regulation of the issues of bank notes. And in spite of all these exceptions—strengthened and purified by these exceptions—the presumption remained undisturbed. Recently, however, some writers, under the influence of the ideal of maximum happiness and impressed by the power of the State, have sought to extend its interference far beyond these admitted principles. But I venture to say, so far as this movement has any theoretical support, the reaction has already begun.

The fundamental importance of freedom of contract has become more apparent than ever through the application of the comparative and historical methods to jurisprudence; the proposition that the progress of society has been from status to contract has almost acquired the force of an axiom. The analysis, too, of modern industrial systems in which division of labour has become more and more intricate and interdependent, has shown the hopelessness of the attempt to transfer the management and control to the State. Changes in the methods of production, in the diffusion of knowledge, and in the transport of material commodities, have been so rapid and so great that no executive government could have overtaken them. In the most advanced communities, even that legislation which is necessary for the new conditions lags behind; even those elementary forms which simply aim at giving an interpretation to contracts in doubtful cases, or which are necessary for the adjustment of responsibility (as in bankruptcy and partnership), are behind the times. The growth of joint-stock enterprise has outstripped the development of the law of companies, and there is a crop of new frauds without corresponding penalties.

Turning to the executive and administrative functions of government, the analysis of existing conditions shows that we have not yet overtaken those exceptions admitted by the strongest supporters of *laisser-faire*. The British government has, it is true, wasted its energies in devising temporary expedients of various kinds, but it has not yet accomplished the programme of Adam Smith. Not only are there privileges and restrictions that ought to have been abolished long ago, but on the positive side the programme is not complete. We have just begun universal education on the lines laid down by Adam Smith, but his scheme for Imperial federation is not yet within the range of practical politics. We have effected great financial reforms, but we still fall far short of the full development of his principles. Even in matters of currency and banking—in relation to which the function of the State has always been recognised—we are lamentably in need of reform.

But if the State cannot overtake those duties which are so necessary and persistent that they were forced on the attention of the strongest supporters of *laisser-faire*, how can we possibly justify the assumption of new functions which rest upon no better principle than the vague idea that the State ought to do something?

This leads me to observe that not only theoretically but practically signs of a reaction in favour of the old position are rapidly increasing. The experiments already made at playing the *rôle* of omnipotence and omniscience, against which governments were so emphatically warned by Adam Smith, have begun to bring forth fruit after their kind—thorns that were carefully nursed by the legislature, instead of producing figs, have produced more thorns and worse thorns.

A principle of the widest application in ethics and politics as well as in economics, which may be described as the principle of formal justice, has begun to operate in a remarkable manner. A government which lends its power and assistance to one set of people must be prepared to act in a similar manner in all similar cases. If once this principle is abandoned, governmental action becomes either a matter of chance or depends upon clamour and jobbery. It is wonderful how quickly the human mind discovers analogies in grievances, and how soon one cry leads to another. Microbes are not more rapid and relentless in their multiplication. A plain man may have his doubts about the similarity of triangles and consent to arbitration on the question, but he has no doubt that for the purpose of governmental grants and aids his needs are similar to his neighbour's. And the plain man is right. How can we justify the use of State

credit for the purchase of lands in Ireland and fishing boats in Scotland if we are not prepared to give similar aid to the poor of England who are similarly situated? If we grant judicial rents in the country why not in the towns, and if we fix by law one set of prices why not all prices?

We must not be content with looking at the immediate effects of legislation; we must consider also the secondary and more remote consequences. If a legislator thinks that there are none of importance, let him read a chapter of Adam Smith—in the original and not in the stale pemmican of popular dogmatism. And if he still thinks that every law must be considered in isolation on its own merits, that it is a temporary remedy for a passing emergency, then let him resign his seat in Parliament; he has mistaken his vocation; in the name of common sense and the happiness of the greatest number let him cease to be a legislator and become a policeman.

There is an old fable about the gradual entrance, little by little, of the camel into the tent of the Arab. The British Government—I speak irrespectively of parties, for with the frankness of my old masters in political economy I make bold to say both are equally to blame—the British Government is beginning to find that the camel is getting too far into the tent. The admission of a single ear is nothing to the admission of the hump, and the knees, and the rest of the beast Now the ear may be interpreted to mean the grant of a few thousand pounds to Scottish fishers, the hump is universal old-age pensions at a cost of some fifteen or twenty millions a year, and for the knees you may take the nationalisation of land at a cost of some two thousand millions, and for the whole beast you have the complete Socialist programme. The conclusion that when the beast was in the Arab was out needs no interpretation.

Let us leave fables for something the exact opposite, namely taxes. It was a favourite doctrine of the old economists that taxes are a burden and the visits of the tax-gatherer are odious This doctrine also is beginning to reassert itself The State can do nothing without money, and it generally does things in the most expensive manner. Fortunately in this country we have not yet reached the limits of tolerable taxation, but at the present rate of growth of Imperial and local expenditure we are rapidly approaching those limits. Now, if there is one position that has been firmly established in theory and confirmed by the abundant experience of many nations, it is that excessive taxation is ruinous to a country We have to consider not only the net proceeds but the indirect cost in all its forms, not only the mere cost of collection but the effects on industry and on the energies of the people.

It may, of course, be replied that those who demand a large increase of expenditure for public purposes do not propose to tax the poor, but only to take the superfluities of the rich—to take, as is sometimes said, twenty shillings in the pound from that part of every income which extends above 400l. a year. The certain effect of this kind of taxation would be that in a very short time nobody would have more than 400l. a year, and the sources of taxation would dry up just as people had become used to and dependent on governmental assistance.

The general argument may be summarised in the favourite phraseology of the day. The utility of every increment of governmental work rapidly diminishes, and the disutility of every increment of taxation rapidly increases. Both propositions, I may add, were abundantly proved before the language I have just employed was invented, and the old language, if less scientific, conveyed a more emphatic condemnation.

I will conclude by calling your attention to one more position of the classical economists, and one that is the foundation of their whole system so far as they deal with the principles of governmental action. They maintain that even if the State could do something for individuals as cheaply and effectively as they could do it for themselves, it is in general better to trust to individual effort. The decisive consideration is the effect on the character and energies of the people. Self-reliance, independence, liberty—these were the old watchwords—not State reliance, dependence, and obedience. In the matter of pauperism, for example, they teach us to distinguish between the immediate effects of relief which may

be beneficial, and the effects of reliance on that relief which may be disastrous. They are bold enough to maintain that the condition of life of the dependent pauper should not be made by aids and allowances better than that of the independent labourer. They insist on the great historical distinction between the sturdy rogues and vagabonds—who can work and will not—and the impotent poor, the poor in very deed, who cannot support themselves. They look upon the payment of poor rates as they look upon other forms of taxation—namely, as the lesser of two evils; they do not try to persuade themselves and other people that it is a duty which is essentially pleasant. And I confess that I never yet met a man who had the audacity to assert that he enjoyed paying poor rates. But I have known many men who have given of their substance to a far greater extent with a cheerful spirit. It is the compulsion that sticks in the throat, and there is no more instructive chapter in economic history than that which describes the slow, painful processes by which Englishmen gradually adopted compulsory assessment for the relief of the poor. I shall be told that these old economic doctrines are cold and hard and opposed to the principles of Christian charity. The retort is easy : If Christian charity realised a tithe of its ideal there would be no need for relief on the part of the State. If I, too, may quote Scripture for my purpose I would say : Go to the ant, thou sluggard ! It does not take ten ants to relieve another ant, and in this land of ours there are more than ten professed Christians to every pauper.

It is time, however, to bring this discourse to an end and not to begin a sermon ; which, moreover, according to my masters the old economists, is beyond our domain. Yet I shall be bold enough to end with these words of advice: To the student I would say : Political economy has a vast literature, and you will not find all the good concentrated in the last marginal increment; you must master the old before you can appreciate the new; a portion of truth just re-discovered for the hundredth time by some amateur is not of such value as a body of doctrines that have been developed for more than a century by economists of repute. And to the legislator I would say : Vaster than the literature of political economy is the economic experience of nations ; the lessons to be learned from the multitudinous experiments of the past can never become antiquated, for they have revealed certain broad features of human character that you can no more disregard than the vital functions of the human body. Just as Harvey did not invent but discovered the circulation of the blood, so Adam Smith did not invent but discovered the system of natural liberty. And nothing has been better established than the position that legislation which neglects to take account of the liberties of individuals is foredoomed to failure. If they cannot break through the law they will get behind the law. The first duty of the legislator is to take account of the natural forces with which he must contend, and the classical economists have made a survey and estimate of these forces which, based as it is on the facts of human nature and the experience of nations, it would be wilful folly to overlook.

# ADDRESS

TO THE

# MECHANICAL SCIENCE SECTION

OF THE

# BRITISH ASSOCIATION,

BY

JEREMIAH HEAD, M.Inst.C.E., Past Pres.Inst.Mech.E., F.C.S.,

PRESIDENT OF THE SECTION.


THIS Section of the British Association for the Advancement of Science was founded with the object of making more widely known, and more generally appreciated, all well-ascertained facts and well-established principles having special reference to mechanical science.

As President of the Section for the year, it becomes my duty to inaugurate the proceedings by addressing you upon some portion of the scientific domain to which I have referred, and in which your presence here indicates that you are all more or less interested.

## MECHANICAL SCIENCE.

The founders of the British Association no doubt regarded the field of operations which they awarded to Section G as a not less purely scientific one than those which they allotted to the other Sections. And, indeed, mechanical science studied, say, by Watt was as free from suspicion of commercial bias as chemical science studied, say, by Faraday.

But whatever may have been the original idea, the practice of the Section has recently been to expend most of its available time in the consideration of more or less beneficial applications of mechanical science, rather than of the first principles thereof. Our Section has become more and more one of applied rather than of pure science. None of the other Sections is free from this fault, if fault it be (which I do not contend or admit), but Section G seems to me to be beyond all question, and beyond all others, the Section of applied science.

The charter of the Institution of Civil Engineers commences by reciting that the object of that society is 'the general advancement of mechanical science, and more particularly for promoting the acquisition of that species of knowledge which constitutes the profession of a civil engineer, being the art of directing the great sources of power in nature for the use and convenience of man.'

It seems that in 1828, when the Institution was incorporated, the term 'mechanical science' had a wider meaning than it is now usually understood to have. For, according to the charter, the art of directing the great sources of power in nature is only a particular species of knowledge which 'mechanical science' includes.

In 1836, or eight years later, the founders of our Section adopted the term without again defining it. Probably they accepted the careful definition of the Great George Street Institution. Time has shown the wisdom of that decision. For we civil engineers and other frequenters of Section G in active practice need far more knowledge than mechanical science can teach us in the ordinary or narrow sense of the term. Our art in its multifarious branches requires, if success is to be

attained, the acquisition and application of almost all the other sciences which belong to the fields of research relegated to the other Sections. For how could the gigantic engineering structures of modern times be designed without recourse to mathematics, or steam and other motors without a knowledge of physics, or modern metallurgical operations be conducted without chemistry, or mining without geology, or communications by rail, ship, and wire be established and carried on with all parts of the world without attention to geography, or extensive manufacturing enterprises be developed if the laws of economics were neglected?

As to biological studies, they seem at first sight to have but little to do with mechanical science. It might even be thought that the civil engineer could afford altogether to neglect this part of the work of the Association. But I trust I shall be able to show you before I finish that any such view is absolutely untenable.

### MECHANISMS IN NATURE.

Indeed, I hope, in the course of this address, to satisfy you that mechanical science is largely indebted to mechanisms as they exist in nature, if not for its origin, at all events for much of its progress hitherto, and that nature must still be our guide.

Mechanical science has been built up entirely upon observation and experiment, and the natural laws which have been induced therefrom by man. The lower animals in their wild condition work with tools or appliances external to their bodies to but a very slight extent, and man in a primitive or savage state does the same. But many, if not most, animals can be taught to use mechanisms if carefully trained from infancy. Thus, the well-known donkey at Carisbrooke Castle draws water from a deep well by a treadmill arrangement just as well as a man could do it. He watches the rope on the barrel till the full pail rises above the parapet of the well, then slacks back a little to allow it to be rested thereon, and only then leaves the drum and retreats to his stable. But, according to his attendant, four years were needed for his education, and unless it had been commenced early it would have been useless

I have seen a canary gradually lift from a little well, situated a foot below its perch, a thimble full of water by pulling up with its beak, bit by bit, a little chain attached to it, and securing each length lifted with its foot till it could take another pull. When the thimble reached its perch level the bird took a drink, and then let it fall back into the well Numerous other examples will doubtless occur to you.

But though animals can be taught to make use of mechanical appliances provided for them—a fact which shows the existence in their brains of a faculty corresponding in kind, if not in degree, to the mechanical faculty in man—they rarely, on their own initiative, make use of anything external to their bodies as tools; and still more rarely, if ever, do they make, alter, or adapt such mechanical aids. Mr. C. Wood, of Middlesbrough, informs me that certain crows which frequent oyster-beds on the coast of India, wait until the receding tide uncovers the oysters, which still remain open for a time. A crow will then put a pebble inside one, and, having thus gagged it and secured his own safety, will proceed to pick it out and eat it at leisure. A monkey will crack a nut between two stones, and will hurl missiles at his enemies. But in some countries he is systematically entrapped by tying to a tree a hollow gourd containing rice, and having a hole large enough for his hand, but too small for his clenched fist, to pass through. He climbs the tree and grasps the rice, and remains there till taken, being too greedy, and not having sufficient sense, to let go the rice and withdraw his hand.

This is on a par with the snuff-taking imbecile, described by Hugh Miller,[1] whom the boys used to tease by giving him a little snuff at the bottom of a deep tin box. The imbecile would try to get at it for hours without the idea ever occurring to him that he might achieve his object by turning the box upside down.

All animals are, however, in their bodily frames, and in the intricate processes

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[1] *My Schools and Schoolmasters*, by Hugh Miller.

and functions which go on continuously therein, mechanisms of so elaborate a kind that we can only look and wonder and strive to imitate them a little here and there. The mechanism of their own bodily frames is that with which the lower animals have to be content, and whilst they are in the prime of life and health, and in their natural environment, it is generally sufficient for all their purposes. Man has a still more perfect, or rather a still more versatile bodily mechanism, and one which in a limited environment would be equally sufficient for his needs. But he has also an enterprising and powerful mind which impels him to strive after and enables him to enjoy fields of conquest unknown to, and uncared for, by the relatively brainless lower animals.

Urged on by these superior mental powers, man must soon have perceived that by the use of instruments he could more quickly and easily gain his ends, and he would not be long in discovering that certain other animals, such as the ox and the horse, were teachable and his willing slaves, provided only he fed and trained them and treated them kindly.

First, in common with other animals, he would find out that stones and sticks were of some use as weapons and tools; then he would go further and utilise skins and thongs for clothing and harness, and by selecting and modifying his stones and sticks he would form them into rough implements, which would enable him to cut down trees and to make rude huts and boats. Animals caught and domesticated would first be taught to haul light logs along the ground, then to move heavier ones on rollers, and later, in order to avoid the necessity for continual replacement of the rollers, the wheel and axle would be gradually developed.

The mechanical nomenclature of all languages is largely derived from the bodies of men and other animals From this it is clear that animals have always been recognised as mechanisms, or as closely related thereto The names borrowed from them generally indicate a resemblance in form rather than in function, though not invariably so.

Thus in our own language we have the 'head' of a ship, a river, a lake. a jetty, a bolt, a nail, a screw, a rivet, a flight of stairs, and a column of water, the brow of an incline; the crown of an arch, the toe of a pier; the foot of a wall; the forefoot, heel, ribs, waist, knees, skin, nose, and dead eyes of a ship; also turtle backs and whale backs; the jaws of a vice; the claws of a clutch; the teeth of wheels, necks, shoulders, eyes, nozzles, legs, ears, mouths, lips, cheeks, elbows, feathers, tongues, throats, and arms; caps, bonnets, collars, sleeves, saddles, gussets, paddles, fins, wings, horns, crabs, donkeys, monkeys, and dogs; flywheels, running nooses, crane necks, grasshopper engines, &c.

Not only has our mechanical nomenclature been largely taken from animals, but many of our principal mechanical devices have pre-existed in them. Thus, examples of levers of all three orders are to be found in the bodies of animals. The human foot contains instances of the first and second, and the forearm of the third order of lever. The patella, or knee-cap, is practically a part of a pulley. There are several hinges and some ball-and-socket joints, with perfect lubricating arrangements. Lungs are bellows, and the vocal organs comprise every requisite of a perfect musical instrument. The heart is a combination of four force-pumps acting harmoniously together. The wrist, ankle, and spinal vertebræ form universal joints. The eyes may be regarded as double-lens cameras, with power to adjust focal length, and able, by their stereoscopic action, to gauge size, solidity, and distance. The nerves form a complete telegraph system with separate up and down lines and a central exchange. The circulation of the blood is a double-line system of canals, in which the canal liquid and canal boats move together, making the complete circuit twice a minute, distributing supplies to wherever required, and taking up return loads wherever ready without stopping. It is also a heat-distributing apparatus, carrying heat from wherever it is generated or in excess to wherever it is deficient, and establishing a general average, just as engineers endeavour, but with less success, to do in houses and public buildings. The respiratory system may be looked upon as that whereby the internal ventilation of the bodily structure is maintained. For by it oxygen is separated from the air and imparted to the blood for conveyance and use where needed, whilst at the same time the

products of combustion are extracted therefrom and discharged into the atmosphere.

Mastication, which is the first process in the alimentary system, is, or rather should be, a perfect system of cutting up and grinding, and to assist and save animal, and especially human, mastication is the chief aim and object of all the gigantic milling establishments of modern times. The later alimentary processes are rather chemical than mechanical, but still the successive muscular contractions, whereby the contents of the canal are forced through their intricate course, are distinctly mechanical, and may have suggested the action of various mechanisms which are used in the arts to operate on plastic materials, and cause them to flow into new forms and directions.

The superiority of man to the lower animals can only have become conspicuous and decided when he began to use his inventive faculties and to fashion weapons and implements of a more efficient kind than the sticks and stones which they also occasionally use.

But human races and individuals were never equally endowed by nature. Some individuals would have greater inventive powers than others, and these and their posterity would gradually become dominant races Large masses of mankind are still more or less in the position of primeval man, which, if we accept the conclusions of Darwin, Lubbock, and other modern scientists, we must regard as one of barbarism. For they are still without tools, appliances, and clothes, except of the most elementary kinds, and mechanical science might almost be non-existent, so far as they are concerned.[1]

It would obviously be impossible for me to treat of or call attention even to an infinitesimal extent to the results of mechanical science which surround us now so profusely, and which make our life so different from that of primeval man ; and, even if it were possible, it would be quite unnecessary. We have all grown up in a mechanical age. We are so familiarised with artificial aids that we have come to regard them as part of our natural environment, and their occasional absence impresses us far more than their habitual presence.

I propose, with your leave, to proceed to the consideration of how far man is, in his natural condition, and has become by aid of mechanical science, able to compete successfully with other and specially endowed animals, each in its own sphere of action.

### BODILY POWERS OF MAN AND OTHER ANIMALS.

The bodily frame of man is adapted for life and movement only on or near to the surface of the earth. Without mechanical aids he can walk for several hours, at a speed which is ordinarily from 3 to 4 miles per hour. Under exceptional circumstances he has accomplished over 8 miles[2] in one hour, and an average of $2\frac{3}{4}$ miles per hour for 141 hours.[3] In running he has covered about $11\frac{1}{2}$ miles in an hour. In water he has proved himself capable of swimming 100 yards at the rate of 3 miles per hour, and 22 miles at rather over 1 mile per hour. He can easily climb the most rugged mountain path and descend the same. He can swarm up a bare pole or a rope, and when of suitable physique and trained from infancy can perform those wonderful feats of strength and agility which we are accustomed to expect from acrobats. He has shown himself able to jump as high as 6 feet $2\frac{3}{4}$ inches from the ground, and over a horizontal distance of 23 feet 3 inches, and has thrown a cricket-ball as far as $382\frac{1}{2}$ feet before it struck the ground.[4]

The attitude and action of a man in throwing a stone or a cricket-ball, where he exerts a considerable force at several feet from the ground, to which the reaction has to be transmitted and to which he is in no way fastened, are unequalled in any artificial machine. The similar but contrary action of pulling a rope horizontally, as in ' tug of war ' competitions, is equally remarkable.

[1] Mr H L Lapage, M.Inst C E , who has just returned from Western Australia states that he found the natives of both sexes and all ages absolutely nude
[2] *Whitaker's Almanack*, 1893, p 395
[3] Recent pedestrian race from Berlin to Vienna.
[4] *Chambers' Encyclopædia*, ' Athletic Sports '

So also the power of the living human mechanism to withstand widely diverse and excessive strains is altogether unapproachable in artificial constructions. Thus, although fitted for an external atmospheric pressure of about 15 lb. per square inch, he has been able, as exemplified by Messrs. Glaisher and Coxwell in 1862, to ascend to a height of 7 miles, and breathe air at a pressure of only $3\frac{1}{2}$ lb. per square inch, and still live. And, on the other hand, divers have been down into water 80 feet deep, entailing an extra pressure of about 36 lb. per square inch, and have returned safely. One has even been to a depth of 150 feet, but the resulting pressure of 67 lb. per square inch cost him his life.[1]

Recent fasting performances (if the published records are to be trusted) are not less remarkable when we are comparing the human body as a piece of mechanism with those of artificial construction. For what artificial motor could continue its functions forty days and nights without fuel, or, if the material of which it was constructed were gradually consumed to maintain the flow of energy, could afterwards build itself up again to its original substance?

These and other performances are, when considered individually and separately, often largely exceeded by other animals specially adapted to their own limited spheres of activity. The marvel is not, therefore, that the human bodily mechanism is capable of any one kind of action, but that, in its various developments, it can do all or any of them, and also carry a mind endowed with far wider powers than any other animal.

Animals other than man are also adapted for life and movement on or about the surface of the earth. This includes a certain distance below the ground, as in the case of earthworms; under the water, as in the case of fish; on the water, as in the case of swimming birds; and in the air, as with flying birds.

As far as I know, no animal burrows downwards into the earth to a greater depth than 8 feet,[2] and then only in dry ground. Man is naturally very ill-adapted for boring into the earth as the earthworm does. Indeed, without mechanical aids he would be helpless in excavating or in dealing with the accumulations of water which are commonly met with underground. But by aid of the steam-engine for pumping, for air-compressing, ventilating, hauling, rock-boring, electric lighting, and so forth, and by the utilisation of explosives, he has obtained a complete mastery over the crust of the earth and its mineral contents, down to the depth where, owing to the increase of temperature, the conditions of existence become difficult to maintain.

I have said that on land man, unaided by mechanism, has been able to cover about $11\frac{1}{2}$ miles in one hour. Two miles he has been able to run at the rate of nearly 13 miles per hour, and 100 yards at the rate of over 20 miles per hour.[3] But the horse, though he cannot walk faster than man, nor exceed him in jumping heights or distances, can certainly beat him altogether when galloping or trotting. A mile has been galloped in 103 seconds, equal to 35 miles per hour, and has been trotted in 124 seconds, equal to 29 miles per hour.[4]

There are many other animals, such as ostriches, greyhounds, antelopes, and wolves, which run at great speeds, but reliable records are difficult to obtain, and are scarcely necessary for our present purpose.

MECHANICAL AID WITHOUT EXTRANEOUS MOTIVE-POWER.

Let us now consider how man's position as a competitor with other animals in speed is affected by his use of mechanical aids, but without any extraneous motive-power.

*Locomotion on Land.*—Where there is a stretch of good ice, and he is able to bind skates on his feet, he can thereby largely augment his running speed. This was exemplified by the winner of the match for amateurs at Haarlem last winter, who accomplished the distance of 3·1 miles at the rate of about 21 miles per hour.

[1] *Pall Mall Gazette*, July 5, 1893, p. 8.
[2] *Vegetable Mould and Earthworms*, by Charles Darwin, p. 111.
[3] *Chambers' Encyclopædia*, 'Athletic Sports.'
[4] *Ibid*, 'Horse'

But the most wonderful increase to the locomotive power of man on land is obtained by the use of the modern cycle. Cycling is easily performed only where roads, wind, and weather are favourable. But similar conditions must also be present to secure the best speed of horses, with which we have been making comparison. One mile has been cycled at the rate of 27·1 miles per hour,[1] 50 at 20,[2] 100 at 16 6,[2] 388 at 12·5,[3] and 900 at 12·43 [1] miles per hour.

The recent race between German and Austrian cavalry officers on the high road between Vienna and Berlin has afforded an excellent opportunity to judge of the speed and endurance of horses as compared with men over long distances. Count Starhemberg, the winner, performed the distance, about 388 miles, in 71·33 hours, equal to 5 45 miles per hour. He rested only one hour in twelve. His horse, though successful, has since died.[4]

Lawrence Fletcher cycled, also along the high roads, from Land's End to John o' Groats' house, 900 miles, in 72·4 hours, equal to 12·43 miles per hour, or more than double the distance that the Count rode, and at above double the speed. To the best of my knowledge he still lives, and is no worse for his effort. The horse in this case would have to carry extra weight equal to one-sixth of his own, and the cyclist equal to a quarter of his own. But the horse carried himself and his rider on his own legs, while the cyclist made his machine bear the weight of itself and rider. Herein was probably the secret of his easy victory

With the very remarkable exception of long-distance cycling, which is of limited application, man, relying on his own bodily strength, cannot successfully compete with other animals which, like the horse, are specially fitted for rapid land locomotion. His only alternatives are either to utilise the horse and ride or drive him, and so get the benefit of his superior strength and speed, or to use his own inventive faculty and construct appliances altogether apart from animal mechanisms. In either case he virtually gives up the contest as a self-moving animal, and to a great extent abandons himself to be carried by others or by inanimate machinery.

Nearly seventy years ago mankind came to this conclusion, and the modern railway system is the result. The locomotive will go at least double the speed of the racehorse. It will carry not only itself but three or four times its own weight in addition, and will go not 2 or 3, but 100 miles or more without stopping, if only the road ahead be clear. And the iron horse is fed and controlled without even so much exertion as that put forth by a man on a horse of flesh and bone.

*Locomotion in Water.*—Let us now consider the powers of man relatively to other animals in moving upon and through the great waters with which three-fourths of the earth's surface is covered. Here he is in competition with fishes, aquatic mammals, and swimming birds.

I have already stated that, unaided by mechanism, he has shown himself able to swim for short distances at the rate of 3, and long distances (22 miles) at the rate of 1 mile per hour. He has also given instances of being able to remain under water for 4½ minutes.[5]

Credible eye-witnesses inform me that porpoises easily overtake and keep pace with a steamer going 12½ knots, or, say, over 14 miles per hour, for an indefinite length of time. This is five and fifteen times the maximum swimming speed of a man for short and long distances respectively. No doubt the form and surface of a fish whose main business is swimming offer less resistance, and his muscular power is more concentrated and better applied towards propulsion in water than is the case with man, whose body is also adapted for so many other purposes.

I am further informed by Mr. Nelson, of Redcar, a naturalist who has made the experiment, that it is impossible for an ordinary sea-boat rowed by two men and going at 5 miles per hour, to overtake the aquatic bird called the Great Northern Diver, when endeavouring to make his escape by alternately swimming

[1] *Whitaker's Almanack*, 1893          [2] *Chambers' Encyclopædia*, ' Cycling '
[3] *Times*, Sept  26 to Oct. 7, 1892          [4] Vienna-Berlin Race, June 1893
[5] *Whitaker's Almanack*, 1893.

on the surface and diving below. His speed is therefore nearly double the short and five times the long distance speed of unaided man in water. As regards remaining under water, fishes properly so-called have unlimited powers, and even aquatic mammals, such as whales, can remain under for $1\frac{1}{2}$ hours.

Using only his own strength, but assisting himself with mechanical devices, man has been able to increase considerably his speed as a swimming animal. Mr. John McCall, of Walthamstow, informs me that in 1868 he constructed and repeatedly used an apparatus which acted like the tail of a fish It consisted of a piece of whalebone, having a broad yet thin and elastic blade, tapering into a shank like the end of an oar. The blade was 15 inches wide and 4 feet long, including the shank. To the end of the latter a horizontal cross-bar 13 inches long was fitted, and leather pockets were provided at the ends for the feet. By swimming on his back and striking out alternately with his legs, he was able, with the assistance of this apparatus, to keep up with a sea-boat pulled by two men at about 4 miles per hour.

By means of boats, which he propels by oars or sculls, and notwithstanding the increased weight, and therefore displacement, involved by them, man has been able to increase his speed on the surface of the water to a maximum of about 12 miles per hour for about 4 miles distance, under favourable circumstances. So, by supplementing his bodily powers by means of mechanical aids, such as the diving-bell and the diving-helmet, dress, and air-pump, or by the portable self-acting apparatus used with such good effect in the construction of the Severn tunnel, man has been able to approach very nearly to the natural diving powers of, at all events, aquatic mammals, except that he cannot move about in subaqueous regions with anything approaching their ease and celerity.

Invariably on water, as almost invariably on land, man is quite unable to compete in power of locomotion with other specially adapted animals, whether or not he avails himself of mechanical aids, so long as his own bodily strength is the only motive-power he employs. He has gradually come to recognise this fact, and to see that he must use his inventive faculties and find new and powerful motors external to himself if he would really claim to dominate the great waters of the earth.

The fastest mechanism of any size, animal or man-made, which, as far as I know, has ever cut its way through the waters for any considerable distance is the torpedo-boat, 'Ariete,' made by Messrs. Thornycroft & Son, of London, in 1887. It has a displacement or total weight of about 110 tons, and machinery capable of exerting 1,290 effective horse-power, or 11·7 horse-power per ton of weight or displacement; or, to put it in another form, an effective horse-power is by it obtained from a weight of 191 lb., which includes vessel, machinery, fuel, stores, and attendants. The speed accomplished at the trials of this little craft, being the average of six one-mile tests, was 26·18 knots, or 30 16 miles per hour.[1] As might be expected, it resembles a fish, in that its interior is almost exclusively devoted to the machinery and accessories necessary for propulsion. During the trials the water, fuel, stores, and other ponderable substances carried amounted to 17·35 tons Two similar boats were able to make the voyage to South America by themselves, though at much slower speed and replenishing their fuel on the way. No fish or swimming bird can match this performance. And inasmuch as 191 lbs of dead weight produced 1 horse-power, as compared with from 150 to 250 lbs. in certain flying birds, it would seem that with suitable adaptations the 'Ariete' might even have been made to navigate the air instead of the water.[2] But I will revert to this subject later on.

Where safety in any weather, and passenger- and cargo-carrying powers are aimed at, as well as, or prior to, the utmost attainable speed—and these must ever

[1] *Engineering*, July 15, 1887.

[2] M. Normand, of Havre, is building for the French Government two torpedo-boats, each having a displacement of 125 tons and 2,717 effective horse-power, or 21·7 horse-power per ton of displacement This is equivalent to 1 horse-power per 103 lbs, and is still within the limits of weight permissible for aerial flight. See *Times*, June 19, 1893.

be the leading features of ocean-transit steamers if they are to attain commercial success—there I must refer you to those magnificent examples of naval architecture which are more or less familiar to you all, and of which we, as a maritime nation, are so justly proud. If, for example, we turn our attention for a moment to the new Cunard liners, the 'Campania' and 'Lucania,' having each a weight or displacement of 18,000 tons and 24,000 effective horse-power, or 1·33 horse-power per ton of displacement, we shall find that, with the commercial advantages alluded to, they obtain a maximum speed of 22·5 knots, or about 26 miles per hour.

If, instead of 1 33 effective horse-power per ton of displacement, they were provided with eight times that amount, or 10·64 horse-power per ton, thereby sacrificing passenger and cargo accommodation and making them nearly as full of propelling machinery as the 'Ariete' torpedo-boat, and if it were then found possible to apply this enormous power effectively, then there is every reason to believe they would accomplish for short distances double the speed, or, say, 45 knots, or about 52 statute miles, per hour.

By inventing and utilising mechanical contrivances entirely independent of his own bodily strength, man can now pass over the surface of the waters at the rate of over 500 knots per day, and at the same time retain the comforts and conveniences of life as though he were on shore. He has in this way beaten the natural and specially fitted denizens of the deep in their own element, as regards speed and continuity of effort. But he is still behind them as to safety. We do not find that fishes or aquatic mammals often perish in numbers, as man does, by collisions in fogs, or by being cast on lee shores and rocks by stress of weather. Shall we ever arrive at the point of making ocean travelling absolutely safe? The Cunard Company is able to boast that from its commencement, fifty-three years ago, it has never lost a passenger's life or a letter, a statement which gives ground for hope that almost absolute safety is attainable. But, on the other hand, other owners of almost equal repute (not excluding the British Admiralty) are ever and anon losing magnificent vessels on rocks, in collisions, by fire, and even by stress of weather, in a way which makes us doubt whether it is possible for Britannia or any one else really to 'rule the waves.'

In one way the chances of serious disaster have been of late largely diminished, and here, again, Nature has been our teacher. The bodies of all animals except the very lowest are symmetrically formed on either side of a central longitudinal plane. Each important limb is in duplicate, and if one side is wounded the other can still act. We have at last found out the enormous advantage and increased safety of having the whole of our ship-propelling machinery in duplicate, and our ships made almost unsinkable by one longitudinal and numerous transverse bulkheads.

*Locomotion in Air.*—I now come to consider what is the position of man as regards locomotion in and through the great atmospheric envelope which surrounds the earth, in comparison with animals specially fitted by Nature for such work.

Nature seems never to bestow all her gifts on one individual or class of animals, and she never leaves any entirely destitute. For instance, the serpent, having no limbs whatever, would seem at first sight to be terribly handicapped; yet, in the language of the late Professor Owen, 'it can out-climb the monkey, out-swim the fish, out-leap the jerboa, and, suddenly loosing the close coils of its crouching spiral, it can spring into the air and seize the bird on the wing.'[1] Here we have the spiral spring in nature before it was devised by man.

Flying animals seem to conform remarkably to this law. Thus we have birds like the penguin, which dive and swim, but cannot fly; others, like the gannet, which dive, swim, fly, and walk; others, like the ostrich, which run, but can neither fly nor swim; and numberless kinds which can fly well, but have only slight pedestrian powers.

Man, unaided by mechanisms, can, as we have seen, walk, run, swim, dive, and jump, and perform many remarkable feats; but for flying in the air he is absolutely unfitted. All his attempts (and there have been many) have up to the present been unsuccessful, whether or not he has availed himself of mechanical aids to his

[1] Pettigrew on *Animal Locomotion*

own bodily powers. It is said that a certain man fitted himself with apparatus in the time of James VI. of Scotland, and actually precipitated himself from the cliff below Stirling Castle, in sight of the king and his courtiers; but the apparatus collapsed, and he broke his leg, and that was the end of the experiment.

But why should not man fly? It is not that he does not desire to do so. For every denizen of our precarious British climate, when he has noticed the ease with which swallows and other migratory birds fly off on the approach of winter, hundreds and even thousands of miles to the sunny south, must have wished he could do the same. One reason why we cannot fly, even with artificial aids, such as wings, is that, as in the case of the penguin or the ostrich, our bodily mechanism is specialised and our muscular power diffused in other directions, so that we could not actuate wings of sufficient area to carry us even if we had them.

M. de Lucy, a French naturalist, has shown that the wing-area of flying animals varies from about 49 square feet per lb of weight in the gnat, and 5 square feet in the swallow, to half a square foot per lb. of weight in the Australian crane, which weighs 21 lb. and yet flies well. If he were to adopt the last or smallest proportion, a man weighing 12 stone would require a pair of wings each of them 14 feet long by 3 feet broad, or double the area of an ordinary room door, to carry him, without taking into account the weight of the wings themselves.

In flying birds there is a strong tripod arrangement to secure firm points of attachment for the wings, and a deep keel in the breast-bone, to which the large pectoral muscles are secured. Think of the wings I have described and the absence of pivots, keel, and muscles in man, and it will be tolerably obvious why he cannot fly, even with artificial wings.

But it might be contended that a man's strength is in his legs rather than in his arms, and that it is conceivable that a successful flying-apparatus might be made if adapted for the most, instead of the least, favourable application of his bodily strength.

According to D. K Clark,[1] a labourer working all day exerts on an average ·13 horse-power. The maximum power of a very strong man for a very short time is ·46 horse-power.

According to Dr. Haughton,[2] the oarsmen in a boat-race of 1 mile, rowed in 7 minutes, exerted each ·26 horse-power.

Suppose we take the rowing case as the maximum maintainable for, say, 7 minutes, by a man weighing 168 lb. Then in flight he would have to sustain a weight of

$$\frac{168}{\cdot 26} = 646 \text{ lb.}$$

per horse-power exerted, besides the weight of the apparatus.

Now, we shall find later (see p. 12) that no birds support even half that weight per horse-power which they have the power to exert, and that recent aeroplane experiments prove its impossibility. On the ground, therefore, that he is too heavy in proportion to his strength, it is clear that man is unfitted for flight, as well as because his limbs are not adapted for it.

It does not follow, however, that by aid of mechanisms apart from his own body, and worked by power independent of his own strength, man may not imitate, compete with, and even outdo the fowls of the air.

Let us consider a few facts showing what birds can do. A gannet hovers in the air above the sea. Suddenly he nearly closes his wings, swoops down, and with a splash disappears below the surface. Shortly after he reappears with a fish in his mouth, which he swallows in a few gulps; then, after swimming on the surface a little, he reascends into the air to repeat the operation.

The swallow rises into the air with a few rapid movements of the wings, then slides down as though on an aerial switchback, and then up again till he nearly reaches his original height, or he circles round by raising one wing, like a runner rounding a curve.

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[1] *Rules, Tables, and Data*, pp 719 and 720, by D K. Clark.
[2] *Animal Mechanics*, by Dr. Haughton

The condor vulture, which measures sometimes 15 feet across the wings, will fly upwards till quite out of sight.

A flock of cranes have been seen migrating at a height of three miles, and proceeding apparently without any movement of the wings.

The peregrine falcon will swoop down upon a partridge, and, missing it by a doubling movement of the latter, will slide upwards, thus converting his kinetic into new potential energy. He will then turn and descend again, this time securing his prey.

Mr. J. E. Harting, one of the principal British ornithological authorities, has, after careful investigation, arrived at the conclusion that the speed of falcons in full flight is about 60 miles per hour.[1]

Mr. W. B. Tegetmeier, another well-known authority, gives[2] the results of a number of experiments on the speed of homing pigeons, made under the auspices of the United Counties Flying Club in 1883. The average speed of the winner in eighteen races was 36 miles, and the maximum 55 miles per hour. The greatest distance flown was 300 miles.

The albatross, the largest web-footed bird, measuring sometimes 17 feet from tip to tip of wing, and weighing up to 20 lb , frequently accompanies ocean steamers from the Cape to Melbourne, a distance of 5,500 knots, without being seen to rest on the way.

An American naturalist, Mr. J. Lancaster, who spent no less than five years on the west coast of Florida,[3] in order to study the habits of aquatic and other birds which frequent these shores, arrived at the following conclusions, viz. :—

Though all birds move their wings sometimes, many can remain indefinitely in the air, with wings extended and motionless, and either with or without forward movement. This he calls ' soaring.'

The wing-area of soaring birds varies from 1 to above 2 square feet per lb. of weight.

The larger the wings per lb. of weight, the greater the power to soar.

The heavier the bird, the steadier his movements.

Soaring birds always face the wind, which, if they do not move forward or downward, must not blow at a less speed than 2 to 5 miles per hour

Mr. Lancaster specially watched a flock of buzzards about 30 feet above his head, waiting for him to leave the body of a dead porpoise. Their wings were about 8 feet from tip to tip, and their average weight about 6 lbs. During three hours at mid day, when the wind which they faced was very strong, they flapped their wings about twenty times each. Later, during two hours, when the wind had subsided, they never moved them at all.

Mr. Lancaster timed frigate birds, and found them able to go at the rate of 100 miles per hour, and that on fixed wings ; he is of opinion that at all events up to that speed they can fly just as fast as they please. He says, further, that the same birds can live in the air a week at a time, night and day, without touching a roost, and that buzzards, cranes, and gannets can do the same for several hours at a time.

The observed facts relating to the phenomena of flight are still but very imperfectly understood. That a bird should be able to maintain a downward pressure on the air sufficient to counteract the effect of its own weight, and a backward pressure sufficient to force itself forward at such speeds as I have named, seems wonderful enough when it is known that it continuously operates its wings. But that it should be able to do the same without any muscular movement at all is almost incomprehensible. It seems to be an instance of the suspension of the laws of gravity and of the existence of cause without effect, and of effect without cause. It is not a case of floatation, like a balloon, for any bird falls to the earth like a stone when shot. Mr. Lancaster suggests that the bird's own weight is the force which enables him to counteract the effect thereof, but this explanation is, I confess, beyond my comprehension.

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[1] *Field*, December 5, 1891, p. 856.      [2] *Field*, January 22, 1887, p 114.
[3] ' Problem of the Soaring Bird,' *American Naturalist*, 1885, pp. 1055–1162.

It seems to me that for every pound of his weight pressing downwards there must be an equivalent force pressing upwards. This can be produced only by his giving downward motion to the air previously at rest, or by his arresting previous motion of air in an upward direction. The latter alternative involves the supposition that the air-currents which soaring birds face are not, as Mr. Lancaster believes, always horizontal, but must have, to some extent, an upward direction. If a parachute were falling in a current of air, which was moving upwards at the same rate as the parachute fell, it would obviously retain its level, yet gravity would be acting. So, if a bird with extended wings were sliding down a stream of air which was tending upwards at the same angle and same velocity, the phenomenon of soaring would be produced

*Weight of Birds in Relation to their Bulk.*—It is generally believed that birds are lighter, bulk for bulk, than other animals, and that to this lightness they owe, in some degree, their power of flight and of floating on water. To account for this it is said that their bone-cavities are filled with air, and that some, though not even all, flying birds have small air-sacs under the skin. It is clear, however, that displacement of external air by air-filled cavities can only assist aerial floatation to an infinitesimal extent, unless highly heated. Such cavities would, however, help aquatic birds to swim, if situated under the immersed portion of their bodies, which is not always the case

Some aquatic birds, such as swans, swim with head, neck, wings, tail, and half their bodies out of the water. The specific gravity of fishes and land animals is clearly about the same as water. For, when swimming, they can keep only a small portion of their heads above the surface, and that by continued exertion. Are, then, birds, in the substance of their bodies, less dense than other animals, although also composed of flesh, blood, and bone, and these components in similar proportions and of similar character and texture? If they are, then land animals might have been made lighter in proportion to their bulk or smaller in proportion to their weight than they have been. If they are not, how is it that some of them can swim and float high out of the water?

Having an opportunity recently of inspecting a large wild, or whooper, swan, I ascertained its weight to be 14 lb I noticed that the whole of the under-part of the body, which would be immersed when swimming, was covered with feathers and underlined with down to an average depth of not less than $1\frac{1}{2}$ inches, or, when closely pressed, say, $1\frac{1}{4}$ inches. The immersed surface I estimated at $1\frac{1}{2}$ square feet. The weight of water displaced by this feather and down jacket, and the consequent extra buoyancy produced thereby, was no less than 9·78 lb. This would account for two-thirds of the bird's body being out of water when swimming, even if the body were of the same specific gravity as water.

I next procured a freshly-shot wild duck, which weighed $2\frac{1}{2}$ lb., and placed it in a tank of sea-water. It floated. I found the area of its immersed surface to be 54 square inches, and the average depth of its under-feathers and down to be $\frac{3}{4}$ inch. The water displaced by this envelope would weigh 1 5 lb., and would support three-fifths of its entire weight. I then had it denuded of all its feathers and down, and again placed in the tank. It then slowly sank to the bottom.

These experiments, so far as they go, seem to prove conclusively that birds are are not lighter, bulk for bulk, than other animals, but, on the other hand, about the same specific gravity, and that their floating power lies entirely in the thick jacket or life-belt with which nature has furnished those, and those only, which are intended to swim.

Inasmuch, therefore, as the specific gravity of the actual bodies of all animals appears to be about the same, there is no reason to believe that any could have been constructed of lighter material or to lighter design.

*Weight in Relation to their Energy*—But notwithstanding this uniformity of specific gravity, there remains the curious fact that flying birds can exert continuously about three times the horse-power per lb. of weight that man can—and, indeed, about three times what is possible for the horse [1] This marvellous flow of energy in proportion to weight is probably due to rapidity of limb-action rather

than to increase of muscular stress. I have timed sea-gulls and found them to flap their wings two hundred times per minute when flying at about 24 knots per hour. and have estimated eider-ducks, making about 36 knots per hour, to be flapping their wings five hundred times in a minute. I say 'estimated,' for their movements are too rapid for precise counting. This outpouring of energy, which seems to me to be unequalled in terrestrial animals, is nevertheless maintained by birds for indefinitely long periods of time.

A proportionately increased rate of combustion and renovation of tissue as well as of food-consumption are necessary consequences. The higher temperature of the bodies of birds, as compared with other animals,[1] and the well-known voracity of those which, like sea-birds, are almost continuously on the wing, are circumstances which seem to point to the same conclusion. It is confirmed by what we know of steam and other motors. For instance, if a steamship were so built and proportioned that a ton of coal per hour consumed in the boilers would maintain the pressure at 100 lb. per square inch and produce 1,000 horse-power at the propeller; and then if, without other alteration, firing was slackened until the steam fell to 50 lb. per square inch, and there maintained, it is clear that the horse-power produced would be greatly lessened, and so would the temperature of the steam in the boilers, steam-pipes, and cylinders. Thus, other things being equal, the temperature of the steam would rise and fall with the energy given forth by the mechanism.

The suggestion is that the higher temperature of birds, as compared with other animals, is similarly connected with their superior power of producing and maintaining energetic effort.

### AERIAL NAVIGATION.

Let us now consider what man has done, and may be able to do in, aërial navigation by aid of contrivances which, as in the case of railway locomotives and ocean steamers, are propelled by a power other than that of his own body.

The scientific world is greatly indebted to Mr. Hiram S. Maxim, of London, for recording in a clear and readable form, the present position of aeronautic mechanisms.[2] So far, the only contrivances which have been fairly successful are balloons, which, unlike birds, depend on atmospheric displacement for their power of sustaining weight or rising or falling.

In balloon experiments our French neighbours have led the way, from the first attempt of the Montgolfier brothers in 1783. During the last twenty years they have made numerous experiments and substantial improvements. Captain Renard and other officers of the French army have constructed a fish-shaped apparatus, and inflated it with hydrogen. It is driven by an electric motor of $8\frac{1}{2}$ horse-power, and has sufficient buoyancy to carry two aeronauts and all necessary accessories. In fair weather Captain Renard has succeeded in travelling at the rate of $12\frac{1}{4}$ miles per hour, in steering in any direction, and even in returning to his point of departure The balloon, it is said, always keeps level, and so far there have not been any accidents, but no expedition has been attempted in wet or windy weather.

Except that a more powerful motor, going at a higher speed, might be fitted to such an apparatus, Mr. Maxim thinks that it is as near perfection as is ever likely to be reached by a machine depending on aerial flotation. He proceeds to give an account of some experiments made by Professor S P. Langley, of the Smithsonian Institute, Washington, and of others by himself, to ascertain how much power is required to produce artificial flight by means of aëro-planes, after the manner of birds, and whether such power can be obtained without exceeding the weight which it would itself sustain.

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[1] *Chambers' Encyclopædia*, 'Bird' and 'Animal Heat'; *Lehrbuch der Zoologie*, by Professor Hertwig, p 538.

[2] 'Progress in Aerial Navigation,' by Hiram S. Maxim, *Fortnightly Review*, October, 1892

He says that heavy birds, with relatively small wings, carry about 150 lb. per horse-power exerted, and birds such as the albatross and vulture probably about 250 lb. Professor Langley, with small slanting planes, was able to carry 250 lb. per horse-power exerted: and Mr. Maxim, using heavier weights in proportion to plane-area, 133 lb. per horse-power, and using lighter ones, nearly the same as Professor Langley.

Mr. Maxim has lately devoted his energies to constructing a motor which should meet the requirements of the case, and has succeeded, he says, in producing one: a steam-engine burning naphtha and with atmospheric condenser, within a total weight of 8 lb per horse-power. He thinks, however,[1] that by using light naphtha and its vapour in the boiler instead of water, as well as in the furnace as fuel, a weight as low as 5 lb. per horse-power may be reached.

Meanwhile Professor Langley's ideas have been embodied in an experimental flying-machine, a drawing and description of which will be found in the 'Daily Graphic' for July 1, 1893. The body, which resembles that of a bird and is 15 feet long, contains the propelling machinery in duplicate The wings, which are 40 feet across, are of China silk spread on a tubular framework, stiffened with wire trusses. The boilers use liquid fuel and contain a highly volatile fluid. The capabilities of the machine have not yet been practically tested.

Promising as are the results hitherto obtained, they are as yet far from placing us on a level with birds in power to utilise the atmosphere as a navigating medium. The absolutely necessary power of delicate guiding, in rising, falling, and turning, whatever the direction or force of the wind, has yet to be considered and worked out. What would happen in case of a temporary breakdown of the aero-plane machinery we shudder to think of.

An important step has been effected by the discovery that parachutes with tubular orifices at the top are comparatively safe appliances for descending to the earth from indefinitely high altitudes. Perhaps it may be arranged that each aeronaut should be able, at a moment's warning, to gird himself with one of these as with a life-belt on board ship, and so descend in safety, or one or more automatically opening in case of disaster might be fitted to the aero-plane as a whole.

### EVENTUAL EXHAUSTION OF FUEL-SUPPLY.

I have still to refer to one other question, the consideration of which must always give rise to very serious thoughts. We have seen that the decisive victories which, in modern times, man has gained over matter and over other animals, have been due to his use of power derived from other than animal sources. That power has invariably proceeded from the combustion and the destruction of fuel, the accumulations of which in the earth are necessarily limited.

Mechanical appliances, involving the consumption of fuel, have, for a century at least, been multiplying with alarming rapidity. Our minds have been set mainly on enlarging the uses and conveniences of man, and scarcely at all on economising the great sources of power in nature, which are now for the most part its fuels. Terrible waste of these valuable stores is daily going on in almost every department of use. Once exhausted they can never be replaced They have been drawn upon to some extent for 1,000 years, and extensively for more than 100. Authorities say that another 1,000 years will exhaust all the more accessible supplies But suppose they last 5,000 years—what then ? Why, then, as far as we can at present see, our only motive-powers will be wind and water and animals, and our only mode of transit, sailing and rowing, driving, cycling, riding, and walking.

Sir Robert Ball has estimated that in not less than 5,000,000 and not more than 10,000,000 years the sun will have become too cold to support life of any kind on this planet. Between the 5,000 years when fuel will certainly be exhausted and the 5,000,000 years when all life may be extinguished, there will still be 4,995,000 years when, according to present appearances, man will have to give up his hardly-earned victories over matter and other animals, and the latter will again surpass him, each in its own element, because he has no fuel.

[1] *Engineer*, January 13, 1893, p 28

## CONCLUSION.

Leaving to our posterity these more remote troubles, we may, I think, justly draw from the entire discussion the conclusion that we have still a great deal to learn from mechanisms as they exist in nature. Great as have been the achievements of man since he first began to study mechanical science, with a view to directing the great sources of power in nature for his own use and convenience, the entire field of research is by no means yet fully exhausted. We must continue to study the same science with undiminished ardour. In so doing we shall do well to bear in mind that success can be achieved only by the patient, accurate, and conscientious observation of the great facts of nature, which are equally open to us all and waiting for our attention; and by drawing correct inferences therefrom, and by applying such inferences correctly to the fulfilment of the future needs and destiny of our race.

# ADDRESS

## TO THE

# ANTHROPOLOGICAL SECTION

### OF THE

# BRITISH ASSOCIATION,

BY

## ROBERT MUNRO, M.A., M.D., F.R.S.E.,

PRESIDENT OF THE SECTION.

THE science of anthropology, in its widest sense, embraces all the materials bearing on the origin and history of mankind. These materials are so comprehensive and diversified, both in their character and methods of study, that they become necessarily grouped into a number of subordinate departments. From a bird's-eye point of view, however, one marked line of demarcation separates them into two great divisions, according as they relate to the structure and functions of man's body, or to the works he has produced—a classification well defined by the words *anthropology* and *archæology.* The former, in its limited acceptation, deals more particularly with the development of man—his physical peculiarities, racial distinctions, linguistic manifestations, mental endowments, and, in short, every morphological or mental modification he has undergone amidst the ever-changing phenomena of his environments. The latter, on the other hand, takes cognisance of man merely as a handicraftsman. During his long journey in past time he has left behind him, scattered on the highways and byways of primeval life, numerous traces of his ways, his works, his culture, and his civilisation, all of which fall to be collected, sorted, and interpreted by the skilled archæologist. In their general aspects and relationship to each other most of the leading subjects in both these branches of the science have already been expounded, in the presidential addresses of my predecessors, by men so distinguished in their respective departments that they have left little to be said by anyone who attempts to follow in their footsteps. There is, however, one phase in the progressive career of man which has not hitherto been so fully illustrated as the subject appears to me to merit. I refer to the direct and collateral advantages which the erect position has conferred on him; and to this I will now briefly direct your attention, concentrating my observations successively on the following propositions:—

(1) The mechanical and physical advantages of the erect position.

(2) The differentiation of the limbs into hands and feet.

(3) The relation between the more perfect condition of these organs and the development of the brain.

In the process of organic evolution it would almost appear as if nature acted on teleological principles, because many of her products exhibit structures which combine the most perfect adaptation of means to ends along with the greatest economy of materials. This is well exemplified in some of the structural details of the organs of locomotion in which many of the so-called mechanical powers may be seen in actual use. The primary object of locomotion was to enable the organism to seek its food over a larger area than was attainable by a fixed position. The acquisition of this power was manifestly so advantageous to animal life that

the principles by which it could be effected became important factors in natural selection. I need not here dwell on the various methods by which this has been accomplished in the lower forms of life, but proceed at once to point out that in the higher vertebrates the problem resolved itself into the well-known mechanism of four movable limbs, capable of supporting and transporting the animal. As these quadrupedal animals became more highly differentiated, in virtue of the necessities of the struggle for life and the different and ever-varying conditions of their surroundings, it followed that the limbs became also modified so as to make them suitable, not only for locomotion in various circumstances, but also useful to the animal economy in other ways. Hence they were subjected to an endless variety of secondary influences, which finally adapted them for such diverse purposes as swimming, flying, climbing, grasping, &c. The anterior limbs, owing to their proximity to the head, were more frequently selected for such transformations as may be seen, for instance, in the wings of a bird. But whatever modifications the fore limbs may have undergone, no animal, with the exception of man, has ever succeeded in divesting them altogether of their primary function. This exceptional result was due to the erect position, which necessitated a complete division of labour as regards the functions of the limbs—the two anterior being entirely restricted to manipulative and prehensile purposes, and the two posterior exclusively devoted to locomotion. Coincident with this notable specialisation of their function a new field for advancement was opened up to man, in which intelligence and mechanical skill became the leading factors in his further development.

Man is thus distinguished from all other animals by the fact that, in the normal position of walking or running, he carries his body upright, i.e., with the axis of the vertebral column perpendicular, instead of horizontal or oblique. In this position all its parts are so arranged as to require a minimum amount of exertion in the performance of their functions. If any of the other higher vertebrates should ever assume an erect attitude it can only be maintained temporarily, and its maintenance involves an additional expenditure of force. In a certain sense a bird may be looked upon as a biped, but there is this distinction to be drawn between it and man, viz., that the former has not only its body balanced obliquely on its two legs, but also its fore limbs converted into special organs for motion in the air. The anthropoid apes hold an intermediate position, and so carry their body in a semi-erect attitude. But this shortcoming in reaching the perfectly upright position, however slight it may be in some of these animals, represents a wide gap which can only be fully appreciated by a careful study of the physiological and psychological phenomena manifested in their respective life-functions.

Everyone acquainted with the ordinary operations of daily life knows how much labour can be saved by attention to the mere mechanical principles involved in their execution. In carrying a heavy load the great object is to adjust it so that its centre of gravity comes as nearly as possible to the vertical axis of the body, as otherwise force is uselessly expended in the effort to keep the entire moving mass in stable equilibrium—a principle well exemplified by the Italian peasant girl when she poises her basket of oranges on her head. Once upon a time a powerful waterman, accustomed to use buckets double the size of those of his fellow-watermen, had the misfortune to have one of them broken. As he could not, then and there, get another bucket to match the remaining one, and wishing to make the best possible use of the appliances at hand, he replaced the broken vessel by one half its size. He then filled both with water and attempted to carry them, as formerly, attached to a yoke, one on each side of him. But to his astonishment this arrangement would not work. The yoke became uneven, and the effort to keep it balanced on his shoulders was so troublesome that he could not proceed. This emergency led to serious reflection, but, after some experimental trials, he ascertained that, by merely making the arm of the yoke on which the small bucket was suspended double the length of the other, he could carry both buckets without inconvenience.

But let me take one other illustration. Suppose that two burglars have concocted a plan to rob a richly-stored mansion by getting access to its rooms through

the windows of an upper story. In order to carry out this design they secure a ladder, easily transported by the two together though too heavy for one. So, bearing the ladder between them one at each end, they come to the house. After a considerable amount of exertion they succeed in placing the ladder in an upright position against the wall, and then one of the men mounts its steps and enters the house  The man left outside soon realised that, once the ladder was balanced perpendicularly, he himself could then easily control it. Moreover, he made the discovery that by resting its weight on each leg alternately, he could gradually shift its position from one window to another. Thus there was no interruption or limit to the extent of their depredations. Experience quickened their perceptions, and ultimately they became adepts in their respective departments—the one in the art of moving the ladder, and the other in the science of the nimble-fingered gentry. The division of labour thus practised by these two men accurately represents what the attainment of the erect attitude has accomplished for man by setting free his upper limbs from any further participation in the locomotion of his body.

The continued maintenance of this unique position necessitated great changes in the general structure of the body. The solution of the problem involved the turning of the ordinary quadruped a quarter of a circle in the vertical plane, thus placing the axis of the spine perpendicular, and consequently in line with the direction of the posterior limbs; and to effect this the osseous walls of the pelvis underwent certain modifications, so as to bear the additional strain put upon them. Stability was given to the trunk in its new position by the development of special groups of muscles, whose powerful and combined actions render to the movements of the human body their characteristic freedom and gracefulness. The lower limbs were placed as widely apart as possible at their juncture with the pelvis, and the thigh- and leg-bones were lengthened and strengthened so as to be capable of supporting the entire weight of the body and of transporting it with due efficiency when required. The spinal column assumed its well-known curves, and the skull, which formerly had to be supported by a powerful muscle attached to the spinous processes of the cervical vertebræ (*ligamentum nuchæ*), moved backwards until it became nearly equipoised on the top of the vertebral column. The upper limbs, instead of taking part in their original function of locomotion, were now themselves carried as flail-like appendages, in order to give them as much freedom and range of action as possible. The shoulder-blades receded to the posterior aspect of the trunk, having their axes at right-angles to that of the spine. Further, like the haunch-bones, they underwent certain modifications, so as to afford points of attachment to the muscles required in the complex movements of the arms. In the pendulous position each arm has its axis at right angles to that of the shoulder, but by a common muscular effort the two axes can be readily brought into line. The elbow-joint became capable of performing the movements of complete extension, flexion, pronation, and supination—in which respects the upper limb of man is differentiated from that of all other vertebrates.

But it is in the distal extremities of the limbs that the most remarkable anatomical changes have to be noted. The foot is virtually a tripod, the heel and the ball of the great toe being the terminal ends of an arch, while the four outer digital columns group themselves together to form the third, or steadying, point. The outer toes thus play but a subordinate part in locomotion, and, as their prehensile function is no longer of use, they may be said to be fast approaching to the condition of rudimentary organs  These three osseous prominences which form this tripod are each covered with a soft elastic pad, which both facilitates progression and acts as a buffer for deadening any possible shock which might arise in the course of running or leaping. The chief movement in the act of progression is performed by an enormously developed group of muscles known as the calf of the leg, so characteristic of man. The walker is thereby enabled to use the heel and the ball of the great toe as successive fulcrums from which the forward spring is made, the action being greatly facilitated by that of the trunk muscles in simultaneously bending the body forwards. The human foot is thus admirably adapted to be both a pillar for supporting the weight of the body, and a lever for mechanically impelling it forwards. Hence the amount of energy expended in progression is

reduced to a minimum, and when estimated proportionally to the size of the body it is believed to be considerably less than that requisite for the corresponding act in quadrupeds.

The anatomical changes effected in the extremity of the upper limb are equally radical, but of a totally different character and scope. Here we have to contemplate the transformation of the same homologous parts into an apparatus for performing a series of prehensile actions of the most intricate character, but among which neither locomotion nor support of the body forms any part whatever. This apparatus is the human hand, the most complete and perfect mechanical organ nature has yet produced. The fingers have become highly developed, and can be opposed singly or in groups to the thumb, so as to form a hook, a clasp, or a pair of pincers; and the palm can be made into a cup-shaped hollow, capable of grasping a sphere. Nor is there any limit to the direction in which many of these manipulations can be performed without any movement of the rest of the body. For example, a pencil held by the thumb and the two forefingers, as in the act of writing, can be placed in all the directions of space by a mere act of volition.

The position of such a perfect piece of mechanism, at the extremity of a movable arm attached to the upper part of the trunk, gives to man a superiority in attack and defence over all other animals, on the same principle as a soldier finds it advantageous to fight from higher ground. Moreover, he possesses the power to perform a variety of quick movements, and to assume attitudes and positions eminently adapted for the exercise of that manipulative skill with which he counteracts the superior brute force of many of his antagonists. He can readily balance his body on one or both legs, can turn on his heels as if they were pivots, and can prostrate himself comfortably in the prone or supine positions. As the centre of gravity of the whole body is nearly in line with the spinal axis, stable equilibrium is easily maintained by the lumbar muscles. Altogether we have in his physical constitution a combination of structures and functions sufficiently unique in its *tout-ensemble* to place man in a category by himself. But at the same time we must not forget that all his morphological peculiarities have been brought about without the destruction of any of the primary and typical homologies common to all the higher vertebrates.

Turning now to the brain, the undoubted organ of the mind, we find, in its intellectual and psychical manifestations, a class of phenomena which gives to man's life-functions their most remarkable character. However difficult it may be for our limited understanding to comprehend the nature of conscious sensation, we are forced to the conclusion that the act invariably takes place through the instrumentality of a few nerve-cells, whose functional activity requires to be renovated in precisely the same manner as the muscular force expended in walking. The aggregation of such cells into ganglia and nerves, by means of which reflex action, consciousness, and a variety of psychical phenomena take place, is found to permeate, in a greater or less degree, the whole of the organic world. In the higher vertebrates the seat of these manifestations is almost exclusively confined to an enormous collection of brain substance placed at the upper end of the vertebral column, and encased in a complete osseous covering called the skull. We learn from numerous experimental researches, carried out by physiologists in recent years, that the brain is a dual organ, consisting of a double series of distinct ganglia and connected to some extent by a complex system of nervous tissues, not only with each other, but with the central seat of consciousness and volition. But the difficulty of determining the nature of its functions, and the *modus operandi* of its psychological manifestations, is so great that I must pass over this part of the subject very lightly indeed. The conditions of ordinary reflex-action require that a group of muscles, by means of which a particular bodily movement is effected, shall be connected with its co-ordinating ganglion by an afferent and an efferent system of nerves. Impressions from without are conveyed by the former, or sensory nerves, to the central ganglion, from which an impulse is retransmitted by the motor nerves and sets in operation the muscular force for producing the required movement. But this efferent message is, in many cases, absolutely controlled by volition, and not only can it prevent the muscular action from taking place, but it

can effect a similar movement, *de novo*, without the direct intervention of external impressions at all. Now it has been proved experimentally that the volitional stimulus, which regulates the various movements of the body, starts from definite portions of the brain according to the different results to be produced. This localisation of brain functions, though still far from being thoroughly understood, comes very appropriately into use in this inquiry. From it we learn that the homology which characterises the structural elements of the bodies of animals extends also to the component parts of their respective brains. The law which differentiates animals according to the greater specialisation of the functions of their various organs has therefore its counterpart in the brain, and we naturally expect an increase of brain substance in every case in which the functional activity of a specific organ is extended. Thus the act of stitching with a needle and thread, an act beyond the mental and physical capacity of any animal but man, would entail a certain increase of brain substance, simply in obedience to the great complexity of the movements involved in its execution, over and above that which may be supposed to be due to the intellectual and reasoning faculties which invented it.

That man's brain and his intelligence are correlated to each other is a fact too axiomatic to require any demonstration; nor can it be doubted that the relationship between them is of the nature of cause and effect. But to maintain that the amount of the latter is directly proportional to the size of the former is rather straining the laws of legitimate inference. In drawing any general conclusion of this nature from the bulk of brain substance, there are some modifying influences which cannot be disregarded, such, for example, as the amount of cranial circulation and the quality of the brain cells. But the determination of this point is not the exact problem with which the evolutionist is primarily concerned. To him the real crux in the inquiry is to account for the evolution of man's comparatively large brain under the influence of existing cosmic forces. After duly considering this problem, and casting about for a possible explanation, I have come to the conclusion that not only is it the result of natural laws, but that one of the main factors in its production was the conversion of the upper limbs into true hands. From the first moment that man recognised the advantage of using a club or a stone in attacking his prey or defending himself from his enemies, the direct incentives to a higher brain development came into existence. He would soon learn by experience that a particular form of club or stone was more suitable for his purposes; and if the desiderated object were not to be found among the natural materials around him, he would proceed to manufacture it. Certain kinds of stones would be readily recognised as better adapted for cutting purposes than others, and he would select his materials accordingly. If these were to be found only in a special locality, he would visit that locality whenever the prized material was needed. Nor would it be an unwarrantable stretch of imagination to suppose that the circumstances would lead him to lay up a store for future use. These simple acts of intelligence assume little more than may be seen in the actions of many of the lower animals. Consciousness of his power to make and to wield a weapon was a new departure in the career of man, and every repetition of such acts became an effective and ever-accumulating training force. What a memorable event in the history of humanity was the manufacture of the first sharp stone implement! Our sapient ancestor, who first used a spear tipped with a sharp flint, became possessed of an irresistible power over his fellow men. The invention of the bow and arrow may be paralleled with the discovery of gunpowder and the use of cannon, both of which revolutionised the principles of warfare in their respective ages. The art of making fire had a greater influence on human civilisation than the modern discovery of electricity. The first boat was in all probability a log—an idea which might have been suggested by the sight of an animal clinging to a floating piece of wood carried away by a flood. To scoop this log into a hollow boat was an afterthought. The successive increments of knowledge by which a single-tree canoe has been transformed into a first-class Atlantic liner are scattered through the unwritten and written annals of many ages. In his expeditions for hunting, fishing, fruit-gathering, &c., primitive man's acquaint-

ance with the mechanical powers of nature would be gradually extended, and *pari passu* with the increasing range of his knowledge there would be a corresponding development in his reasoning faculties. Natural phenomena suggested reflections as to their causes and effects, and so by degrees they were brought into the category of law and order. Particular sounds would be used to represent specific objects, and these would become the first rudiments of language. Thus each generalisation when added to his previous little stock of knowledge widened the basis of his intellectual powers, and as the process progressed man would acquire some notion of the abstract ideas of space, time, motion, force, number, &c ; and continuous thought and reasoning would ultimately become habitual to him. All these mental operations could only take place through the medium of additional nerve cells, and hence the brain gradually became more bulky and more complex in its structure. Thus the functions of the hand and of the brain have been correlated in a most remarkable manner. Whether the mechanical skill of the hand preceded the greater intelligence of the brain, or *vice versa*, I will not pretend to say. But between the two there must have been a constant interchange of gifts. According to Sir C. Bell, ' the hand supplies all instruments, and by its correspondence with the intellect gives him universal dominion.' [1]

That mind, in its higher psychical manifestations, has sometimes been looked upon as a spiritual essence which can exist separately from its material basis need not be wondered at when we consider how the pleasing abstractions of the poet, or the fascinating creations of the novelist, roll out, as it were, from a hidden cavern without the slightest symptom of physical action. It is this marvellous power of gathering and combining ideas, previously derived through the ordinary senses, which gives a *primâ facie* appearance of having here to deal with a force exterior to the brain itself. But indeed it is questionable if such psychological phenomena are really represented by special organic equivalents. May they not be due rather to the power of volitional reflection which summons them from the materials stored up by the various localised portions into which the brain is divided ? From this point of view there may be many phases of pure cerebration which, though not the result of direct natural selection, have nevertheless as natural and physical an origin as conscious sensation. Hence imagination, conception, idealisation, the moral faculties, &c , may be compared to parasites which live at the expense of their neighbours. After all the greatest mystery of life lies in the simple acts of conscious sensation, and not in the higher mental combinations into which they enter. The highest products of intellectuality are nothing more than the transformation of previously existing energy, and it is the power to utilise it that alone finds its special organic equivalent in the brain.

But this brings us on controversial ground of the highest importance. Professor Huxley thus expresses his views on the phase of the argument now at issue :—

' I have endeavoured to show that no absolute structural line of demarcation, wider than that between the animals which immediately succeed us in the scale, can be drawn between the animal world and ourselves ; and I may add the expression of my belief that the attempt to draw a psychical distinction is equally futile, and that even the highest faculties of feeling and of intellect begin to germinate in lower forms of life.' [2]

On the other hand, Mr. Alfred R. Wallace, who holds such a distinguished position in this special field of research, has promulgated a most remarkable theory. This careful investigator, an original discoverer of the laws of natural selection, and a powerful advocate of their adequacy to bring about the evolution of the entire organic world, even including man up to a certain stage, believes that the cosmic forces are insufficient to account for the development of man in his civilised capacity. ' Natural selection,' he writes, ' could only have endowed savage man with a brain a few degrees superior to that of an ape, whereas he actually possesses one very little inferior to that of a philosopher.' This deficiency in the organic

[1] *The Hand, &c. Bridgewater Treatise*, p. 38.
[2] *Evidences as to Man's Place in Nature*, p. 109.

forces of nature he essays to supply by calling in the guiding influence of a
' superior intelligence.' In defending this hypothesis from hostile criticism he ex-
plains that by ' superior intelligence ' he means some intelligence higher than the
' modern cultivated mind,' something intermediate between it and Deity. But as
this is a pure supposition, unsupported by any evidence, and merely a matter of
personal belief, it is unnecessary to discuss it further. I would just, *en passant*, ask
Mr. Wallace why he dispenses with this ' higher intelligence ' in the early stages of
man's evolution, and finds its assistance only requisite to give the final touches to
humanity.

In dealing with the detailed objections raised by Mr. Wallace against the theory
of natural selection as applied to man, we are, however, strictly within the sphere
of legitimate argument : and evolutionists are fairly called upon to meet them. As
his own theory is founded on the supposed failure of natural selection to explain
certain specified peculiarities in the life of man, it is clear that if these difficulties
can be removed, *cadit quæstio*. It is only one of his objections, however, that comes
within the scope of my present inquiry, viz , that which is founded on the
supposed ' surplusage ' of brain power in savage and prehistoric races.

In comparing the brains of the anthropoid apes and man Mr. Wallace adopts
the following numbers to represent their proportional average capacities, viz.,
anthropoid apes 10, savages 26, and civilised man 32—numbers to which there can
be no objection, as they are based on data sufficiently accurate for the requirements
of this discussion. In commenting on the mental ability displayed in actual life
by the recipients of these various brains he states that savage man has ' in an un-
developed state faculties which he never requires to use,' and that his brain is much
beyond his actual requirements in daily life. He concludes his argument thus :—
' We see, then, that whether we compare the savage with the higher developments
of man, or with the brutes around him, we are alike driven to the conclusion that
in his large and well-developed brain he possesses an organ quite disproportionate
to his actual requirements—an organ that seems prepared in advance, only to be
fully utilised as he progresses in civilisation. A brain one half larger than that of
the gorilla would, according to the evidence before us, fully have sufficed for the
limited mental development of the savage ; and we must therefore admit that the
large brain he actually possesses could never have been solely developed by any of
those laws of evolution whose essence is that they lead to a degree of organisation
exactly proportionate to the wants of each species, never beyond those wants ;
that no preparation can be made for the future development of the race ; that one
part of the body can never increase in size or complexity, except in strict co-ordina-
tion to the pressing wants of the whole. The brain of prehistoric and of savage
man seems to me to prove the existence of some power distinct from that which
has guided the development of the lower animals through their ever-varying forms
of being ' [1]

With regard to the closing sentence of the above quotation, let me observe
that the cosmic forces, under which the lower animals have been produced by
means of natural selection, do not disclose, either in their individual or collective
capacity, any guiding power in the sense of a sentient influence, and I believe
that the ' distinct power' which the author summons to his aid, apparently from
the ' vasty deep,' to account for the higher development of humanity is nothing
more than the gradually acquired product of the reasoning faculties themselves.
Not that, for this reason, it is to be reckoned less genuine and less powerful in its
operations than if it had emanated from an outside source. The reasoning power
displayed by man is virtually a higher intelligence, and, ever since its appearance
on the field of organic life, it has, to a certain extent, superseded the laws of
natural selection. Physical science has made us acquainted with the fact that two
or three simple bodies will sometimes combine chemically so as to produce a new
substance, having properties totally different from those of either constituents in a
state of disunion. Something analogous to this has taken place in the development
of man's capacity for reasoning by induction. Its primary elements, which are

[1] *Natural Selection, &c.*, 1891, p. 193.

also those of natural selection, are conscious sensation, heredity, and a few other properties of organic matter, elements which are common, in a more or less degree, to all living things.   As soon as the sequence of natural phenomena attracted the attention of man, and his intelligence reached the stage of consecutive reasoning on the invariableness of certain effects from given causes, this new power came into existence; and its operations are, apparently, so different from those of its component elements that they can hardly be recognised as the offspring of natural forces at all.   Its application to the adjustment of his physical environments has ever since been one of the most powerful factors, not only in the development of humanity, but in altering the conditions and life-functions of many members of the animal and vegetable kingdoms.

I have already pointed out that the brain can no longer be regarded as a single organ, but rather as a series of organs connected by bonds of union—like so many departments in a Government office in telephonic communication—all, however, performing special and separate functions.   When, therefore, we attempt to compare the brain capacity of one animal with that of another, with the view of ascertaining the quality of their respective mental manifestations, we must first determine what are the exact homologous parts that are comparable.   To draw any such inference from a comparison of two brains, by simply weighing or measuring the whole mass of each, would be manifestly of no scientific value. For example, in the brain of a savage the portion representing highly skilled motor energies might be very much larger, while the portion representing logical power might be smaller, than the corresponding parts in the brain of a philosopher. But should these inequalities of development be such as to balance each other, the weight of the two organs would be equal.   In this case what could be the value of any inference as to the character of their mental endowments?   Equal-sized brains do not display equivalent, nor indeed analogous, results.   To postulate such a doctrine would be as irrational as to maintain that the walking capacities of different persons are directly proportional to the weight of their bodies.   Similar remarks are equally applicable to the skulls of prehistoric races, as it would appear that evolution had done the major part of its work in brain development long before the days of neolithic civilisation.   Huxley's well-known description of the Engis skull—'a fair average skull, which might have belonged to a philosopher, or might have contained the thoughtless brains of a savage'—goes far to settle the question from its anatomical point of view.   Until localisation of brain functions makes greater progress it is, therefore, futile to speculate to any great extent on the relative sizes of the skulls of different races either in present or prehistoric times.

But there is another aspect of the question which militates against Mr. Wallace's hypothesis, viz , the probability that many of the present tribes of savages are, in point of civilisation, in a more degenerate condition than their forefathers who acquired originally higher mental qualities under natural selection. There must surely be some foundation of truth in the widely-spread tradition of the fall of man.    And, if such be the case, we naturally expect to find some stray races with inherited brains of greater capacity than their needs, in more degenerate circumstances, may require.   An exact equivalent to this may be seen in the feeble intellectuality of many of the peasants and lower classes among the civilised nations of modern times.   Yet a youth born of such parents, if educated, often becomes a distinguished philosopher.   It is well known that if an organ ceases to perform its functional work it has a tendency to deteriorate and ultimately to disappear altogether.   But from experience we know that it takes a long time for the effects of disuse to become manifest.   It is this persistency that accounts for a number of rudimentary organs, still to be met with in the human body, whose functional activity could only have been exercised ages before man became differentiated from the lower animals.   Such facts give some support to the suggestion, previously made, that philosophy, as such, has no specially localised portion in the brain.   Its function is merely to direct the current of mental forces already existing.

But, again, Mr. Wallace's argument involves the assumption that the un-

necessarily large brain of the savage had been constructed on teleological principles for the sole purpose of philosophising. My opinion is that the greater portion of this so-called surplusage is the organic representative of the energy expended in the exercise of the enormous complexity of human actions, as displayed in the movements of his body and in the skilful manipulations necessary to the manufacture of implements, weapons, clothing, &c. All such actions have to be represented by a larger bulk of brain matter than is required for the most profound philosophical speculations. The kind of intelligence evinced by savages, however low their position in the scale of civilisation may be, is different from, and incomparably greater than, that manifested by the most advanced of the lower animals. To me it is much more rational to suppose that the development of the large brain of man corresponded, *pari passu*, with that of his characteristic physical attributes, more especially those consequent on the attainment of the upright position. That these attributes were acquired exclusively through the instrumentality of the cosmic forces was, as the following quotation will show, the opinion of Mr. Darwin:—'We must remember that nearly all the other and more important differences between man and quadrumana are manifestly adaptive in their nature, and relate chiefly to the erect position of man; such as the structure of his hand, foot, and pelvis, the curvature of his spine, and the position of his head.' [1] Mr Wallace, however, considers the feet and hands of man 'as difficulties on the theory of natural selection.' 'How,' he exclaims, 'can we conceive that early man, *as an animal*, gained anything by purely erect locomotion? Again, the hand of man contains latent capacities and powers which are unused by savages, and must have been even less used by palæolithic man and his still ruder predecessors. It has all the appearance of an organ prepared for the use of civilised man, and one which was required to render civilisation possible.' [2] But here again this acute observer diverges into his favourite by-path, and introduces a 'higher intelligence' to bridge over his difficulties.

We have now reached a stage in this inquiry when a number of questions of a more or less speculative character fall to be considered. On the supposition that, at the start, the evolution of the hand of man was synchronous with the higher development of his reasoning faculties, it is but natural to ask where, when, and in what precise circumstances this remarkable coalition took place. I would not, however, be justified in taking up your time now in discussing these questions in detail; not because I think the materials for their solution are unattainable, but because, in the present state of our knowledge, they are too conjectural to be of scientific value. In the dim retrospective vista which veils these materials from our cognisance I can only see a few faint landmarks. All the osseous remains of man which have hitherto been collected and examined point to the fact that, during the larger portion of the quaternary period, if not, indeed, from its very commencement, he had already acquired his human characteristics. This generalisation at once throws us back to the tertiary period in our search for man's early appearance in Europe. Another fact—disclosed by an analysis of his present corporeal structure—is that, during a certain phase of his previous existence, he passed through a stage when his limbs, like those of the present anthropoid apes, were adapted for an arboreal life. We have therefore to look for the causes which brought about the separation of man from his quadrumanous congeners, and entailed on him such a transformation in his form and habits, in the physical conditions that would supervene on a change from a warm to a cold climate. In the gradual lowering of the temperature of the subtropical climate which prevailed in Central Europe and the corresponding parts of Asia during the Miocene and Pliocene periods, and which culminated in the great Ice age, together with the concurrent changes in the distribution of land, seas, and mountains, we have the most probable explanation of these causes. Whether man forsook his arboreal habits and took to the plains from overcrowding of his own species in search of different kinds of food, before this cold period subjected him to its intensely adverse circumstances, it would be idle for me to offer an opinion. Equally conjectural

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[1] *Descent of Man*, p. 149          [2] *Natural Selection*, p. 198.

would it be to inquire into the exact circumstances which led him to depend exclusively on his posterior limbs for locomotion.

During this early and transitional period in man's career there was no room for ethics. Might was right, whether it emanated from the strength of the arm, the skill of the hand, or the cunning of the brain  Life and death combats would decide the fate of many competing races.  The weak would succumb to the strong, and ultimately there would survive only such as could hold their own by flight, strength, agility, or skill, just as we find among the races of man at the present day.

In summing up these somewhat discursive observations, let me just emphasise the main points of the argument   With the attainment of the erect position, and the consequent specialisation of his limbs into hands and feet, man entered on a new phase of existence.  With the advantage of manipulative organs and a progressive brain he became *Homo sapiens*, and gradually developed a capacity to understand and utilise the forces of nature.  As a handicraftsman he fashioned tools and weapons, with the skilful use of which he got the mastery over all other animals   With a knowledge of the uses of fire, the art of cooking his food, and the power of fabricating materials for clothing his body, he accommodated himself to the vicissitudes of climate, and so greatly extended his habitable area on the globe.  As ages rolled on he accumulated more and more of the secrets of nature, and every such addition widened the basis for further discoveries.  Thus commenced the grandest revolution the organic world has ever undergone—a revolution which culminated in the transformation of a brute into civilised man.  During this long transitional period mankind encountered many difficulties, perhaps the most formidable being due to the internecine struggles of inimical members of their own species.  In these circumstances the cosmic processes, formerly all-powerful so long as they acted only through the constitution of the individual, were of less potency than the acquired ingenuity and aptitude of man himself  Hence local combinations for the protection of common interests became necessary, and with the rise of social organisations the safety of the individual became merged in that of the community.  The recognition of the principle of the division of labour laid the foundations of subsequent nationalities, arts, and sciences.  Coincident with the rise of such institutions sprung up the germs of order, law, and ethics.  The progress of humanity on these novel lines was slow, but in the main steadily upwards.  No doubt the advanced centres of the various civilisations would oscillate, as they still do, from one region to another, according as some new discovery gave a preponderance of skill to one race over its opponents.  Thus the civilised world of modern times came to be fashioned, the outcome of which has been the creation of a special code of social and moral laws for the protection and guidance of humanity. Obedience to its behests is virtue, and this, to use the recent words of a profound thinker, ' involves a course of conduct which, in all respects, is opposed to that which leads to success in the cosmic struggle for existence.  In place of ruthless self-assertion it demands self-restraint; in place of thrusting aside or treading down all competitors, it requires that the individual shall not merely respect but shall help his fellows; its influence is directed, not so much to the survival of the fittest, as to the fitting of as many as possible to survive.  It repudiates the gladiatorial theory of existence.  It demands that each man who enters into the enjoyment of the advantages of a polity shall be mindful of his debt to those who have laboriously constructed it; and shall take heed that no act of his weakens the fabric in which he has been permitted to live.  Laws and moral precepts are directed to the end of curbing the cosmic process and reminding the individual of his duty to the community, to the protection and influence of which he owes, if not existence itself, at least the life of something better than a brutal savage.' [1]

These humble remarks will convey to your minds some idea of the scientific interest and profound human sympathies evoked by the far-reaching problems which fall to be discussed in this Section.  Contrasting the present state of anthropological science with its position some thirty or forty years ago, we can only marvel

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[1] Huxley, on *Evolution and Ethics*, p 33.

at the thoroughness of the change that has taken place in favour of its doctrines. Now man's immense antiquity is accepted by a vast majority of the most thoughtful men, and his place in nature, as a derivative animal at the head of the great chain of life, appeals for elucidation to all sciences and to all legitimate methods of research. But among the joyful pæans of this triumphal march we still hear some discordant notes—notes, however, which seem to me to die with their echoes, and to have as little effect on scientific progress as the whistling of an idle wind. For my own part I cannot believe that a science which seeks in the spirit of truth to trace the mysteries of human life and civilisation to their primary rootlets, a science which aims at purging our beliefs of superstitious figments generated in days when scientific methods were too feeble to expose the errors on which they were founded, a science which reminds us in a thousand ways that success in life depends on a correct knowledge of the cosmic forces around us, can be opposed to the highest and most durable interests of humanity.

# British Association for the Advancement of Science.

## IPSWICH, 1895.

# ADDRESS

BY

## SIR DOUGLAS GALTON, K.C.B., D.C.L., F.R.S.,

### PRESIDENT.

My first duty is to convey to you, Mr. Mayor, and to the inhabitants of Ipswich, the thanks of the British Association for your hospitable invitation to hold our sixty-fifth meeting in your ancient town, and thus to recall the agreeable memories of the similar favour which your predecessors conferred on the Association forty-four years ago.

In the next place I feel it my duty to say a few words on the great loss which science has recently sustained—the death of the Right Hon. Thomas Henry Huxley. It is unnecessary for me to enlarge, in the presence of so many to whom his personality was known, upon his charm in social and domestic life; but upon the debt which the Association owes to him for the assistance which he rendered in the promotion of science I cannot well be silent. Huxley was preeminently qualified to assist in sweeping away the obstruction by dogmatic authority, which in the early days of the Association fettered progress in certain branches of science. For, whilst he was an eminent leader in biological research, his intellectual power, his original and intrepid mind, his vigorous and masculine English, made him a writer who explained the deepest subject with transparent clearness. And as a speaker his lucid and forcible style was adorned with ample and effective illustration in the lecture-room; and his energy and wealth of argument in a more public arena largely helped to win the battle of evolution, and to secure for us the right to discuss questions of religion and science without fear and without favour.

It may, I think, interest you to learn that Huxley first made the

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acquaintance of Tyndall at the meeting of the Association held in this town in 1851.

About forty-six years ago I first began to attend the meetings of the British Association , and I was elected one of your general secretaries about twenty-five years ago.

It is not unfitting, therefore, that I should recall to your minds the conditions under which science was pursued at the formation of the Association, as well as the very remarkable position which the Association has occupied in relation to science in this country.

Between the end of the sixteenth century and the early part of the present century several societies had been created to develop various branches of science. Some of these societies were established in London, and others in important provincial centres.

In 1831, in the absence of railways, communication between different parts of the country was slow and difficult. Science was therefore localised ; and in addition to the universities in England, Scotland, and Ireland, the towns of Birmingham, Manchester, Plymouth and York each maintained an important nucleus of scientific research.

### ORIGIN OF THE BRITISH ASSOCIATION

Under these social conditions the British Association was founded in September 1831.

The general idea of its formation was derived from a migratory society which had been previously formed in Germany ; but whilst the German society met for the special occasion on which it was summoned, and then dissolved, the basis of the British Association was continuity.

The objects of the founders of the British Association were enunciated in their earliest rules to be :—

' To give a stronger impulse and a more systematic direction to scientific inquiry ; to promote the intercourse of those who cultivated science in different parts of the British Empire with one another, and with foreign philosophers ; to obtain a more general attention to the objects of science, and a removal of any disadvantages of a public kind which impede its progress.'

Thus the British Association for the Advancement of Science based its utility upon the opportunity it afforded for combination.

The first meeting of the Association was held at York with 353 members.

As an evidence of the want which the Association supplied, it may be mentioned that at the second meeting, which was held at Oxford, the number of members was 435. The third meeting, at Cambridge, numbered over 900 members, and at the meeting at Edinburgh in 1834 there were present 1,298 members.

At its third meeting, which was held at Cambridge in 1833, the Association, through the influence it had already acquired, induced the

Government to grant a sum of 500*l.* for the reduction of the astronomical observations of Baily. And at the same meeting the General Committee commenced to appropriate to scientific research the surplus from the subscriptions of its members. The committees on each branch of science were desired 'to select definite and important objects of science, which they may think most fit to be advanced by an application of the funds of the society, either in compensation for labour, or in defraying the expense of apparatus, or otherwise, stating their reasons for their selection, and, when they may think proper, designating individuals to undertake the desired investigations.'

The several proposals were submitted to the Committee of Recommendations, whose approval was necessary before they could be passed by the General Committee. The regulations then laid down still guide the Association in the distribution of its grants. At that early meeting the Association was enabled to apply 600*l.* to these objects.

I have always wondered at the foresight of the framers of the constitution of the British Association, the most remarkable feature of which is the lightness of the tie which holds it together. It is not bound by any complex central organisation. It consists of a federation of Sections, whose youth and energy are yearly renewed by a succession of presidents and vice-presidents, whilst in each Section some continuity of action is secured by the less movable secretaries.

The governing body is the General Committee, the members of which are selected for their scientific work ; but their controlling power is tempered by the law that all changes of rules, or of constitution, should be submitted to, and receive the approval of, the Committee of Recommendations. This committee may be described as an ideal Second Chamber. It consists of the most experienced members of the Association.

The administration of the Association in the interval between annual meetings is carried on by the Council, an executive body, whose duty it is to complete the work of the annual meeting (*a*) by the publication of its proceedings ; (*b*) by giving effect to resolutions passed by the General Committee ; (*c*) it also appoints the Local Committee and organises the *personnel* of each Section for the next meeting.

I believe that one of the secrets of the long-continued success and vitality of the British Association lies in this purely democratic constitution, combined with the compulsory careful consideration which must be given to suggested organic changes.

The Association is now in the sixty-fifth year of its existence. In its origin it invited the philosophical societies dispersed throughout Great Britain to unite in a co-operative union.

Within recent years it has endeavoured to consolidate that union.

At the present time almost all important local scientific societies scattered throughout the country, some sixty-six in number, are in correspondence with the Association. Their delegates hold annual conferences at our meetings. The Association has thus extended the sphere of its action :

it places the members of the local societies engaged in scientific work in relation with each other, and brings them into co-operation with members of the Association and with others engaged in original investigations, and the papers which the individual societies publish annually are catalogued in our Report. Thus by degrees a national catalogue will be formed of the scientific work of these societies.

The Association has, moreover, shown that its scope is coterminous with the British Empire by holding one of its annual meetings at Montreal, and we are likely soon to hold a meeting in Toronto.

### CONDITION OF CERTAIN SCIENCES AT THE FORMATION OF THE BRITISH ASSOCIATION.

The Association, at its first meeting, began its work by initiating a series of reports upon the then condition of the several sciences.

A rapid glance at some of these reports will not only show the enormous strides which have been made since 1831 in the investigation of facts to elucidate the laws of Nature, but it may afford a slight insight into the impediments offered to the progress of investigation by the mental condition of the community, which had been for so long satisfied to accept assumptions without undergoing the labour of testing their truth by ascertaining the real facts  This habit of mind may be illustrated by two instances selected from the early reports made to the Association. The first is afforded by the report made in 1832, by Mr. Lubbock, on ' Tides '

This was a subject necessarily of importance to England as a dominant power at sea. But in England records of the tides had only recently been commenced at the dockyards of Woolwich, Sheerness, Portsmouth, and Plymouth, on the request of the Royal Society, and no information had been collected upon the tides on the coasts of Scotland and Ireland.

The British Association may feel pride in the fact that within three years of its inception, viz. by 1834, it had induced the Corporation of Liverpool to establish two tide gauges, and the Government to undertake tidal observations at 500 stations on the coasts of Britain.

Another cognate instance is exemplified by a paper read at the second meeting, in 1832, upon the State of Naval Architecture in Great Britain. The author contrasts the extreme perfection of the carpentry of the internal fittings of the vessels with the remarkable deficiency of mathematical theory in the adjustment of the external form of vessels, and suggests the benefit of the application of refined analysis to the various practical problems which ought to interest shipbuilders—problems of capacity, of displacement, of stowage, of velocity, of pitching and rolling, of masting, of the effects of sails and of the resistance of fluids ; and, moreover, suggests that large-scale experiments should be made by Government, to afford the necessary data for calculation.

Indeed, when we consider how completely the whole habit of mind of the populations of the Western world has been changed, since the beginning

of the century, from willing acceptance of authority as a rule of life to a universal spirit of inquiry and experimental investigation, is it not probable that this rapid change has arisen from society having been stirred to its foundations by the causes and consequences of the French Revolution ?

One of the earliest practical results of this awakening in France was the conviction that the basis of scientific research lay in the accuracy of the standards by which observations could be compared ; and the following principles were laid down as a basis for their measurements of length, weight, and capacity viz. (1) that the unit of linear measure applied to matter in its three forms of extension, viz., length, breadth, and thickness, should be the standard of measures of length, surface, and solidity ; (2) that the cubic contents of the linear measure in decimetres of pure water at the temperature of its greatest density should furnish at once the standard weight and the measure of capacity.[1] The metric system did not come into full operation in France till 1840 ; and it is now adopted by all countries on the continent of Europe except Russia.

The standards of length which were accessible in Great Britain at the formation of the Association were the Parliamentary standard yard lodged in the Houses of Parliament (which was destroyed in 1834 in the fire which burned the Houses of Parliament) ; the Royal Astronomical Society's standard ; and the 10-foot bar of the Ordnance Survey.

The first two were assumed to afford exact measurements at a given temperature. The Ordnance bar was formed of two bars on the principle of a compensating pendulum, and afforded measurements independent of temperature. Standard bars were also disseminated throughout the country, in possession of the corporations of various towns.

The British Association early recognised the importance of uniformity in the record of scientific facts, as well as the necessity for an easy method of comparing standards and for verifying differences between instruments and apparatus required by various observers pursuing similar lines of investigation. At its meeting at Edinburgh in 1834 it caused a comparison to be made between the standard bar at Aberdeen, constructed by Troughton, and the standard of the Royal Astronomical Society, and reported that the scale 'was exceedingly well finished ; it was about $\frac{1}{500}$th of an inch shorter than the 5-feet of the Royal Astronomical Society's scale, but it was evident that a great number of minute, yet important, circumstances have hitherto been neglected in the formation of such scales, without an attention to which they cannot be expected to accord with that degree of accuracy which the present state of science demands.' Subsequently, at the meeting at Newcastle in 1863, the Association appointed a committee to report on the best means of providing for a uniformity of weights and measures with reference to the

[1] The litre is the volume of a kilogramme of pure water at its maximum density, and is slightly less than the litre was intended to be, viz , one cubic decimetre. The weight of a cubic decimetre of pure water is 1 000013 kilogrammes.

interests of science. This committee recommended the metric decimal system—a recommendation which has been endorsed by a committee of the House of Commons in the last session of Parliament.

British instrument-makers had been long conspicuous for accuracy of workmanship. Indeed, in the eighteenth century practical astronomy had been mainly in the hands of British observers ; for although the mathematicians of France and other countries on the continent of Europe were occupying the foremost place in mathematical investigation, means of astronomical observation had been furnished almost exclusively by English artisans.

The sectors, quadrants, and circles of Ramsden, Bird, and Cary were inimitable by Continental workmen.

But the accuracy of the mathematical-instrument maker had not penetrated into the engineer's workshop. And the foundation of the British Association was coincident with a rapid development of mechanical appliances.

At that time a good workman had done well if the shaft he was turning, or the cylinder he was boring, 'was right to the $\frac{1}{32}$nd of an inch.' This was, in fact, a degree of accuracy as fine as the eye could usually distinguish.

Few mechanics had any distinct knowledge of the method to be pursued for obtaining accuracy ; nor, indeed, had practical men sufficiently appreciated either the immense importance or the comparative facility of its acquisition.

The accuracy of workmanship essential to this development of mechanical progress required very precise measurements of length, to which reference could be easily made. No such standards were then available for the workshops. But a little before 1830 a young workman named Joseph Whitworth realised that the basis of accuracy in machinery was the making of a true plane. The idea occurred to him that this could only be secured by making three independent plane surfaces ; if each of these would lift the other, they must be planes, and they must be true.

The true plane rendered possible a degree of accuracy beyond the wildest dreams of his contemporaries in the construction of the lathe and the planing machine, which are used in the manufacture of all tools.

His next step was to introduce an exact system of measurement, generally applicable in the workshop.

Whitworth felt that the eye was altogether inadequate to secure this, and appealed to the sense of touch for affording a means of comparison. If two plugs be made to fit into a round hole, they may differ in size by a quantity imperceptible to the eye, or to any ordinary process of measurement, but in fitting them into the hole the difference between the larger and the smaller is felt immediately by the greater ease with which the smaller one fits. In this way a child can tell which is the larger of two cylinders differing in thickness by no more than $\frac{1}{5000}$th of an inch.

Standard gauges, consisting of hollow cylinders with plugs to fit, but

differing in diameter by the $\frac{1}{10,000}$th or the $\frac{1}{100,000}$th of an inch, were given to his workmen, with the result that a degree of accuracy inconceivable to the ordinary mind became the rule of the shop.

To render the construction of accurate gauges possible Whitworth devised his measuring machine, in which the movement was effected by a screw; by this means the distance between two true planes might be measured to the one-millionth of an inch.

These advances in precision of measurement have enabled the degree of accuracy which was formerly limited to the mathematical-instrument maker to become the common property of every machine shop. And not only is the latest form of steam-engine, in the accuracy of its workmanship, little behind the chronometer of the early part of the century, but the accuracy in the construction of experimental apparatus which has thus been introduced has rendered possible recent advances in many lines of research.

Lord Kelvin said in his Presidential Address at Edinburgh, ' Nearly all the grandest discoveries of science have been but the rewards of accurate measurement and patient, long-continued labour in the sifting of numerical results.' The discovery of argon, for which Lord Rayleigh and Professor Ramsay have been awarded the Hodgkin prize by the Smithsonian Institution affords a remarkable illustration of the truth of this remark. Indeed, the provision of accurate standards not only of length, but of weight, capacity, temperature, force, and energy, are amongst the foundations of scientific investigation.

In 1842 the British Association obtained the opportunity of extending its usefulness in this direction.

In that year the Government gave up the Royal Observatory at Kew, and offered it to the Royal Society, who declined it. But the British Association accepted the charge. Their first object was to continue Sabine's valuable observations upon the vibrations of a pendulum in various gases, and to promote pendulum observations in various parts of the world. They subsequently extended it into an observatory for comparing and verifying the various instruments which recent discoveries in physical science had suggested for continuous meteorological and magnetic observations, for observations and experiments on atmospheric electricity, and for the study of solar physics.

This new departure afforded a means for ascertaining the advantages and disadvantages of the several varieties of scientific instruments; as well as for standardising and testing instruments, not only for instrument-makers, but especially for observers by whom simultaneous observations were then being carried on in different parts of the world; and also for training observers proceeding abroad on scientific expeditions.

Its special object was to promote original research, and expenditure was not to be incurred on apparatus merely intended to exhibit the necessary consequences of known laws.

The rapid strides in electrical science had attracted attention to the

measurement of electrical resistances, and in 1859 the British Association appointed a special committee to devise a standard. The standard of resistance proposed by that committee became the generally accepted standard, until the requirements of that advancing science led to the adoption of an international standard.

In 1866 the Meteorological Department of the Board of Trade entered into close relations with the Kew Observatory.

And in 1871 Mr. Gassiot transferred 10,000*l.* upon trust to the Royal Society for the maintenance of the Kew Observatory, for the purpose of assisting in carrying on magnetical, meteorological, and other physical observations. The British Association thereupon, after having maintained this Observatory for nearly thirty years, at a total expenditure of about 12,000*l.*, handed the Observatory over to the Royal Society.

The 'Transactions' of the British Association are a catalogue of its efforts in every branch of science, both to promote experimental research and to facilitate the application of the results to the practical uses of life.

But probably the marvellous development in science which has accompanied the life-history of the Association will be best appreciated by a brief allusion to the condition of some of the branches of science in 1831 as compared with their present state.

### Geological and Geographical Science.

#### *Geology.*

At the foundation of the Association geology was assuming a prominent position in science. The main features of English geology had been illustrated as far back as 1821, and, among the founders of the British Association, Murchison and Phillips, Buckland, Sedgwick and Conybeare, Lyell and De la Beche, were occupied in investigating the data necessary for perfecting a geological chronology by the detailed observations of the various British deposits, and by their co-relation with the Continental strata. They were thus preparing the way for those large generalisations which have raised geology to the rank of an inductive science.

In 1831 the Ordnance maps published for the southern counties had enabled the Government to recognise the importance of a geological survey by the appointment of Mr. De la Bèche to affix geological colours to the maps of Devonshire and portions of Somerset, Dorset and Cornwall; and in 1835 Lyell, Buckland and Sedgwick induced the Government to establish the Geological Survey Department, not only for promoting geological science, but on account of its practical bearing on agriculture, mining, the making of roads, railways and canals, and on other branches of national industry.

*Geography.*

The Ordnance Survey appears to have had its origin in a proposal of the French Government to make a joint-measurement of an arc of the meridian. This proposal fell through at the outbreak of the Revolution; but the measurement of the base for that object was taken as a foundation for a national survey. In 1831, however, the Ordnance Survey had only published the 1-inch map for the southern portion of England, and the great triangulation of the kingdom was still incomplete.

In 1834 the British Association urged upon the Government that the advancement of various branches of science was greatly retarded by the want of an accurate map of the whole of the British Isles; and that, consequently, the engineer and the meteorologist, the agriculturist and the geologist, were each fettered in their scientific investigations by the absence of those accurate data which now lie ready to his hand for the measurement of length, of surface, and of altitude.

Yet the first decade of the British Association was coincident with a considerable development of geographical research. The Association was persistent in pressing on the Government the scientific importance of sending the expedition of Ross to the Antarctic and of Franklin to the Arctic regions. We may trust that we are approaching a solution of the geography of the North Pole , but the Antarctic regions still present a field for the researches of the meteorologist, the geologist, the biologist, and the magnetic observer, which the recent voyage of M. Borchgrevink leads us to hope may not long remain unexplored.

In the same decade the question of an alternative route to India by means of a communication between the Mediterranean and the Persian Gulf was also receiving attention, and in 1835 the Government employed Colonel Chesney to make a survey of the Euphrates valley in order to ascertain whether that river would enable a practicable route to be formed from Iskanderoon, or Tripoli, opposite Cyprus, to the Persian Gulf. His valuable surveys are not, however, on a sufficiently extensive scale to enable an opinion to be formed as to whether a navigable waterway through Asia Minor is physically practicable, or whether the cost of establishing it might not be prohibitive.

The advances of Russia in Central Asia have made it imperative to provide an easy, rapid, and alternative line of communication with our Eastern possessions, so as not to be dependent upon the Suez Canal in time of war. If a navigation cannot be established, a railway between the Mediterranean and the Persian Gulf has been shown by the recent investigations of Messrs. Hawkshaw and Hayter, following on those of others, to be perfectly practicable and easy of accomplishment ; such an undertaking would not only be of strategical value, but it is believed it would be commercially remunerative.

Speke and Grant brought before the Association, at its meeting at Newcastle in 1863, their solution of the mystery of the Nile basin, which

had puzzled geographers from the days of Herodotus ; and the efforts of Livingstone and Stanley and others have opened out to us the interior of Africa. I cannot refrain here from expressing the deep regret which geologists and geographers, and indeed all who are interested in the progress of discovery, feel at the recent death of Joseph Thomson. His extensive, accurate, and trustworthy observations added much to our knowledge of Africa, and by his premature death we have lost one of its most competent explorers.

## CHEMICAL, ASTRONOMICAL AND PHYSICAL SCIENCE.

### *Chemistry.*

The report made to the Association on the state of the chemical sciences in 1832, says that the efforts of investigators were then being directed to determining with accuracy the true nature of the substances which compose the various products of the organic and inorganic kingdoms, and the exact ratios by weight which the different constituents of these substances bear to each other.

But since that day the science of chemistry has far extended its boundaries. The barrier has vanished which was supposed to separate the products of living organisms from the substances of which minerals consist, or which could be formed in the laboratory. The number of distinct carbon compounds obtainable from organisms has greatly increased ; but it is small when compared with the number of such compounds which have been artificially formed. The methods of analysis have been perfected. The physical, and especially the optical, properties of the various forms of matter have been closely studied, and many fruitful generalisations have been made. The form in which these generalisations would now be stated may probably change, some, perhaps, by the overthrow or disuse of an ingenious guess at Nature's workings, but more by that change which is the ordinary growth of science—namely, inclusion in some simpler and more general view.

In these advances the chemist has called the spectroscope to his aid. Indeed, the existence of the British Association has been practically coterminous with the comparatively newly developed science of spectrum analysis, for though Newton,[1] Wollaston, Fraunhofer, and Fox Talbot had worked at the subject long ago, it was not till Kirchhoff and Bunsen set a seal on the prior labours of Stokes, Ångstrom, and Balfour Stewart that the spectra of terrestrial elements have been mapped out and grouped ; that by its help new elements have been discovered, and that

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[1] Joannes Marcus Marci, of Kronland in Bohemia, was the only predecessor of Newton who had any knowledge of the formation of a spectrum by a prism He not only observed that the coloured rays diverged as they left the prism, but that a coloured ray did not change in colour after transmission through a prism His book, *Thaumantias, liber de arcu cœlesti deque colorum apparentium natura*, Prag, 1648, was, however, not known to Newton, and had no influence upon future discoveries.

the idea has been suggested that the various orders of spectra of the same element are due to the existence of the element in different molecular forms—allotropic or otherwise—at different temperatures.

But great as have been the advances of terrestrial chemistry through its assistance, the most stupendous advance which we owe to the spectroscope lies in the celestial direction.

### Astronomy.

In the earlier part of this century, whilst the sidereal universe was accessible to investigators, many problems outside the solar system seemed to be unapproachable.

At the third meeting of the Association, at Cambridge, in 1833, Dr. Whewell said that astronomy is not only the queen of science, but the only perfect science, which was 'in so elevated a state of flourishing maturity that all that remained was to determine with the extreme of accuracy the consequences of its rules by the profoundest combinations of mathematics ; the magnitude of its data by the minutest scrupulousness of observation.'

But in the previous year, viz. 1832, Airy, in his report to the Association on the progress of astronomy, had pointed out that the observations of the planet Uranus could not be united in one elliptic orbit ; a remark which turned the attention of Adams to the discovery of Neptune. In his report on the position of optical science in 1832, Brewster suggested that with the assistance of adequate instruments 'it would be possible to study the action of the elements of material bodies upon rays of artificial light, and thereby to discover the analogies between their affinities and those which produce the fixed lines in the spectra of the stars ; and thus to study the effects of the combustions which light up the suns of other systems.'

This idea has now been realised. All the stars which shine brightly enough to impress an image of the spectrum upon a photographic plate have been classified on a chemical basis. The close connection between stars and nebulæ has been demonstrated ; and while on the one hand the modern science of thermodynamics has shown that the hypothesis of Kant and Laplace on stellar formation is no longer tenable, inquiry has indicated that the true explanation of stellar evolution is to be found in the gradual condensation of meteoritic particles, thus justifying the suggestions put forward long ago by Lord Kelvin and Professor Tait.

We now know that the spectra of many of the terrestrial elements in the chromosphere of the sun differ from those familiar to us in our laboratories. We begin to glean the fact that the chromospheric spectra are similar to those indicated by the absorption going on in the hottest stars, and Lockyer has not hesitated to affirm that these facts would indicate that in those localties we are in the presence of the actions of temperatures sufficiently high to break up our chemical elements into finer forms. Other

students of these phenomena may not agree in this view, and possibly the discrepancies may be due to default in our terrestrial chemistry. Still, I would recall to you that Dr. Carpenter, in his Presidential Address at Brighton in 1872, almost censured the speculations of Frankland and Lockyer in 1868 for attributing a certain bright line in the spectrum of solar prominences (which was not identifiable with that of any known terrestrial source of light) to a hypothetical new substance which they proposed to call ' helium,' because ' it had not received that verification which, in the case of Crookes' search for thallium, was afforded by the actual discovery of the new metal.' Ramsay has now shown that this gas is present in dense minerals on earth ; but we have now also learned from Lockyer that it and other associated gases are not only found with hydrogen in the solar chromosphere, but that these gases, with hydrogen, form a large percentage of the atmospheric constituents of some of the hottest stars in the heavens.

The spectroscope has also made us acquainted with the motions and even the velocities of those distant orbs which make up the sidereal universe. It has enabled us to determine that many stars, single to the eye, are really double, and many of the conditions of these strange systems have been revealed. The rate at which matter is moving in solar cyclones and winds is now familiar to us. And I may also add that quite recently his wonderful instrument has enabled Professor Keeler to verify Clerk-Maxwell's theory that the rings of Saturn consist of a marvellous company of separate moons—as it were, a cohort of courtiers revolving round their queen—with velocities proportioned to their distances from the planet.

## *Physics*

If we turn to the sciences which are included under physics, the progress has been equally marked.

In optical science, in 1831 the theory of emission as contrasted with the undulatory theory of light was still under discussion.

Young, who was the first to explain the phenomena due to the interference of the rays of light as a consequence of the theory of waves, and Fresnel, who showed the intensity of light for any relative position of the interference-waves, both had only recently passed away.

The investigations into the laws which regulate the conduction and radiation of heat, together with the doctrine of latent and of specific heat, and the relations of vapour to air, had all tended to the conception of a material heat, or caloric, communicated by an actual flow and emission.

It was not till 1834 that improved thermometrical appliances had enabled Forbes and Melloni to establish the polarisation of heat, and thus to lay the foundation of an undulatory theory for heat similar to that which was in progress of acceptation for light.

Whewell's report, in 1832, on magnetism and electricity shows that

these branches of science were looked upon as cognate, and that the theory of two opposite electric fluids was generally accepted.

In magnetism, the investigations of Hansteen, Gauss, and Weber in Europe, and the observations made under the Imperial Academy of Russia over the vast extent of that empire, had established the existence of magnetic poles, and had shown that magnetic disturbances were simultaneous at all the stations of observation.

At their third meeting the Association urged the Government to establish magnetic and meteorological observatories in Great Britain and her colonies and dependencies in different parts of the earth, furnished with proper instruments, constructed on uniform principles, and with provisions for continued observations at those places.

In 1839 the British Association had a large share in inducing the Government to initiate the valuable series of experiments for determining the intensity, the declination, the dip, and the periodical variations of the magnetic needle which were carried on for several years, at numerous selected stations over the surface of the globe, under the directions of Sabine and Lefroy.

In England systematic and regular observations are still made at Greenwich, Kew, and Stonyhurst. For some years past similar observations by both absolute and self-recording instruments have also been made at Falmouth—close to the home of Robert Were Fox, whose name is inseparably connected with the early history of terrestrial magnetism in this country—but under such great financial difficulties that the continuance of the work is seriously jeopardised. It is to be hoped that means may be forthcoming to carry it on. Cornishmen, indeed, could found no more fitting memorial of their distinguished countryman, John Couch Adams, than by suitably endowing the magnetic observatory in which he took so lively an interest.

Far more extended observation will be needed before we can hope to have an established theory as to the magnetism of the earth. We are without magnetic observations over a large part of the Southern Hemisphere. And Professor Rucker's recent investigations tell us that the earth seems as it were alive with magnetic forces, be they due to electric currents or to variations in the state of magnetised matter ; that the disturbances affect not only the diurnal movement of the magnet, but that even the small part of the secular change which has been observed, and which has taken centuries to accomplish, is interfered with by some slower agency. And, what is more important, he tells us that none of these observations stand as yet upon a firm basis, because standard instruments have not been in accord ; and much labour, beyond the power of individual effort, has hitherto been required to ascertain whether the relations between them are constant or variable.

In electricity, in 1831, just at the time when the British Association was founded, Faraday's splendid researches in electricity and magnetism at the Royal Institution had begun with his discovery of magneto-

electric induction, his investigation of the laws of electro-chemical decomposition, and of the mode of electrolytical action.

But, the practical application of our electrical knowledge was then limited to the use of lightning-conductors for buildings and ships. Indeed, it may be said that the applications of electricity to the use of man have grown up side by side with the British Association.

One of the first practical applications of Faraday's discoveries was in the deposition of metals and electro-plating, which has developed into a large branch of national industry; and the dissociating effect of the electric arc, for the reduction of ores, and in other processes, is daily obtaining a wider extension.

But probably the application of electricity which is tending to produce the greatest change in our mental, and even material condition, is the electric telegraph and its sister, the telephone. By their agency not only do we learn, almost at the time of their occurrence, the events which are happening in distant parts of the world, but they are establishing a community of thought and feeling between all the nations of the world which is influencing their attitude towards each other, and, we may hope, may tend to weld them more and more into one family.

The electric telegraph was introduced experimentally in Germany in 1833, two years after the formation of the Association. It was made a commercial success by Cooke and Wheatstone in England, whose first attempts at telegraphy were made on the line from Euston to Camden Town in 1837, and on the line from Paddington to West Drayton in 1838.

The submarine telegraph to America, conceived in 1856, became a practical reality in 1861 through the commercial energy of Cyrus Field and Pender, aided by the mechanical skill of Latimer Clark, Gooch, and others, and the scientific genius of Lord Kelvin. The knowledge of electricity gained by means of its application to the telegraph largely assisted the extension of its utility in other directions.

The electric light gives, in its incandescent form, a very perfect hygienic light. Where rivers are at hand the electrical transmission of power will drive railway trains and factories economically, and might enable each artisan to convert his room into a workshop, and thus assist in restoring to the labouring man some of the individuality which the factory has tended to destroy.

In 1843 Joule described his experiments for determining the mechanical equivalent of heat. But it was not until the meeting at Oxford, in 1847, that he fully developed the law of the conservation of energy, which, in conjunction with Newton's law of the conservation of momentum, and Dalton's law of the conservation of chemical elements, constitutes a complete mechanical foundation for physical science.

Who, at the foundation of the Association, would have believed some far-seeing philosopher if he had foretold that the spectroscope would analyse the constituents of the sun and measure the motions of the stars; that we should liquefy air and utilise temperatures approaching to the

absolute zero for experimental research ; that, like the magician in the 'Arabian Nights,' we should annihilate distance by means of the electric telegraph and the telephone , that we should illuminate our largest buildings instantaneously, with the clearness of day, by means of the electric current . that by the electric transmission of power we should be able to utilise the Falls of Niagara to work factories at distant places ; that we should extract metals from the crust of the earth by the same electrical agency to which, in some cases, their deposition has been attributed ?

These discoveries and their applications have been brought to their present condition by the researches of a long line of scientific explorers, such as Dalton, Joule, Maxwell, Helmholtz, Herz, Kelvin, and Rayleigh, aided by vast strides made in mechanical skill. But what will our successors be discussing sixty years hence ' How little do we yet know of the vibrations which communicate light and heat ! Far as we have advanced in the application of electricity to the uses of life, we know but little even yet of its real nature. We are only on the threshold of the knowledge of molecular action, or of the constitution of the all-pervading æther Newton, at the end of the seventeenth century, in his preface to the 'Principia,' says : 'I have deduced the motions of the planets by mathematical reasoning from forces ; and I would that we could derive the other phenomena of Nature from mechanical principles by the same mode of reasoning. For many things move me, so that I somewhat suspect that all such may depend on certain forces by which the particles of bodies, through causes not yet known, are either urged towards each other according to regular figures, or are repelled and recede from each other ; and these forces being unknown, philosophers have hitherto made their attempts on Nature in vain.'

In 1848 Faraday remarked : 'How rapidly the knowledge of molecular forces grows upon us, and how strikingly every investigation tends to develop more and more their importance !

'A few years ago magnetism was an occult force, affecting only a few bodies ; now it is found to influence all bodies, and to possess the most intimate relation with electricity, heat, chemical action, light, crystallisation ; and through it the forces concerned in cohesion. We may feel encouraged to continuous labours, hoping to bring it into a bond of union with gravity itself.'

But it is only within the last few years that we have begun to realise that electricity is closely connected with the vibrations which cause heat and light, and which seem to pervade all space—vibrations which may be termed the voice of the Creator calling to each atom and to each cell of protoplasm to fall into its ordained position, each, as it were, a musical note in the harmonious symphony which we call the universe.

*Meteorology.*

At the first meeting, in 1831, Professor James D. Forbes was requested to draw up a report on the State of Meteorological Science, on the ground that this science is more in want than any other of that systematic direction which it is one great object of the Association to give.

Professor Forbes made his first report in 1832, and a subsequent report in 1840. The systematic records now kept in various parts of the world of barometric pressure, of solar heat, of the temperature and physical conditions of the atmosphere at various altitudes, of the heat of the ground at various depths, of the rainfall, of the prevalence of winds, and the gradual elucidation not only of the laws which regulate the movements of cyclones and storms, but of the influences which are exercised by the sun and by electricity and magnetism, not only upon atmospheric conditions, but upon health and vitality, are gradually approximating meteorology to the position of an exact science.

England took the lead in rainfall observations. Mr. G. J. Symons organised the British Rainfall System in 1860 with 178 observers, a system which until 1876 received the help of the British Association. Now Mr. Symons himself conducts it, assisted by more than 3,000 observers, and these volunteers not only make the observations, but defray the expense of their reduction and publication. In foreign countries this work is done by Government officers at the public cost.

At the present time a very large number of rain gauges are in daily use throughout the world. The British Islands have more than 3,000, and India and the United States have nearly as many ; France and Germany are not far behind ; Australia probably has more—indeed, one colony alone, New South Wales, has more than 1,100.

The storm warnings now issued under the excellent systematic organisation of the Meteorological Committee may be said to have had their origin in the terrible storm which broke over the Black Sea during the Crimean War, on November 27, 1855. Leverrier traced the progress of that storm, and seeing how its path could have been reported in advance by the electric telegraph, he proposed to establish observing stations which should report to the coasts the probability of the occurrence of a storm. Leverrier communicated with Airy, and the Government authorised Admiral FitzRoy to make tentative arrangements in this country. The idea was also adopted on the Continent, and now there are few civilised countries north or south of the equator without a system of storm warning.[1]

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[1] It has often been supposed that Leverrier was also the first to issue a daily weather map, but that was not the case, for in the Great Exhibition of 1851 the Electric Telegraph Company sold daily weather maps, copies of which are still in existence, and the data for them were, it is believed, obtained by Mr. James Glaisher, F R S., at that time Superintendent of the Meteorological Department at Greenwich.

## BIOLOGICAL SCIENCE.

### *Botany.*

The earliest Reports of the Association which bear on the biological sciences were those relating to botany.

In 1831 the controversy was yet unsettled between the advantages of the Linnean, or Artificial system, as contrasted with the Natural system of classification. Histology, morphology, and physiological botany, even if born, were in their early infancy.

Our records show that von Mohl noted cell division in 1835, the presence of chlorophyll corpuscles in 1837; and he first described protoplasm in 1846.

Vast as have been the advances of physiological botany since that time, much of its fundamental principles remain to be worked out, and I trust that the establishment, for the first time, of a permanent Section for botany at the present meeting will lead the Association to take a more prominent part than it has hitherto done in the further development of this branch of biological science.

### *Animal Physiology.*

In 1831 Cuvier, who during the previous generation had, by the collation of facts followed by careful inductive reasoning, established the plan on which each animal is constructed, was approaching the termination of his long and useful life. He died in 1832; but in 1831 Richard Owen was just commencing his anatomical investigations and his brilliant contributions to palæontology.

The impulse which their labours gave to biological science was reflected in numerous reports and communications, by Owen and others, throughout the early decades of the British Association, until Darwin propounded a theory of evolution which commanded the general assent of the scientific world. For this theory was not absolutely new. But just as Cuvier had shown that each bone in the fabric of an animal affords a clue to the shape and structure of the animal, so Darwin brought harmony into scattered facts, and led us to perceive that the moulding hand of the Creator may have evolved the complicated structures of the organic world from one or more primeval cells.

Richard Owen did not accept Darwin's theory of evolution, and a large section of the public contested it. I well remember the storm it produced—a storm of praise by my geological colleagues, who accepted the result of investigated facts; a storm of indignation such as that which would have burned Galileo at the stake from those who were not yet prepared to question the old authorities; but they diminish daily.

We are, however, as yet only on the threshold of the doctrine of evolution. Does not each fresh investigation, even into the embryonic stage of the simpler forms of life, suggest fresh problems?

*Anthropology.*

The impulse given by Darwin has been fruitful in leading others to consider whether the same principle of evolution may not have governed the moral as well as the material progress of the human race. Mr. Kidd tells us that nature as interpreted by the struggle for life contains no sanction for the moral progress of the individual, and points out that if each of us were allowed by the conditions of life to follow his own inclination the average of each generation would distinctly deteriorate from that of the preceding one ; but because the law of life is ceaseless and inevitable struggle and competition, ceaseless and inevitable selection and rejection, the result is necessarily ceaseless and inevitable progress. Evolution, as Sir William Flower said, is the message which biology has sent to help us on with some of the problems of human life, and Francis Galton urges that man, the foremost outcome of the awful mystery of evolution, should realise that he has the power of shaping the course of future humanity by using his intelligence to discover and expedite the changes which are necessary to adapt circumstances to man, and man to circumstances.

In considering the evolution of the human race, the science of preventive medicine may afford us some indication of the direction in which to seek for social improvement. One of the early steps towards establishing that science upon a secure basis was taken in 1835 by the British Association, who urged upon the Government the necessity of establishing registers of mortality showing the causes of death 'on one uniform plan in all parts of the King's dominions, as the only means by which general laws touching the influence of causes of disease and death could be satisfactorily deduced.' The general registration of births and deaths was commenced in 1838. But a mere record of death and its proximate cause is insufficient. Preventive medicine requires a knowledge of the details of the previous conditions of life and of occupation. Moreover, death is not our only or most dangerous enemy, and the main object of preventive medicine is to ward off disease. Disease of body lowers our useful energy. Disease of body or of mind may stamp its curse on succeeding generations.

The anthropometric laboratory affords to the student of anthropology a means of analysing the causes of weakness, not only in bodily, but also in mental life.

Mental actions are indicated by movements and their results. Such signs are capable of record, and modern physiology has shown that bodily movements correspond to action in nerve-centres, as surely as the motions of the telegraph-indicator express the movements of the operator's hands in the distant office.

Thus there is a relation between a defective status in brain power and defects in the proportioning of the body. Defects in physiognomical details, too finely graded to be measured with instruments, may be appreciated with accuracy by the senses of the observer ; and the records

show that these defects are, in a large degree, associated with a brain status lower than the average in mental power.

A report presented by one of your committees gives the results of observations made on 100,000 school-children examined individually in order to determine their mental and physical condition for the purpose of classification. This shows that about 16 per 1,000 of the elementary school population appear to be so far defective in their bodily or brain condition as to need special training to enable them to undertake the duties of life, and to keep them from pauperism or crime.

Many of our feeble-minded children, and much disease and vice, are the outcome of inherited proclivities. Francis Galton has shown us that types of criminals which have been bred true to their kind are one of the saddest disfigurements of modern civilisation ; and he says that few deserve better of their country than those who determine to lead celibate lives through a reasonable conviction that their issue would probably be less fitted than the generality to play their part as citizens.

These considerations point to the importance of preventing those suffering from transmissible disease, or the criminal, or the lunatic, from adding fresh sufferers to the teeming misery in our large towns. And in any case, knowing as we do the influence of environment on the development of individuals, they point to the necessity of removing those who are born with feeble minds, or under conditions of moral danger, from surrounding deteriorating influences.

These are problems which materially affect the progress of the human race, and we may feel sure that, as we gradually approach their solution, we shall more certainly realise that the theory of evolution, which the genius of Darwin impressed on this century, is but the first step on a biological ladder which may possibly eventually lead us to understand how in the drama of creation man has been evolved as the highest work of the Creator.

*Bacteriology.*

The sciences of medicine and surgery were largely represented in the earlier meetings of the Association, before the creation of the British Medical Association afforded a field for their more intimate discussion. The close connection between the different branches of science is causing a revival in our proceedings of discussions on some of the highest medical problems, especially those relating to the spread of infectious and epidemic disease.

It is interesting to contrast the opinion prevalent at the foundation of the Association with the present position of the question.

A report to the Association in 1834, by Professor Henry, on contagion, says :—

'The notion that contagious emanations are at all connected with the diffusion of animalculæ through the atmosphere is at variance with all that is known of the diffusion of volatile contagion.'

Whilst it had long been known that filthy conditions in air, earth

and water fostered fever, cholera, and many other forms [of disease, and that the ¡disease ceased to spread on the removal of these conditions, yet the reason for their propagation or diminution remained under a veil.

Leeuwenhoek in 1680 described the yeast-cells, but Schwann in 1837 first showed clearly that fermentation was due to the activity of the yeast-cells ; and, although vague ideas of fermentation had been current during the past century, he laid the foundation of our exact knowledge of the nature of the action of ferments, both organised and unorganised. It was not until 1860, after the prize of the Academy of Sciences had been awarded to Pasteur for his essay against the theory of spontaneous generation, that his investigations into the action of ferments [1] enabled him to show that the effects of the yeast-cell are indissolubly bound up with the activities of the cell as a living organism, and that certain diseases, at least, are due to the action of ferments in the living being. In 1865 he showed that the disease of silkworms, which was then undermining the silk industry in France, could be successfully combated. His further researches into anthrax, fowl cholera, swine fever, rabies, and other diseases proved the theory that those diseases are connected in some way with the introduction of a microbe into the body of an animal ; that the virulence of the poison can be diminished by cultivating the microbes in an appropriate manner ; and that when the virulence has been thus diminished their inoculation will afford a protection against the disease.

Meanwhile it had often been observed in hospital practice that a patient with a simple-fractured limb was easily cured, whilst a patient with a compound fracture often died from the wound. Lister was thence led, in 1865, to adopt his antiseptic treatment, by which the wound is protected from hostile microbes.

These investigations, followed by the discovery of the existence of a multitude of micro-organisms and the recognition of some of them— such as the bacillus of tubercle and the comma bacillus of cholera—as essential factors of disease ; and by the elaboration by Koch and others of methods by which the several organisms might be isolated, cultivated, and their histories studied, have gradually built up the science of bacteriology. Amongst later developments are the discovery of various so-called antitoxins, such as those of diphtheria and tetanus, and the utilisation of these for the cure of disease. Lister's treatment formed a landmark in the science of surgery, and enabled our surgeons to perform operations never before dreamed of ; whilst later discoveries are tending to place the practice of medicine on a firm scientific basis. And the science of bacteriology is leading us to recur to stringent rules for the

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[1] In speaking of ferments one must bear in mind that there are two classes of ferments : one, living beings, such as yeast—' organised ' ferments, as they are sometimes called—the other the products of living beings themselves, such as pepsin, &c —' unorganised ' ferments. Pasteur worked with the former, very little with the latter.

isolation of infectious disease, and to the disinfection (by superheated steam) of materials which have been in contact with the sufferer.

These microbes, whether friendly or hostile, are all capable of multi-plying at an enormous rate under favourable conditions. They are found in the air, in water, in the soil ; but, fortunately, the presence of one species appears to be detrimental to other species, and sunshine, or even light from the sky, is prejudicial to most of them. Our bodies, when in health, appear to be furnished with special means of resisting attacks, and, so far as regards their influence in causing disease, the success of the attack of a pathogenic organism upon an individual depends, as a rule, in part at least, upon the power of resistance of the individual.

But notwithstanding our knowledge of the danger arising from a state of low health in individuals, and of the universal prevalence of these micro-organisms, how careless we are in guarding the health conditions of everyday life ! We have ascertained that pathogenic organisms pervade the air Why, therefore, do we allow our meat, our fish, our vegetables, our easily contaminated milk, to be exposed to their inroads, often in the foulest localities ? We have ascertained that they pervade the water we drink, yet we allow foul water from our dwellings, our pigsties, our farmyards, to pass into ditches without previous clarification, whence it flows into our streams and pollutes our rivers. We know the conditions of occupation which foster ill-health. Why, whilst we remove outside sources of impure air, do we permit the occupation of foul and unhealthy dwellings ?

The study of bacteriology has shown us that although some of these organisms may be the accompaniments of disease, yet we owe it to the operation of others that the refuse caused by the cessation of animal and vegetable life is reconverted into food for fresh generations of plants and animals.

These considerations have formed a point of meeting where the biologist, the chemist, the physicist, and the statistician unite with the sanitary engineer in the application of the science of preventive medicine.

ENGINEERING.

*Sewage Purification.*

The early reports to the Association show that the laws of hydro-statics, hydrodynamics, and hydraulics necessary to the supply and removal of water through pipes and conduits had long been investigated by the mathematician. But the modern sanitary engineer has been driven by the needs of an increasing population to call in the chemist and the biologist to help him to provide pure water and pure air.

The purification and the utilisation of sewage occupied the attention of the British Association as early as 1864, and between 1869 and 1876 a committee of the Association made a series of valuable reports on the subject. The direct application of sewage to land, though effective as a

means of purification, entailed difficulties in thickly settled districts, owing to the extent of land required.

The chemical treatment of sewage produced an effluent harmless only after having been passed over land, or if turned into a large and rapid stream, or into a tidal estuary ; and it left behind a large amount of sludge to be dealt with.

Hence it was long contended that the simplest plan in favourable localities was to turn the sewage into the sea, and that the consequent loss to the land of the manurial value in the sewage would be recouped by the increase in fish-life

It was not till the chemist called to his aid the biologist, and came to the help of the engineer, that a scientific system of sewage purification was evolved.

Dr. Frankland many years ago suggested the intermittent filtration of sewage ; and Mr. Baldwin Latham was one of the first engineers to adopt it. But the valuable experiments made in recent years by the State Board of Health in Massachusetts have more clearly explained to us how by this system we may utilise micro-organisms to convert organic impurity in sewage into food fitted for higher forms of life.

To effect this we require, in the first place, a filter about five feet thick of sand and gravel, or, indeed, of any material which affords numerous surfaces or open pores. Secondly, that after a volume of sewage has passed through the filter, an interval of time be allowed, in which the air necessary to support the life of the micro-organisms is enabled to enter the pores of the filter. Thus this system is dependent upon oxygen and time. Under such conditions the organisms necessary for purification are sure to establish themselves in the filter before it has been long in use. Temperature is a secondary consideration.

Imperfect purification can invariably be traced either to a lack of oxygen in the pores of the filter, or to the sewage passing through so quickly that there is not sufficient time for the necessary processes to take place. And the power of any material to purify either sewage or water depends almost entirely upon its ability to hold a sufficient proportion of either sewage or water in contact with a proper amount of air.

### Smoke Abatement.

Whilst the sanitary engineer has done much to improve the surface conditions of our towns, to furnish clean water, and to remove our sewage, he has as yet done little to purify town air. Fog is caused by the floating particles of matter in the air becoming weighted with aqueous vapour ; some particles, such as salts of ammonia or chloride of sodium, have a greater affinity for moisture than others. You will suffer from fog so long as you keep refuse stored in your towns to furnish ammonia, or so long as you allow your street surfaces to supply dust, of which much consists of powdered horse manure, or so long as you send the products of

combustion into the atmosphere. Therefore, when you have adopted mechanical traction for your vehicles in towns you may largely reduce one cause of fog. And if you diminish your black smoke, you will diminish black fogs.

In manufactories you may prevent smoke either by care in firing, by using smokeless coal, or by washing the soot out of the products of consumption in its passage along the flue leading to the main chimney-shaft.

The black smoke from your kitchen may be avoided by the use of coke or of gas. But so long as we retain the hygienic arrangement of the open fire in our living-rooms I despair of finding a fireplace, however well constructed, which will not be used in such a manner as to cause smoke, unless, indeed, the chimneys were reversed and the fumes drawn into some central shaft, where they might be washed before being passed into the atmosphere.

Electricity as a warming and cooking agent would be convenient, cleanly, and economical when generated by water power, or possibly wind power, but it is at present too dear when it has to be generated by means of coal. I can conceive, however, that our descendants may learn so to utilise electricity that they in some future century may be enabled by its means to avoid the smoke in their towns.

*Mechanical Engineering.*

In other branches of civil and mechanical engineering, the reports in 1831 and 1832 on the state of this science show that the theoretical and practical knowledge of the strength of timber had obtained considerable development. But in 1830, before the introduction of railways, cast iron had been sparingly used in arched bridges for spans of from 160 to 200 feet, and wrought iron had only been applied to large-span iron bridges on the suspension principle, the most notable instance of which was the Menai Suspension Bridge, by Telford. Indeed, whilst the strength of timber had been patiently investigated by engineers, the best form for the use of iron girders and struts was only beginning to attract attention, and the earlier volumes of our Proceedings contained numerous records of the researches of Eaton Hodgkinson, Barlow, Rennie, and others. It was not until twenty years later that Robert Stephenson and William Fairbairn erected the tubular bridge at Menai, followed by the more scientific bridge erected by Brunel at Saltash. These have now been entirely eclipsed by the skill with which the estuary of the Forth has been bridged with a span of 1,700 feet by Sir John Fowler and Sir Benjamin Baker.

The development of the iron industry is due to the association of the chemist with the engineer. The introduction of the hot blast by Neilson, in 1829, in the manufacture of cast iron had effected a large saving of fuel. But the chemical conditions which affect the strength and other qualities of iron, and its combinations with carbon, silicon, phosphorus, and other substances, had at that time scarcely been investigated.

In 1856 Bessemer brought before the British Association at Cheltenham his brilliant discovery for making steel direct from the blast furnace, by which he dispensed with the laborious process of first removing the carbon from pig-iron by puddling, and then adding by cementation the required proportion of carbon to make steel. This discovery, followed by Siemens's regenerative furnace, by Whitworth's compressed steel, and by the use of alloys and by other improvements too numerous to mention here, have revolutionised the conditions under which metals are applied to engineering purposes.

Indeed, few questions are of greater interest, or possess more industrial importance, than those connected with metallic alloys. This is especially true of those alloys which contain the rarer metals; and the extraordinary effects of small quantities of chromium, nickel, tungsten and titanium on certain varieties of steel have exerted profound influence on the manufacture of projectiles and on the construction of our armoured ships.

Of late years, investigations on the properties and structure of alloys have been numerous, and among the more noteworthy researches may be mentioned those of Dewar and Fleming on the distinctive behaviour, as regards the thermo-electric powers and electrical resistance, of metals and alloys at the very low temperatures which may be obtained by the use of liquid air.

Professor Roberts-Austen, on the other hand, has carefully studied the behaviour of alloys at very high temperatures, and by employing his delicate pyrometer has obtained photographic curves which afford additional evidence as to the existence of allotropic modifications of metals, and which have materially strengthened the view that alloys are closely analogous to saline solutions. In this connection it may be stated that the very accurate work of Heycock and Neville on the lowering of the solidifying points of molten metals, which is caused by the presence of other metals, affords a valuable contribution to our knowledge.

Professor Roberts-Austen has, moreover, shown that the effect of any one constituent of an alloy upon the properties of the principal metal has a direct relation to the atomic volumes, and that it is consequently possible to foretell, in a great measure, the effect of any given combination.

A new branch of investigation, which deals with the micro-structure of metals and alloys, is rapidly assuming much importance. It was instituted by Sorby in a communication which he made to the British Association in 1864, and its development is due to many patient workers, among whom M. Osmond occupies a prominent place.

Metallurgical science has brought aluminium into use by cheapening the process of its extraction; and if by means of the wasted forces in our rivers, or possibly of the wind, the extraction be still further cheapened by the aid of electricity, we may not only utilise the metal or its alloys in increasing the spans of our bridges, and in affording strength and lightness in the construction of our ships, but we may hope to obtain a material which may render practicable the dreams of Icarus and of Maxim, and for purposes of rapid transit enable us to navigate the air.

Long before 1831 the steam-engine had been largely used on rivers and lakes, and for short sea passages, although the first Atlantic steam-service was not established till 1838.

As early as 1820 the steam-engine had been applied by Gurney, Hancock, and others to road traction. The absurd impediments placed in their way by road trustees, which, indeed, are still enforced, checked any progress. But the question of mechanical traction on ordinary roads was practically shelved in 1830, at the time of the formation of the British Association, when the locomotive engine was combined with a tubular boiler and an iron road on the Liverpool and Manchester Railway.

Great, however, as was the advance made by the locomotive engine of Robert Stephenson, these earlier engines were only toys compared with the compound engines of to-day which are used for railways, for ships, or for the manufacture of electricity. Indeed, it may be said that the study of the laws of heat, which have led to the introduction of various forms of motive power, are gradually revolutionising all our habits of life.

The improvements in the production of iron, combined with the developed steam-engine, have completely altered the conditions of our commercial intercourse on land ; whilst the changes caused by the effects of these improvements in shipbuilding, and on the ocean carrying trade, have been, if anything, still more marked.

At the foundation of the Association all ocean ships were built by hand, of wood, propelled by sails and manœuvred by manual labour ; the material limited their length, which did not often exceed 100 feet, and the number of English ships of over 500 tons burden was comparatively small.

In the modern ships steam power takes the place of manual labour. It rolls the plates of which the ship is constructed, bends them to the required shape, cuts, drills and rivets them in their place. It weighs the anchor ; it propels the ship in spite of winds or currents ; it steers, ventilates, and lights the ship when on the ocean. It takes the cargo on board and discharges it on arrival

The use of iron favours the construction of ships of a large size, of forms which afford small resistance to the water, and with compartments which make the ships practically unsinkable in heavy seas, or by collision   Their size, the economy with which they are propelled, and the certainty of their arrival, cheapens the cost of transport.

The steam-engine, by compressing air, gives us control over the temperature of cool chambers. In these not only fresh meat, but the delicate produce of the Antipodes, is brought across the ocean to our doors without deterioration.

Whilst railways have done much to alter the social conditions of each individual nation, the application of iron and steam to our ships is revolutionising the international commercial conditions of the world ; and it is gradually changing the course of our agriculture, as well as of our domestic life.

But great as have been the developments of science in promoting the commerce of the world, science is asserting its supremacy even to a greater extent in every department of war. And perhaps this application of science affords at a glance, better than almost any other, a convenient illustration of the assistance which the chemical, physical, and electrical sciences are affording to the engineer.

The reception of warlike stores is not now left to the uncertain judgment of 'practical men,' but is confided to officers who have received a special training in chemical analysis, and in the application of physical and electrical science to the tests by which the qualities of explosives, of guns, and of projectiles can be ascertained.

For instance, take explosives. Till quite recently black and brown powders alone were used, the former as old as civilisation, the latter but a small modern improvement adapted to the increased size of guns. But now the whole family of nitro-explosives are rapidly superseding the old powder. These are the direct outcome of chemical knowledge ; they are not mere chance inventions, for every improvement is based on chemical theories, and not on random experiment.

The construction of guns is no longer a haphazard operation. In spite of the enormous forces to be controlled and the sudden violence of their action, the researches of the mathematician have enabled the just proportions to be determined with accuracy ; the labours of the physicist have revealed the internal conditions of the materials employed, and the best means of their favourable employment. Take, for example, Longridge's coiled-wire system, in which each successive layer of which the gun is formed receives the exact proportion of tension which enables all the layers to act in unison. The chemist has rendered it clear that even the smallest quantities of certain ingredients are of supreme importance in affecting the tenacity and trustworthiness of the materials.

The treatment of steel to adapt it to the vast range of duties it has to perform is thus the outcome of patient research. And the use of the metals—manganese, chromium, nickel, molybdenum—as alloys with iron has resulted in the production of steels possessing varied and extraordinary properties. The steel required to resist the conjugate stresses developed, lightning fashion, in a gun necessitates qualities that would not be suitable in the projectile which that gun hurls with a velocity of some 2,500 feet per second against the armoured side of a ship. The armour, again, has to combine extreme superficial hardness with great toughness, and during the last few years these qualities are sought to be attained by the application of the cementation process for adding carbon to one face of the plate, and hardening that face alone by rapid refrigeration.

The introduction of quick-firing guns from ·303 (*i.e.* about one-third) of an inch to 6-inch calibre has rendered necessary the production of metal cartridge-cases of complex forms drawn cold out of solid blocks or plate of the material; this again has taxed the ingenuity of the mechanic in the device of machinery, and of the metallurgist in producing a metal possessed

of the necessary ductility and toughness. The cases have to stand a pressure at the moment of firing of as much as twenty-five tons to the square inch—a pressure which exceeds the ordinary elastic limits of the steel of which the gun itself is composed.

There is nothing more wonderful in practical mechanics than the closing of the breech openings of guns, for not only must they be gas-tight at these tremendous pressures, but the mechanism must be such that one man by a single continuous movement shall be able to open or close the breech of the largest gun in some ten or fifteen seconds

The perfect knowledge of the recoil of guns has enabled the reaction of the discharge to be utilised in compressing air or springs by which guns can be raised from concealed positions in order to deliver their fire, and then made to disappear again for loading ; or the same force has been used to run up the guns automatically immediately after firing, or, as in the case of the Maxim gun, to deliver in the same way a continuous stream of bullets at the rate of ten in one second.

In the manufacture of shot and shell cast iron has been almost super-seded by cast and wrought steel, though the hardened Palliser projectiles still hold their place. The forged-steel projectiles are produced by methods very similar to those used in the manufacture of metal cartridge-cases, though the process is carried on at a red heat and by machines much more powerful.

In every department concerned in the production of warlike stores electricity is playing a more and more important part. It has enabled the passage of a shot to be followed from its seat in the gun to its destination.

In the gun, by means of electrical contacts arranged in the bore, a time-curve of the passage of the shot can be determined.

From this the mathematician constructs the velocity-curve, and from this, again, the pressures producing the velocity are estimated, and used to check the same indications obtained by other means. The velocity of the shot after it has left the gun is easily ascertained by the Boulangé apparatus.

Electricity and photography have been laid under contribution for obtaining records of the flight of projectiles and the effects of explosions at the moment of their occurrence. Many of you will recollect Mr. Vernon Boys' marvellous photographs showing the progress of the shot driving before it waves of air in its course.

Electricity and photography also record the properties of metals and their alloys as determined by curves of cooling.

The readiness with which electrical energy can be converted into heat or light has been taken advantage of for the firing of guns, which in their turn can, by the same agency, be laid on the object by means of range-finders placed at a distance and in advantageous and safe positions ; while the electric light is utilised to illumine the sights at night, as well as to search out the objects of attack.

The compact nature of the glow-lamp, the brightness of the light, the circumstance that the light is not due to combustion, and therefore independent of air, facilitates the examination of the bore of guns, the insides of shells, and other similar uses—just as it is used by a doctor to examine the throat of a patient.

### INFLUENCE OF INTERCOMMUNICATION AFFORDED BY BRITISH ASSOCIATION ON SCIENCE PROGRESS.

The advances in engineering which have produced the steam-engine, the railway, the telegraph, as well as our engines of war, may be said to be the result of commercial enterprise rendered possible only by the advances which have taken place in the several branches of science since 1831. Having regard to the intimate relations which the several sciences bear to each other, it is abundantly clear that much of this progress could not have taken place in the past, nor could further progress take place in the future, without intercommunication between the students of different branches of science.

The founders of the British Association based its claims to utility upon the power it afforded for this intercommunication. Mr. Vernon Harcourt (the uncle of your present General Secretary), in the address he delivered in 1832, said : 'How feeble is man for any purpose when he stands alone—how strong when united with other men !

'It may be true that the greatest philosophical works have been achieved in privacy, but it is no less true that these works would never have been accomplished had the authors not mingled with men of corresponding pursuits, and from the commerce of ideas often gathered germs of apparently insulated discoveries, and without such material aid would seldom have carried their investigations to a valuable conclusion.'

I claim for the British Association that it has fulfilled the objects of its founders, that it has had a large share in promoting intercommunication and combination.

Our meetings have been successful because they have maintained the true principles of scientific investigation. We have been able to secure the continued presence and concurrence of the master-spirits of science. They have been willing to sacrifice their leisure, and to promote the welfare of the Association, because the meetings have afforded them the means of advancing the sciences to which they are attached.

The Association has, moreover, justified the views of its founders in promoting intercourse between the pursuers of science, both at home and abroad, in a manner which is afforded by no other agency.

The weekly and sessional reunions of the Royal Society, and the annual *soirées* of other scientific societies, promote this intercourse to some extent, but the British Association presents to the young student during its week of meetings easy and continuous social opportunities for

making the acquaintance of leaders in science, and thereby obtaining their directing influence.

It thus encourages, in the first place, opportunities of combination, but, what is equally important, it gives at the same time material assistance to the investigators whom it thus brings together.

The reports on the state of science at the present time, as they appear in the last volume of our Proceedings, occupy the same important position, as records of science progress, as that occupied by those Reports in our earlier years. We exhibit no symptom of decay.

### SCIENCE IN GERMANY FOSTERED BY THE STATE AND MUNICIPALITIES.

Our neighbours and rivals rely largely upon the guidance of the State for the promotion of both science teaching and of research. In Germany the foundations of technical and industrial training are laid in the Realschulen, and supplemented by the Higher Technical Schools. In Berlin that splendid institution, the Royal Technical High School, casts into the shade the facilities for education in the various Polytechnics which we are now establishing in London. Moreover, it assists the practical workman by a branch department, which is available to the public for testing building materials, metals, paper, oil, and other matters. The standards of all weights and measures used in trade can be purchased from or tested by the Government Department for Weights and Measures.

For developing pure scientific research and for promoting new applications of science to industrial purposes the German Government, at the instance of von Helmholtz, and aided by the munificence of Werner von Siemens, created the Physikalische Technische Reichsanstalt at Charlottenburg.

This establishment consists of two divisions The first is charged with pure research, and is at the present time engaged in various thermal, optical, and electrical and other physical investigations. The second branch is employed in operations of delicate standardising to assist the wants of research students—for instance, dilatation, electrical resistances, electric and other forms of light, pressure gauges, recording instruments, thermometers, pyrometers, tuning forks, glass, oil-testing apparatus, viscosity of glycerine, &c.

Dr. Kohlrausch succeeded Helmholtz as president, and takes charge of the first division. Professor Hagen, the director under him, has charge of the second division. A professor is in charge of each of the several sub-departments. Under these are various subordinate posts, held by younger men, selected for previous valuable work, and usually for a limited time.

The general supervision is under a Council consisting of a president, who is a Privy Councillor, and twenty-four members, including the president and director of the Reichsanstalt ; of the other members, about ten are professors or heads of physical and astronomical observatories

connected with the principal universities in Germany. Three are selected from leading firms in Germany representing mechanical, optical, and electric science, and the remainder are principal scientific officials connected with the Departments of War and Marine, the Royal Observatory at Potsdam, and the Royal Commission for Weights and Measures

This Council meets in the winter, for such time as may be necessary, for examining the research work done in the first division during the previous year, and for laying down the scheme for research for the ensuing year ; as well as for suggesting any requisite improvements in the second division. As a consequence of the position which science occupies in connection with the State in Continental countries, the services of those who have distinguished themselves either in the advancement or in the application of science are recognised by the award of honours ; and thus the feeling for science is encouraged throughout the nation.

### ASSISTANCE TO SCIENTIFIC RESEARCH IN GREAT BRITAIN.

Great Britain maintained for a long time a leading position among the nations of the world by virtue of the excellence and accuracy of its workmanship, the result of individual energy ; but the progress of mechanical science has made accuracy of workmanship the common property of all nations of the world. Our records show that hitherto, in its efforts to maintain its position by the application of science and the prosecution of research, England has made marvellous advances by means of voluntary effort, illustrated by the splendid munificence of such men as Gassiot, Joseph Whitworth, James Mason, and Ludwig Mond ; and, whilst the increasing field of scientific research compels us occasionally to seek for Government assistance, it would be unfortunate if by any change voluntary effort were fettered by State control

The following are the principal voluntary agencies which help forward scientific research in this country :—The Donation Fund of the Royal Society, derived from its surplus income. The British Association has contributed 60,000l. to aid research since its formation. The Royal Institution, founded in the last century, by Count Rumford, for the promotion of research, has assisted the investigations of Davy, of Young, of Faraday, of Frankland, of Tyndall, of Dewar, and of Rayleigh. The City Companies assist scientific research and foster scientific education both by direct contributions and through the City and Guilds Institute. The Commissioners of the Exhibition of 1851 devote 6,000l. annually to science research scholarships, to enable students who have passed through a college curriculum and have given evidence of capacity for original research to continue the prosecution of science, with a view to its advance or to its application to the industries of the country. Several scientific societies, as, for instance, the Geographical Society and the Mechanical Engineers, have promoted direct research, each in their own branch of science, out of their surplus income ; and every scientific society largely assists research by the publication, not only of its own proceedings, but

often of the work going on abroad in the branch of science which it represents.

The growing abundance of matter year by year increases the burden thus thrown on their finances, and the Treasury has recently granted to the Royal Society 1,000*l.* a year, to be spent in aid of the publication of scientific papers not necessarily limited to those of that Society.

The Royal Society has long felt the importance to scientific research of a catalogue of all papers and publications relating to pure and applied science, arranged systematically both as to authors' names and as to subject treated, and the Society has been engaged for some time upon a catalogue of that nature. But the daily increasing magnitude of these publications, coupled with the necessity of issuing the catalogue with adequate promptitude, and at appropriate intervals, renders it a task which could only be performed under International co-operation. The officers of the Royal Society have therefore appealed to the Government to urge Foreign Governments to send delegates to a Conference to be held next July to discuss the desirability and the scope of such a catalogue, and the possibility of preparing it.

The universities and colleges distributed over the country, besides their function of teaching, are large promoters of research, and their voluntary exertions are aided in some cases by contributions from Parliament in alleviation of their expenses.

Certain executive departments of the Government carry on research for their own purposes, which in that respect may be classed as voluntary. The Admiralty maintains the Greenwich Observatory, the Hydrographical Department, and various experimental services ; and the War Office maintains its numerous scientific departments. The Treasury maintains a valuable chemical laboratory for Inland Revenue, Customs, and agricultural purposes. The Science and Art Department maintains the Royal College of Science, for the education of teachers and students from elementary schools. It allows the scientific apparatus in the national museum to be used for research purposes by the professors. The Solar Physics Committee, which has carried on numerous researches in solar physics, was appointed by and is responsible to this Department. The Department also administers the Sir Joseph Whitworth engineering research scholarships. Other scientific departments of the Government are aids to research, as, for instance, the Ordnance and the Geological Surveys, the Royal Mint, the Natural History Museum, Kew Gardens, and other lesser establishments in Scotland and Ireland ; to which may be added, to some extent, the Standards Department of the Board of Trade, as well as municipal museums, which are gradually spreading over the country.

For direct assistance to voluntary effort the Treasury contributes 4,000*l.* a year to the Royal Society for the promotion of research, which is administered under a board whose members represent all branches of Science. The Treasury, moreover, contributes to marine biological observatories, and in recent years has defrayed the cost of various expedi-

tions for biological and astronomical research, which in the case of the 'Challenger' expedition involved very large sums of money.

In addition to these direct aids to science, Parliament, under the Local Taxation Act, handed over to the County Councils a sum, which amounted in the year 1893 to 615,000*l.*, to be expended on technical education. In many country districts, so far as the advancement of real scientific technical progress in the nation is concerned, much of this money has been wasted for want of knowledge. And whilst it cannot be said that the Government or Parliament have been indifferent to the promotion of scientific education and research, it is a source of regret that the Government did not devote some small portion of this magnificent gift to affording an object-lesson to County Councils in the application of science to technical instruction, which would have suggested the principles which would most usefully guide them in the expenditure of this public money.

Government assistance to science has been based mainly on the principle of helping voluntary effort. The Kew Observatory was initiated as a scientific observatory by the British Association. It is now supported by the Gassiot trust fund, and managed by the Kew Observatory Committee of the Royal Society. Observations on magnetism, on meteorology, and the record of sun-spots, as well as experiments upon new instruments for assisting meteorological, thermometrical, and photographic purposes, are being carried on there. The Committee has also arranged for the verification of scientific measuring instruments, the rating of chronometers, the testing of lenses and of other scientific apparatus. This institution carries on to a limited extent some small portion of the class of work done in Germany by that magnificent institution, the Reichsanstalt at Charlottenburg, but its development is fettered by want of funds. British students of science are compelled to resort to Berlin and Paris when they require to compare their more delicate instruments and apparatus with recognised standards. There could scarcely be a more advantageous addition to the assistance which Government now gives to science than for it to allot a substantial annual sum to the extension of the Kew Observatory, in order to develop it on the model of the Reichsanstalt. It might advantageously retain its connection with the Royal Society, under a Committee of Management representative of the various branches of science concerned, and of all parts of Great Britain.

## CONCLUSION.

The various agencies for scientific education have produced numerous students admirably qualified to pursue research ; and at the same time almost every field of industry presents openings for improvement through the development of scientific methods. For instance, agricultural operations alone offer openings for research to the biologist, the chemist, the physicist, the geologist, the engineer, which have hitherto been largely

overlooked.   If students do not easily find employment, it is chiefly attributable to a want of appreciation for science in the nation at large.

This want of appreciation appears to arise from the fact that those who
nearly half a century ago directed the movement of national education
were trained in early life in the universities, in which the value of scientific
methods was not at that time fully recognised.   Hence our elementary,
and even our secondary and great public schools, neglected for a long time
to encourage the spirit of investigation which develops originality.   This
defect is diminishing daily.

There is, however, a more intangible cause which may have had influence
on the want of appreciation of science by the nation.   The Government,
which largely profits by science, aids it with money, but it has done very
little to develop the national appreciation for science by recognising that
its leaders are worthy of honours conferred by the State.   Science is not
fashionable, and science students—upon whose efforts our progress as a
nation so largely depends—have not received the same measure of recognition which the State awards to services rendered by its own officials, by
politicians, and by the Army and by the Navy, whose success in future
wars will largely depend on the effective applications of science.

The Reports of the British Association afford a complete chronicle of
the gradual growth of scientific knowledge since 1831.   They show that
the Association has fulfilled the objects of its founders in promoting and
disseminating a knowledge of science throughout the nation.

The growing connection between the sciences places our annual
meeting in the position of an arena where representatives of the different
sciences have the opportunity of criticising new discoveries and testing
the value of fresh proposals, and the Presidential and Sectional Addresses
operate as an annual stock-taking of progress in the several branches of
science represented in the Sections.   Every year the field of usefulness of
the Association is widening.   For, whether with the geologist we seek
to write the history of the crust of the earth, or with the biologist to trace
out the evolution of its inhabitants, or whether with the astronomer, the
chemist, and the physicist we endeavour to unravel the constitution of the
sun and the planets or the genesis of the nebulæ and stars which make
up the universe, on every side we find ourselves surrounded by mysteries
which await solution.   We are only at the beginning of work.

I have, therefore, full confidence that the future records of the British
Association will chronicle a still greater progress than that already
achieved, and that the British nation will maintain its leading position
amongst the nations of the world, if it will energetically continue its
voluntary efforts to promote research, supplemented by that additional
help from the Government which ought never to be withheld when a
clear case of scientific utility has been established.

# British Association for the Advancement of Science.

## IPSWICH, 1895.

# ADDRESS

### TO THE

# MATHEMATICAL AND PHYSICAL SECTION

### BY

### PROFESSOR W. M. HICKS, M.A., D.Sc, F.R.S

#### PRESIDENT OF THE SECTION.

IN making a choice of subject for my address the difficulty is not one of finding material but of making selection. The field covered by this Section is a wide one. Investigation is active in every part of it, and is being rewarded with a continuous stream of new discoveries and with the growth of that co-ordination and correlation of facts which is the surest sign of real advancement in science. The ultimate aim of pure science is to be able to explain the most complicated phenomena of nature as flowing by the fewest possible laws from the simplest fundamental data. A statement of a law is either a confession of ignorance or a mnemonic convenience. It is the latter, if it is deducible by logical reasoning from other laws. It is the former when it is only discovered as a fact to be a law. While, on the one hand, the end of scientific investigation is the discovery of laws, on the other, science will have reached its highest goal when it shall have reduced ultimate laws to one or two, the necessity of which lies outside the sphere of our cognition. These ultimate laws—in the domain of physical science at least—will be the dynamical laws of the relations of matter to number, space, and time. The ultimate data will be number, matter, space, and time themselves. When these relations shall be known, all physical phenomena will be a branch of pure mathematics. We shall have done away with the necessity of the conception of potential energy, even if it may still be convenient to retain it; and—if it should be found that all phenomena are manifestations of motion of one single continuous medium —the idea of force will be banished also, and the study of dynamics replaced by the study of the equation of continuity.

Before, however, this can be attained, we must have the working drawings of the details of the mechanism we have to deal with. These details lie outside the scope of our bodily senses; we cannot see, or feel, or hear them, and this, not because they are unseeable, but because our senses are too coarse-grained to transmit impressions of them to our mind. The ordinary methods of investigation here fail us; we must proceed by a special method, and make a bridge of communication between the mechanism and our senses by means of hypotheses. By our imagination, experience, intuition we form theories; we deduce the consequences of these theories on phenomena which come within the range of our senses, and reject or modify and try again. It is a slow and laborious process. The wreckage of rejected theories is appalling; but a knowledge of what actually goes on behind what we can see or feel is surely if slowly being

attained. It is the rejected theories which have been the necessary steps towards formulating others nearer the truth. It would be an extremely interesting study to consider the history of these discarded theories; to show the part they have taken in the evolution of truer conceptions, and to trace the persistence and modification of typical ideas from one stratum of theories to a later. I propose, however, to ask your attention for a short time to one of these special theories—or rather to two related theories—on the constitution of matter and of the ether. They are known as the vortex atom theory of matter, and the vortex sponge theory of the ether. The former has been before the scientific world for a quarter of a century, since its first suggestion by Lord Kelvin in 1867, the second for about half that time. In what I have to say I wish to take the position not of an advocate for or against. but simply as a prospector attempting to estimate what return is likely to be obtained by laying down plant to develop an unknown district. This is in fact the state of these two theories at present. Extremely little progress has been made in their mathematical development, and until this has been done more completely we cannot test them as to their powers of adequately explaining physical phenomena.

The theory of the rigid atom has been a very fruitful one, especially in explaining the properties of matter in the gaseous state; but it gives no explanation of the apparent forces which hold atoms together, and in many other respects it requires supplementing. The elastic solid ether explained much, but there are difficulties connected with it—especially in connection with reflection and refraction —which decide against it. The mathematical rotational ether of MacCullagh is admirably adapted to meet these difficulties, but he could give no physical conception of its mechanism. Maxwell and Faraday proposed a special ether for electrical and magnetic actions. Maxwell's identification of the latter with the luminiferous ether, his deduction of the velocity of propagation of light and of indices of refraction in terms of known electrical and magnetic constants, will form one of the landmarks in the history of science. This ether requires the same mathematical treatment as that of MacCullagh. Lord Kelvin's gyrostatic model of an ether is also of the MacCullagh type. Lastly, we have Lord Kelvin's Labile ether, which again avoids the objections to the elastic solid ether. In MacCullagh's type of ether the energy of the medium when disturbed depends only on the twists produced in it. This ether has recently been mathematically discussed by Dr. Larmor, who has shown that it is adequate to explain all the various phenomena of light, electricity, and magnetism. To this I hope to return later. Meanwhile, it may be borne in mind that the vortex sponge ether belongs to MacCullagh's type.

Already before a formal theory of a fluid ether had been attempted, Lord Kelvin [1] had proposed his theory of vortex atoms. The permanence.of a vortex filament with its infinite flexibility, its fundamental simplicity with its potential capacity for complexity, struck the scientific imagination as the thing which was wanted. Unfortunately the mathematical difficulties connected with the discussion of these motions, especially the reactions of one on another, have retarded the full development of the theory. Two objections in chief have been raised against it, viz. the difficulty of accounting for the densities of various kinds of matter, and the fact that in a vortex ring the velocity of translation decreases as the energy increases. There are two ways of dealing with a difficulty occurring in a general theory—one is to give up the theory, the other is to try and see if it can be modified to get over the difficulty. Such difficulties are to be welcomed as means of help in arriving at greater exactness in details. It is a mistake to submit too readily to crucial experiments. The very valid crucial objection of Stokes to MacCullagh's ether is a case in point. It drew away attention from a theory which, in the light of later developments, gives great hope of leading us to correct ideas. As Larmor has pointed out, this objection vanishes when we have intrinsic rotations in the ether itself. A special danger to guard against is the importation into one theory of ideas which have grown out of one essentially different. This

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[1] 'Vortex Atoms,' *Proc. Roy. Soc. Edin.*, vi. 94; *Phil. Mag.* (4), 34.

remark has reference to the apparent difficulty of decrease of velocity with increased energy.

Maxwell was, I believe, the first to point out the difficulty of explaining the masses of the elements on the vortex atom hypothesis. To me it has always appeared one of the greatest stumbling-blocks to the acceptance of the theory. We have always been accustomed to regard the ether as of extreme tenuity, as of a density extremely though not infinitely smaller than that of gross matter, and we carry in our minds that Lord Kelvin has given an inferior limit of about $10^{-19}$. There are two directions in which to seek a solution. The first is to cut the knot by supposing that the atoms of gross matter are composed of filaments whose rotating cores are of much greater density than the ether itself. The second is to remember that Lord Kelvin's number was obtained on the supposition of elastic solid ether, and does not necessarily apply to the vortex sponge. Unfortunately, however, for the first explanation, the mathematical discussion [1] shows that a ring cannot be stable unless the density of the fluid outside the core is equal to, or greater than, that inside. This instability also cannot be cured by supposing an additional circulation added outside the core. Unless, therefore, some modification of the theory can be made to secure stability this idea of dense fluid cores must be given up.

We seem, therefore, forced back to the conclusion that the density of the ether must be comparable with that of ordinary matter. The effective mass of any atom is not composed of that of its core alone, but also of that portion of the surrounding ether which is carried along with it as it moves through the medium. Thus a rigid sphere moving in a liquid behaves as if its mass were increased by half that of the displaced liquid. In the case of a vortex filament the ratio of effective to actual mass may be much larger. In this explanation the density of the matter composing an atom is the same for all, whilst their masses depend on their volumes and configurations combined. Now the configuration alters with the energy, and this would make the mass depend to some extent at least on the temperature. However repugnant this may be to current ideas, we are not entitled to deny its possibility, although such an effect must be small or it would have been detected. Such a variation, if it exists, is not to be looked for by means of the ordinary gravitation balance, but by the inertia or ballistic balance. The mass of the core itself remains, of course, constant, but the effective mass—that which we can measure by the mechanical effects which the moving vortex produces—is a much more complicated matter, and requires much fuller consideration than has been given to it.

The conditions of stability allow us to assume vacuous cores or cores of less density than the rest of the medium. If we do this then the density of the ether itself may be greater than that of gross matter. Until, however, we meet with phenomena whose explanation requires this assumption, it would seem preferable to take the density everywhere the same. In this case the density of the ether must be rather less than the apparent density of the lightest of any of the elements, taking the apparent density to mean the effective mass of a vortex atom per its volume. This will probably be commensurable with the density of the matter in its most compressed state, and will lie between ·5 and 1—comparable, that is to say, with the density of water. Larmor,[2] from a special form of hypothesis for a magnetic field in the rotationally elastic ether, is led to assign a density of the same order of magnitude. If the density be given it is easy to calculate the intrinsic energy per c.c. in the medium. The velocity of propagation of light in a vortex sponge ether, as deduced by Lord Kelvin,[3] is ·47 times the mean square

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[1] An error in the expression on p 768 of 'Researches in the Theory of Vortex Rings,' *Phil. Trans*, pt. ii. 1885, vitiates the conclusion there drawn. If this be corrected the result mentioned above follows. See also Basset, *Treatise on Hydrodynamics*, § 338, and *Amer. Jour. Math.*

[2] 'A Dynamical Theory of the Electric and Luminiferous Medium,' *Phil. Trans*, 1894, A. p. 779.

[3] 'On the Propagation of Laminar Motion through a Turbulently Moving Inviscid Liquid,' *Phil. Mag*, October 1887.

velocity of the intrinsic motion of the medium. This gives for the mean square velocity $6.3 \times 10^{10}$ cm. per second. If we follow Lord Kelvin and use for comparison the energy of radiation per c.c. near the sun, or say 1·8 erg per c.c., the resulting density will be $10^{-21}$. The energy per c.c. in a magnetic field of 15,000 c g.s. units is about 1 joule. If we take this for comparison we get a density of $10^{-14}$. But the intrinsic energy of the fluid must be extremely great compared with the energy it has to transmit. If it were a million times greater the density would still only amount to $10^{-8}$—comparable with the density of the residual gas in our highest vacua. To account for the density of gross matter on the supposition that it is built up out of the same material as the ether leads to a density between ·5 and 1. This gives the enormous energy of $10^{14}$ joules per c c. In other words, the energy contained in one cubic centimetre of the ether is sufficient to raise a kilometre cube of lead 1 metre high against its weight. Thus the difficulty in explaining the mass of ordinary matter seems to reduce itself to a difficulty in believing that the ether possesses such an enormous store of energy. It may be that there are special reasons against such a large density. Larmor refers to the large forcives which would be called into play by hydrodynamical motions. Perhaps an answer to this may be found in the remark that where all the matter is of the same density the motions are kinematically deducible from the configuration at the instant, and are independent of the density. It is only where other causes act, such, e.g., as indirectly depend on the mean pressure of the fluid or where vacuous spaces occur, that the actual value of the density may modify the measurable forcives.

Ever since Professor J. J. Thomson proved that a vortex atom theory of matter is competent to serve as a basis of a kinetic theory of gases, it has been urged by various persons as a fatal objection that the translation velocity of the atoms falls off as the temperature rises. I must confess this objection has never appealed to me. Why should not the velocity fall off? The velocity of gaseous molecules has never been directly observed, nor has it been experimentally proved that it increases with rise of temperature. We have no right to import ideas based on the kinetic theory of hard discrete atoms into the totally distinct theory of mobile atoms in continuity with the medium surrounding them. Doubtless the molecules of a gas effuse through a small orifice more quickly as the temperature rises, but it is natural to suppose that a vortex ring would do the same as its energy increases. To make the objection valid, it is necessary to show that a vortex ring passing through a small tube, comparable with its own diameter, would pass through more slowly the greater its energy. It is not, however, necessarily the case that in every vortex aggregate the velocity decreases as the energy increases. The mathematical treatment of thin vortex filaments is comparatively easy, and little attention has been paid to other cases. Let us attempt to trace the life history as to translation velocity and energy of a vortex ring. We start with the energy large; the ring now has a very large aperture, and has a very thin filament. As the energy decreases the aperture becomes smaller, the filament thicker, and the velocity of translation greater. We can trace quantitatively the whole of this part of its history until the thickness of the ring has increased to about four times the diameter of the aperture, or perhaps a little further. Then the mathematical treatment employed fails us or becomes very laborious to apply. Till eighteen months ago, this was the only portion of its history we could trace. Then Professor M. J. M. Hill [1] published his beautiful discovery of the existence of a spherical vortex. This consists of a spherical mass of fluid in vortical motion and moving bodily through the surrounding fluid, precisely as if it were a rigid sphere. This enables us to catch a momentary glimpse as it were of our vortex ring some little time after it has passed out of our ken. The aperture has gone on contracting, the ring thickening, and altering the shape of its cross section in a manner whose exact details have not yet been calculated. At last we just catch sight of it again as the aperture closes up. We find the ring has changed into a spherical ball, with still further diminished energy and increased velocity. We then lose sight of it again, but it now lengthens

[1] 'On a Spherical Vortex,' *Phil. Trans*, 1894.

out, and towards the end of its course approximates to the form of a rod moving
parallel to its length through the fluid with energy and velocity which again can
be approximately determined. In this part of its life the velocity of translation
decreases with decrease of energy. I believe it will be found, when the theory is
completely worked out, that the spherical atom is the stage where this reversal of
property takes place.

Even in the ring state, however, the change of velocity with energy is very
small; much smaller, I think, than is generally recognised. When the energy
is increased to twenty times that of the spherical vortex, the velocity is only
diminished to two-thirds its previous value. If at ordinary temperatures, say 27° C.,
the vortex was in the spherical shape, then at 3,000° C. its velocity of translation
would only have been reduced to four-fifths its value at the lower temperature,
whilst the aperture of the ring would have a radius about 1·4 time that of the
sphere. At 2,000° C. the velocity would not differ by much more than one-
twentieth from its original value. In fact, near the spherical state the alteration
in velocity of translation is very slow. It is therefore possible, that if the atoms
of matter be vortex aggregates, the state in which we can experimentally test our
theory is just that in which the mathematical discussion fails us. Other modifica-
tions tend to diminish this change of velocity. I will refer here to three only. The
first is that of hollow vortices. We must not, however, postulate vacuous atoms
without any rotational core at all; for in this case we should probably lose the
essential property of permanence. The question has not been fully investigated,
but there can be little doubt but that by diminishing the energy of a completely
hollow vortex we can cause it to disappear. We can certainly create one in a per-
fect fluid. Secondly, J. J. Thomson has shown that if a molecule be composed of
linked filaments, the energy increases as the components move further apart. In
such a case an extra supply of energy goes to expanding the molecule, and less, if
any, to increasing the aperture. Lastly, a modification of the atomic motion to
which I shall refer later, and which seems called for to explain the magnetic rota-
tion of the plane of polarisation of light, will also tend to lessen the change of size,
and therefore change of velocity with change of energy, even if it does not reverse
the property.

If we pass on to consider how a vortex atom theory lends itself to the explana-
tion of physical and chemical properties of matter independently of what may be
called ether relations, we find that we owe almost all our knowledge on this point
to the work of Professor J. J. Thomson,[1] which obtained the Adams' Prize in
1882. This, however, is confined to the treatment of *thin* vortex rings, still leaving
a wide field for future investigation in connection with thick rings and with vortex
aggregates which produce no cyclosis in the surrounding medium. His work is an
extremely suggestive one. He shows that such a theory is capable not only of
explaining the gaseous laws of a so-called perfect gas, but possibly also the slight
deviations therefrom. Quite as striking is his explanation of chemical combina-
tion—an explanation which flows quite naturally from the theory. A vortex fila-
ment can be linked on itself: two or more can be linked together, like helices
drawn on an anchor ring; or, lastly, several can be arranged together like parallel
rings successively threading one another. In the latter case, for such an arrange-
ment to be permanent, the strengths of each ring must be the same, and further,
not more than six can thus be combined together. The linked vortices will be in
permanent combination on account of their linkedness; the other arrangement may
be permanent if subject to no external actions. If, however, they are disturbed by
the presence of other vortices they may break up. When atoms are thus combined
to form a compound, a certain number of molecules will always be dissociated;
the compound will be permanent when the ratio of the average paired time to the
unpaired time of any atom is large. Thomson considers every filament to be of the
same strength. Then an atom consisting of two links will behave like a ring of
twice the strength, one of three links, of three times the strength, and so on. On
this theory chemical compounds are to be regarded as systems of rings, not linked

'A Treatise on the Motion of Vortex Rings.' Macmillan, 1883.

into one another but close together, and all engaged in the operation of threading each other. The conditions for permanence are : (1st) the strength of each ring must be the same, (2nd) the number must be less than 6. Now apply this. H and Cl have equal linkings, therefore equal strength. Consequently we can have molecules of HCl, or any combinations up to 6 atoms per molecule, although the simpler one is the most likely. O has twice the linking, therefore the strength double. Hence one of H and one of O cannot revolve in permanent connection. We require first to arrange two of H together to form one system. This system has the same strength as O, and can therefore revolve in permanent connection, and we get the water molecule. Or we may take two of the O atoms and one of the double H molecule, and they can form a triple system of three rings threading one another in permanent connection, and we get the molecule $H_2O_2$. This short example will be sufficient to indicate how the theory gives a complete account of valency.

The energy of rings thus combined is less than when free ; consequently they are stable, and the act of combination sets free energy. Further, Thomson points out that for two rings to combine their sizes must be about the same when they come into proximity ; consequently combination can only occur between two limits of temperature corresponding to the energies within which the radii of both kinds of rings are near an equality.

We can easily extend Thomson's reasoning to explain the combination of two elements by the presence of a third neutral substance. Call the two elements which are to combine A and B, and the neutral substance C. The radii of A and B are to be supposed too unequal to allow them to come close enough together to combine. If now at the given temperature the C atom has a radius intermediate to those of A and B, it is more nearly equal to each than they are to one another ; C picks up one of A, and after a short time drops it ; A will leave C with its radius brought up (say) to closer equality with it. The same thing happens with the B atoms, and they leave C with their radii brought down to closer equality with it. The result is that A and B are brought into closer equality with one another, and if this is of sufficient amount, they can combine and do so, while C remains as before and apparently inert.

Thomson's theory of chemical combination applies only to thin rings. Something analogous may hold also for thick rings, but it is clearly inapplicable to vortex aggregates similar to that of Hill's. We are not confined, however, to this particular kind of association of vortex atoms in a molecule. For instance, I have recently found[1] that one of Hill's vortices can swallow up another and retain it inside in relative equilibrium. The matter requires fuller discussion, but it seems to open up another mode of chemical combination

A most important matter which has not yet been discussed at all is the relation between the mean energy of the vortex cores, and the energy of the medium itself when the atoms are close enough to affect each other's motions (as in a gas). The fundamental ideas are quite different from those underlying the well-known kinetic theory of gases of hard atoms. Nevertheless, many of the results must be very similar, based as both are on dynamical ideas  Whether it will avoid certain difficulties of the latter, especially those connected with the ratio of the specific heats, remain to be seen. The first desideratum is the determination of the equilibrium of energy between vortices and medium, and before this is done it is useless to speculate further in this region.

A vortex atom theory of matter carries with it the necessity of a fluid ether. If such a fluid is to transmit transversal radiations, some kind of quasi-elasticity must be produced in it. This can be done by supposing it to possess energetic rotational motions whose mean velocity is zero, within a volume whose linear dimension is small compared with the wave length of light, but whose velocity of mean square is considerable. That an ether thus constituted is capable of transmitting transverse vibrations I showed before this Section at the Aberdeen meeting of the Association,[2] by considering a medium composed of closely packed discrete

[1] Not yet published.
[2] 'On the Constitution of the Luminiferous Ether on the Vortex Atom Theory,' *Brit Assoc. Reports*, 1885, p. 930.

small vortex rings. Lord Kelvin[1] at the Manchester meeting discussed the question much more thoroughly and satisfactorily, and deduced that the velocity of propagation was $\sqrt{2/3}$ times the velocity of mean square of the turbulent motion. We can make little further progress until we know something of the arrangement of the small motions which confer the quasi-rigidity. This may be completely irregular and unsteady, or arranged in some definite order of steady motions. I am inclined to the view that the latter is nearer the truth. In this case we should expect a regular structure of small cells in which the motions are all similar. By the word cell I do not mean a small vessel bounded by walls, but a portion of the fluid in which the motion is a complete system in itself. Such a theory might be called a cell theory of the ether. The simplest type perhaps is to suppose the medium spaced into rectangular boxes, in each of which the motion may be specified as follows. Holding the box with one set of faces horizontal the fluid streams up in the centre of the box, then turns round, flows down the sides and up the centre again. In fact it behaves like a Hill's vortex squeezed from a spherical into a box form. Each box has thus rotational circulation complete in itself. The six adjoining compartments have their motion the same in kind but in the reverse direction, and so on. In this way we get continuous and energetic small motions throughout the medium, and the state is a stable one. If there is a shear, so that each cell becomes slightly rhomboidal, the rotational motions inside tend to prevent it, and thus propagate the disturbance, but the cells produce no effect on the general irrotational motion of the fluid, at least when the irrotational velocities are small compared with those of the propagation of light. In this case the rate at which the cells adjust themselves to an equilibrium position is far quicker than the rate at which this equilibrium distribution is disturbed by the gross motions. The linear dimensions of the cells must be small compared with the wave lengths of light. They must probably be small also compared with the atoms of gross matter, which are themselves small compared with the same standard.

We may regard each cell as a dynamical system by itself, into which we pour or take away energy. This added energy will depend only on the shape into which the box is deformed. We may then, for our convenience in considering the gross motions of the medium as a whole, *i.e.* our secondary medium, regard these as interlocked systems, neglect the direct consideration of the motions inside them, but regard the energy which they absorb as a potential function for the general motion. This potential function will contain terms of two kinds, one involving the shear of the cells, and this shear will be the *same* as the rotational deformation in the secondary medium. The second will depend on alterations in the ratios of the edges of the cells.[2] The former will give rise to waves of transversal displacements. The second cannot be transmitted as waves, but may produce local effects.

If a continuous solid be placed in such a medium, the cells will rearrange themselves so as to keep the continuity of their motions. The cells will become distorted (but without resultant shear), and a static stress will be set up. We have then to deal with the primary stuff itself, whose rotation gives a structure to the ether, and the structural ether itself. The former we may call the primary medium. The ether which can transmit transversal disturbances, and which is built up out of the first, we may call the secondary medium. Whether an atom of matter is to be considered as a vortical mass of the primary or of the secondary medium is a matter to be left open in the present state of the theory.

At the Bath meeting of this Association, I sketched out a theory of the electrical action of a fluid ether in which electrical lines of force were vortex filaments combined with an equivalent number of hollow vortices of the same vortical strength.[3] An electric charge on a body depended on the number of ends of filaments abutting on it, the sign being determined by the direction of rotation of the filament looked at from the body. This theory gave a complete account of

[1] 'On the Vortex Theory of the Luminiferous Ether,' *Brit. Assoc. Reports,* 1887, p. 486, also *Phil Mag*, October 1887, p. 342

[2] Including other changes of form involving no rotations.

[3] 'A Vortex Analogue of Static Electricity,' *Brit. Assoc Rep*, 1888, p. 577.

electrostatic actions, both quantitatively and qualitatively, and a more speculative one as to currents and magnetism. I could only succeed in proving at that time that if the filaments were distributed according to the same laws as electric lines of force, the distribution would be one of equilibrium. Larmor [1] has recently proved that this is also the necessary distribution for any type of a rotationally elastic ether, and consequently also for this particular case. Currents along a wire were supposed to consist of the ends of filaments running along it, with disappearance of the hollow companions, the filaments producing at the same time a circulation round the wire. A magnetic field was thus to be produced by a flow of the ether, but probably with the necessary accompaniment of rotational elements in it.

This latter, however, was clearly wrong, because each kind of filament would produce a circulation in opposite directions. The correct deduction would have been to lay stress on the fact that the field is due to the motion through the stationary ether of the vortex filaments, the field being perpendicular to the filament and to its direction of motion. This motion would doubtless produce stresses in the cell-ether due to deformations of the cells, and be the proximate cause of the mechanical forces in the field. In any case, it is not difficult to show that a magnetic field cannot be due to an irrotational flow of the ether alone.[2] Such electrostatic and magnetic fields produce states of motion in the medium, but no bodily flow in it; consequently we ought not to expect an effect to be produced on the velocity of transmission of light through it

The fundamental postulate underlying this explanation of electric action is that when two different kinds of matter are brought into contact a distribution of vortex filaments in the neighbourhood takes place, so that a larger number stretch from one to the other than in the opposite direction—the distinction between positive and negative ends being that already indicated. To see how such a distribution may be caused, let us consider each vortex atom to be composed of a vortical mass of our secondary medium or cell-structure ether. The atom is much larger than a cell, and contains practically an infinite number of them. It is a dynamical system of these cells with equilibrium of energy throughout its volume. The second atom is a dynamical system with a different equilibrium of energy. Where they come into contact there will be a certain surface rearrangement, which will show itself as a surface distribution of energy in a similar manner to that which exists between a molar collection of one kind of molecules in contact with one of another, and which shows itself in the phenomenon which we call surface tension. In the present case the effect may take place at the interface of two atomic systems in actual contact, or be a difference effect between the two interfaces of the ether and each atom when the latter are sufficiently close. The surface effect we are now considering shows itself as contact electricity.

Such a distribution of small vortex filaments, stretching from one atom to another, will tend to hold them together. We therefore get an additional cause for aggregation of atoms. This does not exclude the others already referred to. They may all act concurrently, some producing one effect, some another—one combining, perhaps, unknown primitive atoms into elements, one elements into chemical compounds, and another producing the cohesion of matter into masses.

On this theory the difference between a conductor and a dielectric is that in a dielectric the ends of the filaments cannot pass from atom to atom, possibly

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[1] 'A Dynamical Theory of the Electric and Luminiferous Medium,' *Phil Trans*, 1894, p. 748.

[2] To prove this, consider a straight conductor moving parallel to itself and perpendicular to a uniform magnetic field. There exists a permanent potential difference between its ends. If, however, the field consists of a flow of ether, the effect is the same as if the conductor is at rest, and the direction of the magnetic field shifted through an angle. But this is the case of a conductor at rest in a field, and there is therefore no potential difference between the ends. Hence a magnetic field must consist of some structure across which the conductor cuts. A field may possibly demand a flow of the ether, but, if so, it must carry in it some structure definitely oriented at each point to the direction of flow.

because the latter never come into actual contact. In a conductor, however, we are to suppose that the atomic elements can do so. When a current is flowing, a filament and its equivalent hollow stretch between two neighbouring atoms, they are pulled into contact, or their motions bring them into contact, the hollow disappears, and the rotational filament joins its two ends and sails away as a small neutral vortex ring into the surrounding medium, or returns to its function as an ether cell. The atoms being free are now pulled back to perform a similar operation for other filaments. The result is that the atoms are set into violent vibrations, causing the heating of the conductor. When, however, the metal is at absolute zero of temperature, there is no motion, the atoms are already in contact, and there is no resistance, as the observation of Dewar and Fleming tends to show. Further, as the resistance depends on the communication of motion from molecule to molecule, we should expect the electrical conductivity of a substance to march with its thermal conductivity. Again, on this theory the resistance clearly increases with increase of distance between atoms—*i e*, with increase of temperature. On the contrary, in electrolytic conduction the same junction of filament ends is brought about, not by oscillations of molecule to molecule, but by disruption of the molecule and passage of atom to atom. In this case conduction is easier the more easily a molecule is split up, and thus resistance decreases with increase of temperature. To explain the laws of electrolysis it is only necessary to assume that the strengths of all filaments are the same. A similar hypothesis, as we have seen, lies at the basis of J. J. Thomson's explanation of chemical combination, although it is not necessarily the case that we are dealing with the same kind of filaments. It is evident that the theory easily lends itself to his views as to the mechanism of the electric discharge through gases. The *modus operandi* of the production of the mechanical forcive on a conductor carrying a current in a magnetic field and of electrodynamic induction is not clear. Probably the full explanation is to be found in the stresses produced in the ether owing to the deformation of the cells by the passage of the filaments through them. The fluid moves according to the equation of continuity without slip, and subject to the surface conditions at the conductors. This motion, however, distorts the cells, and stresses are called into play. Any theory which can explain the mechanical forcives and also Ohm's law, must, on the principles of the conservation of energy, also explain the induction of currents.

The magnetic rotation of the plane of polarisation of light does not depend on the structure of the ether, or on the magnetic field itself, but is a result of the atomic configuration of the matter in the field modified by the magnetism. It is generally recognised as caused by something in the field rotating round the direction of the magnetic lines of force. Now the vortex atom, as usually pictured, is incapable of exhibiting this property. It is, however, an interesting fact, and one which I hope to demonstrate to this Section during the meeting, that a vortex ring can have two simultaneous and independent cyclic motions—one the ordinary one, and another which is capable of producing just the action on light which shows itself as a rotation of the plane of polarisation. The motion is rather a complicated one to describe without a diagram, but an idea of its nature may be obtained by considering the case of a straight cylindrical vortex. The ordinary straight vortex consists, as every one knows, of a cylinder of fluid revolving like a solid, and surrounded by a fluid in irrotational motion. In the core the velocity increases from zero at the axis to a maximum at its surface. Thence it continuously decreases in the outer fluid as the distance increases. Everywhere the motion is in a plane perpendicular to the axis  Let us now consider a quite different kind of vortical motion. Suppose the fluid is flowing along the core like a viscous fluid through a pipe; the velocity is zero at the surface and a maximum at the axis. Everywhere it is parallel to the axis, the vortex lines are circles in planes perpendicular to the axis, and concentric with it  Since the velocity at the surface of the core is zero, the surrounding fluid is also at rest. Now superpose this motion on the previous one, and it will be found to be steady. If a short length of this vortex be supposed cut off, bent into the shape of a circle and the ends joined, we shall have a very rough idea of the compound vortex ring of which I speak. I

say a very rough idea, because the actual state of motion in a ring vortex or a Hill's vortex is not quite so simple as the analogy might lead one to think.

Now a compound vortex atom of this kind is just what we want to produce rotation of the plane of polarisation of light. The light passing through such a vortex has the direction of vibration twisted in the wave front. In ordinary matter no such rotation is produced, because the various atoms are indifferently directed, and they neutralise each other's effects. Let, however, a magnetic field be produced, and they will range themselves so that, on the average, the primary[1] circulations through the apertures will point in the direction of the field. Consequently the average direction of the secondary spin will be in planes perpendicular to this, and will rotate the plane of polarisation of any light whose wave front passes them. The rotation is produced only on the light which is transmitted *through* the vortex. The rotation observed is a resultant effect. In fact it is clear that in the case of refraction the optical media belong to the type in which every portion transmits the light, and not to the type in which refraction is produced by opaque bodies embedded in the ether. The atoms are only opaque if they contain vacuous cores. The question of the grip of the particles on the ether does not enter, but difference of quality—showing itself in refraction and dispersion—is due to difference in average rotational quasi-elasticity produced by the atomic circulations, and possibly absorption is due to precessional and nutational motion set up by the secondary spins. These, however, are perhaps rather vague speculations.

Instead of attempting to invent ethers, to deduce their properties from their specifications, and then seeing whether they fit in with experience, we may begin half way. We may assume different forms for the function, giving the energy of the medium when disturbed, apply general dynamical methods, and distinguish between those which are capable of explaining the phenomena we are investigating and those which are not. Invention is then called upon to devise a medium for which the desired energy-function is appropriate. This was the method applied by MacCullagh to the luminiferous ether. He obtained an algebraical form of the energy function which completely satisfied the conditions for a luminiferous ether; its essential property being that the energy depended only on the rotational displacements of its small parts. He was unable, however, to picture a stable material medium which would possess this property. We recognise now that such a medium is possible if the rotational rigidity is produced by intrinsic motions in the small parts of the medium of a gyrostatic nature. In a most masterly manner Larmor[2] has recently investigated by general dynamical methods the possibility of explaining electric and magnetic phenomena by means of the same energy function. Electric lines of force are rotational filaments in the ether,[3] similar in fact to those I suggested at Bath, whilst a magnetic field consists of a flow of the ether. The same difficulty in accounting for electro-dynamic induction arises, but the general form of the equations for the electro-dynamic and magnetic fields are the same as those generally received.

Towards the end of this paper he is led to postulate a theory of electrons whose convection through the ether constitutes an electric current. Two rotating round each other are supposed to produce the same effect as a vortex ring. The mass of ordinary matter is attributed to the electric inertia of these electrons. The electron itself is a centre or nucleus of rotational strain. If I express a doubt as to the possibility of the existence of these nuclei as specified, I do so with great diffidence.[4]

[1] 'Primary' refers to the motion as usually understood; 'secondary,' to the superposed, as explained above

[2] 'A Dynamical Theory of the Electric and Luminiferous Medium,' *Phil. Trans*, 1894.

[3] The necessity that the filaments shall be in pairs does not seem to be recognised. This is, however, essential Moreover, if the complementary circulations of the filaments between (say) a plate condenser be placed otherwhere than in the same region, the filaments between the plates must rotate as a whole; that is, an electric field would always be combined with a magnetic one.

[4] It would appear that the same results would flow if two particles oppositely electrified—*i e.* joined by two complementary filaments, as already described—were to rotate round each other.

Whether they can or cannot exist, however, the general results of the investigation are not affected.

Since this paper was published Larmor has read a second one on the same subject before the Royal Society, developing further his theory of the electron. The publication of this will be awaited with interest. It is impossible in an address such as this to go *seriatim* into the numerous points which he takes up and illuminates, because the mathematical treatment of the general question does not lend itself easily to oral exposition even to an audience composed of professed mathematicians. There is no doubt but that this paper has put the theory of a rotationally elastic ether—and with it that of a fluid vortex ether—on a sounder basis, and will lead to its discussion and elucidation by a wider circle of investigators.

One further class of physical phenomena yet remains, viz., those of gravitation. The ether must be capable of transmitting gravitational forces as well as electric and optical effects. Does the rotational ether give any promise of doing this? No satisfactory explanation of gravitation on any theory has yet been offered. Perhaps the least unsatisfactory is that depending on the vortex atom theory of matter,[1] which attributes it to pulsations of hollow vortex atoms. But this necessitates that they should all pulsate with the same period and in the same phase. It is very difficult to conceive how this can happen, unless, as Larmor suggests, all matter is built up of constant elements like his electrons, whose periods are necessarily all alike. It is possible that the vortex cell theory of the ether, of which I have already spoken, may suffice to explain gravitation also. The cells, besides their rotational rigidity, have, in addition, as we saw, a peculiar elasticity of form. To get an idea of how this theory may account for weight, let us suppose the simplest case where all the cells are exactly alike, and the medium is in equilibrium. Now suppose one of the cells begins to grow. It forces the medium away on all sides, the cells will be distorted in some definite way, and a strain set up. Further, this strain will be transmitted from the centre, so that the total amount across any concentric sphere will be the same. Stresses will therefore be set up in the whole medium. If a second cell begins to grow at another place it will produce also a state of strain, the total strain depending on the presence of both. The stresses called into play in the medium will produce a stress between the bodies, but it is questionable whether it would be inversely as the square of the distance. Whether it would be an attraction or repulsion can only be determined by mathematical investigation. The problem is quite determinate, though probably a very difficult one, and would be of mathematical interest quite apart from its physical importance. Since apparently the phenomena of gravitation have no direct interaction with those of light and electricity, whilst the mind rejects the possibility of two different media occupying the same space, we seem driven to look for it in an independent structure of the same medium. Such a structure is already to our hands, with its effects waiting to be determined. It may well be that it may prove to be the cause we are seeking.

The rapid survey I have attempted to make is no doubt a medley of suppositions and inferences combined with some sound deductions. This is the necessary consequence of a prospecting survey in a region whose surface has been merely scratched by pioneers. My object has been to show that this theory of an ether, based on a primitive perfect fluid, is one which shows very promising signs of being able to explain the various physical phenomena of our material universe. Probably, nay certainly, the explanations suggested are not all the true ones. Some will have to be given up, others modified with further knowledge. We cannot proceed to particularise in our secondary hypotheses until we know more about the properties of such media as we have been considering. Every special problem solved in vortex motion puts us in a position to form clearer ideas of what can and what cannot happen. The whole question of vortex aggregates and their interactions is

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[1] 'On the Problem of Two Pulsating Spheres in a Fluid,' *Proc Camb. Phil. Soc.*, iii. p. 283.

practically untouched, and a rich field is open for mathematical investigation in this portion only of the subject. In all cases, whether a fluid ether is an actual fact or not, the results obtained will be of special interest as types of fluid motion. It is at present a subject in which the mathematicians must lead the attack. I shall have attained my object in choosing this subject for my address, if by it I can induce some of our younger mathematicians to take it up, and work out its details.

# British Association for the Advancement of Science.

## IPSWICH, 1895.

# ADDRESS

TO THE

# CHEMICAL SECTION

BY

Professor RAPHAEL MELDOLA, F.R.S., F.I.C., For. Sec. C.S.

PRESIDENT OF THE SECTION

### The State of Chemical Science in 1851.

In order to estimate the progress of chemical science since the year 1851, when the British Association last met in this town, it will be of interest for us to endeavour to place ourselves in the position of those who took part in the proceedings of Section B on that occasion. Perhaps the best way of performing this retrograde feat will be to confront the fundamental doctrines of modern chemistry with the state of chemical theory at that period, because at any point in the history of a science the theoretical conceptions in vogue—whether these conceptions have survived to the present time or not—may be taken as the abstract summation of the facts, $i.e.$, of the real and tangible knowledge existing at the period chosen as the standard of reference.

Without going too far back in time I may remind you that in 1811 the atomic theory of the chemists was grafted on to the kindred science of physics through the enunciation of the law associated with the name of Avogadro di Quaregna. The rationalising of this law had been accomplished in 1845, but the kinetic theory of gases, which had been foreshadowed by D. Bernoulli in 1738, and in later times by Herapath, Joule, and Krönig, lay buried in the archives of the Royal Society until recently unearthed by Lord Rayleigh and given to the world in 1892 under the authorship of Waterston, the legitimate discoverer. The later developments of this theory did not take place till after the last Ipswich meeting, viz., in 1857–1862, by Clausius, and by Clerk Maxwell in 1860–1867. Thus the kinetic theory of gases of the physicists had not in 1851 acquired the full significance for chemists which it now possesses: the hypothesis of Avogadro was available, analogous conceptions had been advanced by Davy in 1812, and by Ampère in 1814, but no substantial chemical reasons for its adoption were adduced until the year 1846, when Laurent published his work on the law of even numbers of atoms and the nature of the elements in the free state.[1]

The so-called 'New Chemistry' with which students of the present time are familiar was, in fact, being evolved about the period when the British Association last assembled at Ipswich, but it was not till some years later, and then chiefly through the writings of Laurent and Gerhardt, that the modern views became accepted. It is of interest to note in passing that the nomenclature of organic compounds formed the subject of a report by Dr. Daubeny at that meeting in which he says:—' It has struck me as a matter of surprise that none of the British treatises on Chemistry with which I am acquainted should contain any rules to guide us, either in affixing names to substances newly discovered or

[1] *Ann. Chim. Phys.* [3] 18 266

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in divining the nature and relations of bodies from the appellations attached to them. Nor do I find this deficiency supplied in a manner which to me appears satisfactory when I turn to the writings of Continental chemists' In a subsequent portion of the report Dr. Daubeny adds:—' No name ought, for the sake of convenience, to exceed in length six or seven syllables.' I am afraid the requirements of modern organic chemistry have not enabled us to comply with this condition.

Among other physical discoveries which have exerted an important influence on chemical theory the law of Dulong and Petit, indicating the relationship between specific heat and atomic weight, had been announced in 1819, had been subsequently extended to compounds by Neumann, and still later had been placed upon a sure basis by the classical researches of Regnault in 1839. But here, again, it was not till after 1851 that Cannizzaro (1858) gave this law the importance which it now possesses in connection with the determination of atomic weights. Thermo-chemistry as a distinct branch of our science may also be considered to have arisen since 1851, although the foundations were laid before this period by the work of Favre and Silbermann, Andrews, Graham, and especially Hess, whose important generalisation was announced in 1840, and whose claim to just recognition in the history of physical chemistry has been ably advocated in recent times by Ostwald. But the elaboration of thermo-chemical facts and views in the light of the dynamical theory of heat was first commenced in 1853 by Julius Thomsen, and has since been carried on concurrently with the work of Berthelot in the same field which the latter investigator entered in 1865. Electro-chemistry in 1851 was in an equally rudimentary condition. Davy had published his electro-chemical theory in 1807, and in 1812 Berzelius had put forward those views on electric affinity which became the basis of his dualistic system of formulation. In 1833 Faraday announced his famous law of electro-chemical equivalence, which gave a fatal blow to the conception of Berzelius, and which later (1839-1840) was made use of by Daniell in order to show the untenability of the dualistic system. By 1851 the views of Berzelius had been abandoned, and, so far as chemical theory is concerned, the whole subject may be considered to have been in abeyance at that time. It is of interest to note, however, that in that year Williamson advanced on quite distinct grounds his now well-known theory of atomic interchange between molecules, which theory in a more extended form was developed independently from the physical side and applied to electrolytes by Clausius in 1857. The modern theory of electrolysis associated with the names of Arrhenius, van 't Hoff, and Ostwald is of comparatively recent growth. It appears that Hittorf in 1878 was the first to point out the relationship between electrolytic conductivity and chemical activity, this same author as far back as 1856 having combated the prevailing view that the electric current during electrolysis does the work of overcoming the affinities of the ions. Arrhenius formulated his theory of electrolytic dissociation in 1887, Planck having almost simultaneously arrived at similar views on other grounds.

Closely connected with electrolysis is the question of the constitution of solutions, and here again a convergence of work from several distinct fields has led to the creation of a new branch of physical chemistry which may be considered a modern growth. The relationship between the strength of a solution and its freezing point had been discovered by Blagden towards the end of the last century, but in 1851 chemists had no notion that this observation would have any influence on the future development of their science. Another decade elapsed before the law was rediscovered by Rudorff (1861), and ten years later was further elaborated by de Coppet. Raoult published his first work on the freezing point of solutions in 1882, and two years later the relationship between osmotic pressure and the lowering of freezing point was established by H. de Vries, who first approached the subject as a physiologist, through observations on the cell contents of living plants. As the work done in connection with osmotic pressure has had such an important influence on the ' dissociation ' theory of solutions, it will be of interest to note that at the last Ipswich meeting Thomas Graham made

a communication on liquid diffusion, in which he gave a view of some of the unpublished results, to ascertain whether solutions of saline bodies had a power of diffusion among liquids, especially water.' In 1877 Pfeffer, who, like de Vries, entered the field from the botanical physiological side, succeeded in effecting the measurement of osmotic pressure. Ten years later van 't Hoff formulated the modern dissociation theory of solution by applying to dissolved substances the laws of Boyle, Gay-Lussac, and Avogadro, the law of osmotic pressure, and Raoult's law connecting the depression of freezing point with molecular weight, thus laying the foundation of a doctrine which, whether destined to survive in its present form or not, has certainly exerted a great influence on contemporary chemical thought.

Consider, further, the state of knowledge in 1851 concerning such leading principles as dissociation or thermolysis, mass action, and chemical equilibrium. Abnormal vapour densities had been observed by Avogadro in 1811, and by Ampère in 1814. Grove had dissociated water vapour by heat in 1847, but the first great advance was made ten years later by Sainte-Claire Deville, from whose work has emanated our existing knowledge of this subject. I may add that the application of this principle to explain the cases of abnormal vapour density was made in 1858 by Kopp, Kekulé, and Cannizzaro almost simultaneously; but, strangely enough, this explanation was not accepted by Deville himself. The subsequent stages are subjects of modern history  The current views on mass action were foreshadowed, as is well known, by Berthollet in his 'Statique Chimique,' published in 1803, but no great advance had been made when the British Association last met here  The subject first began to assume a quantitative aspect through the researches of Bunsen and Debus in 1853, and was much advanced by Gladstone in 1865, and by Harcourt and Esson a year later. Guldberg and Waage published their classical work on this subject in 1867.

Equally striking will appear the advances made since 1851 if we consider that the whole subject of spectrum analysis, which brings our science into relationship with astronomy, has been called into existence since that date. The celebrated work of Bunsen and Kirchhoff was not published till 1859. Neither can I refrain from reminding you that the coal-tar colour industry, with which I have been to a small extent connected, was started into activity by Perkin's discovery of mauve in 1856 ; the reaction of this industry on the development of organic chemistry is now too well known to require further mention.  In that direction also which brings chemistry into relationship with biology the progress has been so great that it is not going beyond the fact to state that a new science has been created.  Pasteur began his studies on fermentation in 1857, and out of that work has arisen the science of bacteriology, with its multifarious and far-reaching consequences.  As this chapter of chemical history forms the subject of one of the evening discourses at the present meeting it is unnecessary to dwell further upon it now.  One other generalisation may be chronicled among the great developments achieved since 1851.  I refer to the periodic law connecting the atomic weights of the chemical elements with their physical and chemical properties.  Attempts to establish numerical relationships in the case of isolated groups of elements had been made by Döbereiner in 1817, by Gmelin in 1826, and again by Döbereiner in 1829  The triad system of grouping was further developed by Dumas in 1851.  I am informed by Dr Gladstone that at the last Ipswich meeting Dumas' speculations in this direction excited much interest. All the later steps of importance have, however, been made since that time, viz., by de Chancourtois in 1862, the 'law of octaves' by Newlands in 1864, the periodic law by Mendeléeff, and almost contemporaneously by Lothar Meyer in 1869

I have been tempted into giving this necessarily fragmentary and possibly tedious historical sketch because it is approaching half a century since the British Association visited this town, and the opportunity seemed favourable for going through that process which in commercial affairs is called 'taking stock.'  The result speaks for itself.  Our students of the present time who are nourished intellectually by these doctrines should be made to realise how rapid has been

their development.  The pioneers of our science on whose shoulders we stand—and many of whom are happily still among us—will derive satisfaction from the retrospect, and will admit that their labours have borne goodly fruit  It is not, however, simply for the purpose of recording this enormous progress that I have ventured to assume the office of stock-taker.  The year 1851 may be regarded as occurring towards the close of one epoch and the dawn of a new era in chemical history.  Consider broadly the state of organic chemistry at that time.  There is no occasion for going into detail, even if time admitted, because our literature has recently been enriched by the concise and excellent historical works of Schorlemmer and of Ernst von Meyer.  It will suffice to mention that the work and writings of Liebig, Berzelius, Wöhler, Dumas, Gay-Lussac, Bunsen, and others had given us the leading ideas of isomerism, substitution, compound radicles, and types.  Wurtz and Hofmann had just discovered the organic ammonias, Williamson that same year made known his celebrated work on the ethers, and Gerhardt discovered the acid anhydrides a year later.  The newer theory of types was undergoing development by Gerhardt and his followers; the mature results were published in the fourth volume of the 'Traité de Chimie' in 1856.  In this country the theory was much advanced by the writings of Odling and Williamson.

### SUBSEQUENT DEVELOPMENT OF CHEMISTRY ALONG TWO LINES.

The new era which was dawning upon us in 1851 was that of structural or constitutional chemistry, based on the doctrine of the valency of the atoms.  It is well known that this conception was broached by Frankland in 1852, as the result of his investigations on the organo-metallic compounds  But it was not till 1858 that Kekulé, who had previously done much to develop the theory of types, and Couper, almost simultaneously, recognised the quadrivalent character of carbon.  To attempt to give anything approaching an adequate notion of the subsequent influence of this idea on the progress of organic chemistry would be tantamount to reviewing the present condition of that subject.  I imagine that no conception more prolific of results has ever been introduced into any department of science.  If we glance back along the stream it will be seen that shortly after the last meeting here the course of discovery began to concentrate itself into two channels.  In one we now find the results of the confluent labours of those who have regarded our science from its physical side.  In the other channel is flowing the tide of discovery arising from the valency doctrine and its extension to the structure of chemical molecules.  The two channels are at present fairly parallel and not far apart; an occasional explorer endeavours now and again to make a cross-cut so as to put the streams into communication.  The currents in both are running very rapidly, and the worker who has embarked on one or the other finds himself hurried along at such a pace that there is hardly breathing time to step ashore and see what his neighbours are doing.  It speaks well for the fertility of the conception of valency that the current in this channel is flowing with unabated vigour, although its catchment area—to pursue the metaphor—is by no means so extensive as that of the neighbouring stream.

The modern tendency to specialisation, which is a necessity arising from the large number of workers and the rapid multiplication of results, is apparently in the two directions indicated.  We have one class of workers dealing with the physics of matter in relation to general chemical properties, and another class of investigators concerning themselves with the special properties of individual compounds and classes of compounds—with atomic idiosyncrasies.  The workers of one class are differentiating while their colleagues are integrating.  It would be nothing less than unscientific to institute a comparison between the relative merits of the two methods; both are necessary for the development of our science.  All methods of attacking the unknown are equally welcomed  In some cases physical methods are available, in other cases purely chemical methods have alone been found of use.  There is no antagonism, but co-operation.  If the results of the two methods are sometimes at variance it is simply because we have not known how to interpret them.  The physical chemist has adopted the results of the application of chemical

methods of determining 'constitution,' and is endeavouring to furnish us with new
weapons for attacking this same problem.   The chemist who is seeking to unravel
the architecture of molecules is dependent at the outset upon physical methods of
determining the relative weights of his molecules.   The worker who is bringing
about new atomic groupings is furnishing material for the further development of
generalisations from which new methods applicable to the problem of chemical
structure may again be evolved.   The physical chemist sometimes from the broad-
ness of his view is apt to overlook or to minimise the importance of chemical
individuality   On the other hand the chemist who is studying the numberless
potentialities of combination resident in the atoms, and who has grasped to the full
extent their marvellous individualities, is equally liable to forget that there are
connecting relationships as well as specific differences in the properties of elements
and compounds.   These are but the mental traits—the unconscious bias engendered
by the necessary specialisation of work to which I have referred, and which is
observable in every department of scientific labour.

### THE PRESENT STATE OF STRUCTURAL CHEMISTRY.

The success attending the application of the doctrine of valency to the com-
pounds of carbon has helped its extension to all compounds formed by other
elements, and the student of the present day is taught to use structural formulæ as
the A B C of his science.   It is, I think, generally recognised among chemists that
this doctrine in its present state is empirical, but it does not appear to me that this
point is sufficiently insisted upon in chemical teaching.   I do not mean to assert
that for the last thirty years chemists have been pursuing a phantom , neither do
I think that we should be justified in applying to this doctrine the words applied
to its forerunner, the 'types' of Gerhardt, by Lothar Meyer, who says that these
'have rendered great service in the development of the science, but they can only
be regarded as a part of the scaffolding which was removed when the erection of
the system of organic chemistry had made sufficient progress to be able to dispense
with it.'[1]   It appears to me, on the contrary, that there is a physical reality under-
lying the conception of valency, if for no other reason because of the conform-
ability of this property of the atoms to the periodic law.   But the doctrine as it
stands is empirical in so far that it is only representative and not explanatory.
Frankland and Kekulé have given us a great truth, but its very success is now
making it more and more obvious that it is a truth which is pressing for further
development from the physical side.   If we are asked why CO exists, and why
$CH_2$ and $CCl_2$ do not, together with innumerable similar questions which the in-
quisitive mind will raise, we get no light from this doctrine.   If any over-sanguine
disciple goes so far as to assert that all the possible compounds of the elements indi-
cated by their valency are capable of existence, and will sooner or later be prepared,
he will, I imagine, find himself rapidly travelling away from the region of fact.

There is something to be reckoned with besides valency.   The one great desi-
deratum of modern chemistry is unquestionably a physical or mechanical interpre-
tation of the combining capacities of the atoms.   Attempts at the construction of
such theories have been made, but thus far only in a tentative way, and these
views cannot be said to have yet come within the domain of practical chemical
politics   I have in mind, among other suggestions, the dynamical theory of van
't Hoff published in 1881,[2] the theory of electric charges on the atoms broached
by Johnstone Stoney in 1874, and so ably advocated by the late Professor v.
Helmholtz in his Faraday lecture in 1881, and the electric polar theory of Victor
Meyer and Riecke, published in 1888.[3]

Pending the rationalisation of the doctrine of valency its promulgation must
continue in its present form.   Its services in the construction of rational formulæ,

[1] *Modern Theories of Chemistry*, p 194.

[2] *Ansichten uber die organische Chemie.*

[3] 'Einige Bemerkungen uber den Kohlenstoffatom und die Valenz,' *Ber.*, 21,
pp 946, 1620

especially within the limits of isomerism, have been incalculable. It is the ladder by which we have climbed to the present brilliant achievements in chemical synthesis, and we are not in a position to perform the ungracious task of kicking it away. In recalling attention to its weaknesses I am only putting myself in the position of the physician who diagnoses his patient's case with the ulterior object of getting him strengthened. There can be no doubt that renewed vitality has been given to the doctrine by the conceptions of tautomerism and desmotropy, formulated by Conrad Laar in 1885, and by Paul Jacobson in 1887. The import-ance of these ideas is becoming more evident with the advancement of chemical discovery. Any attempt to break down the rigidly statical conception of our structural formulæ appears to me to be a step in the right direction. Then, again, I will remind you of the prolific development of the doctrine in the hands of Le Bel and van 't Hoff by the introduction of the stereochemical hypothesis in 1874—un-questionably the greatest advance in structural chemistry since the recognition of the quadrivalent character of the carbon atom. If evidence be required that there is a physical reality underlying the conception of valency, we need only point to the close accordance of this notion of the asymmetric carbon atom with the facts of so-called 'physical isomerism' and the splendid results that have followed from its introduction into our science, especially in the field of the carbohydrates through the investigations of Emil Fischer and his pupils. In other directions the stereo-chemical hypothesis has proved to be a most suggestive guide. It was applied by Professor v. Baeyer in 1885 [1] to explain the conditions of stability or instability of certain atomic groupings, such as the explosiveness of polyacetylene compounds and the stability of penta- and hexa-cyclic systems. Again, in 1888 this eminent chemist showed its fertility in a series of brilliant researches upon benzene deriva-tives. [2] Nor can I omit to mention the great impetus given in this field by the classical work of Wislicenus, who in 1887 applied the hypothesis to unsaturated compounds and to cyclic systems with remarkable success. [3] Quite recently Victor Meyer and J. Sudborough have shown that the ability of certain derivatives of benzoic and naphthoic acids to form ethers is governed by stereochemical consider-ations. [4] But I must avoid the temptation to enlarge upon this theme because the whole subject has been recently brought together by C. A. Bischoff in his ' Hand-buch der Stereochemie' (Frankfurt, 1893-94), a work to which all who are interested in the subject will naturally turn for reference.

While the present advanced state of structural chemistry may thus be looked upon as the outcome of the conceptions of Frankland and Kekulé, it may be well to bear in mind that the idea of structure is not *necessarily* bound up with the hypothesis of valency in its present form. Indeed, some advance had been made in representing ' constitution,' especially by Kolbe, before the formal introduction of this hypothesis. The two ideas have grown up together, but the experimental evidence that in any molecule the atoms are grouped together in a particular way is really independent of any theory of valency. It is only after this evidence has been acquired, either by analysis or synthesis, that we proceed to apply the hypo-thesis in building up the structural formula. It is of course legitimate to assume the truth of the hypothesis, and to endeavour by its use to convert an empirical into a rational formula ; but this method generally gives us a choice of formulæ from which the true one can only be selected by further experimental investigation. Even within the narrower limits of isomerism it is by no means certain that all the modifications of a compound indicated by hypothesis are actually capable of exist-ence. There is, for example, evidence that some of the ' position isomerides' among the derivatives of mono- and polycyclic compounds are too unstable to exist ; a fact which in itself is sufficient to indicate the necessity for a revision and extension of our notions of valency. Thus, by way of illustration, there is nothing in the hypothesis to indicate why orthoquinones of the benzene series should not be capable of existence ; yet it is a fact that in spite of all efforts such compounds

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[1] *Ber*, **18**, 2277　　　　[2] *Ann*, **137**, 158, and subsequent papers
[3] *Ueber die räumliche Anordnung der Atome in organischen Molekulen*, &c.
[4] *Ber*, **27**, 510, 1580, 3146, and **28**, 182, 1254.

have never been obtained. The conditions essential for the existence of these compounds appear to be that the hydrogen of the benzene ring should be replaced by acid substituents such as oxygen, hydroxyl, chlorine, or bromine. Under these circumstances, as Zincke has shown,[1] tetrachlor and tetrabrom-orthobenzoquinone are stable compounds. So also the interesting researches of Nietzki have proved that in such a compound as rhodizonic acid[2] orthoquinone oxygen atoms are present. But there is nothing in the doctrine of valency which leads us to suspect that these orthoquinone derivatives can exist while their parent compound resists all attempts at isolation. I am aware that it is dangerous to argue from negative evidence, and it would be rash to assert that these orthoquinones will never be obtained. But even in the present state of knowledge it may be distinctly affirmed that the methods which readily furnish an orthoquinone of naphthalene completely fail in the case of benzene, and it is just on such points as this that the inadequacy of the hypothesis becomes apparent. In other words, the doctrine fails in the fundamental requirement of a scientific theory; in its present form it gives us no power of prevision—it hints at possibilities of atomic groupings, but it does not tell us *à priori* which of these groupings are likely to be stable and which unstable. I am not without hope that the next great advance in the required direction may yet come from the stereochemical extension of the hypothesis, although the attempts which have hitherto been made to supply its deficiencies cannot but be regarded as more or less tentative.

### THE NEW THEORY OF ABSTRACT TYPES.

I will venture, in the next place, to direct attention to a modern development of structural chemistry which will help to illustrate still further some of the points raised. For many years we have been in the habit of abstracting from our structural formulæ certain ideal complexes of atoms which we consider to represent the nucleus or type from which the compound of known constitution is derived. In other words the hypothesis of valency which was developed originally from Gerhardt's types is now leading us back to another theory of types based upon a more intimate knowledge of atomic grouping within the molecule. In some cases these types have been shown to be capable of existence; in others they are still ideal. Used in this way the doctrine of valency is most suggestive, but at the same time its lack of prevision is constantly forcing itself upon the attention of chemical investigators. The parent compound has sometimes been known before its derivatives, as in the case of ammonia, which was known long before the organic amines and amides. In other instances the derivatives were obtained before the type was isolated, as in the case of the hydrazines, which were characterised by Emil Fischer in 1875, and the hydrazo-compounds, which have been known since 1863, while hydrazine itself was first obtained by Curtius in 1887. Phenylazimide was discovered by Griess in 1864, and many representatives of this group have been since prepared; but the parent compound, hydrazoic acid, was only isolated by Curtius in 1890. Derivatives of triazole and tetrazole were obtained by Bladin in 1885; the types were isolated by this chemist and by Andreocci in 1892. Pyrazole derivatives were prepared by Knorr in 1883; pyrazole itself was not isolated till 1889, by Buchner. Alkyl nitramides were discovered by Franchimont and Klobbie many years before the typical compound, nitramide, $NO_2.NH_2$, which was isolated last year by Thiele and Lachman.[3] Examples might be multiplied to a formidable extent, but enough have been given to illustrate the principle of the erection of types, which were at first imaginary, but which have since become real. The utility of the hypothesis is undeniable in these cases, and we are justified in pushing it to its extreme limits. But no chemist, even if endowed with prophetic instinct, could have certainly foretold six years ago that the type of Griess' 'triazobenzene' would be capable of free existence, and still less that when obtained it would prove to be a strong acid. The fact, established

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[1] *Ber.*, **20**, 1776.  [2] *Ibid*, **19**, 308, and **23**, 3136.  [3] *Ibid*, **27**, 1909.

by Curtius, that the group $\underset{N}{\diagup}>N\cdot$ functions in chemical molecules like the atom of chlorine is certainly among the most striking of recent discoveries. Only last year the list of nitrogen compounds was enriched by the addition of $CO(N_3)_2$, the nitrogen analogue of phosgene.[1]

These illustrations, drawn from the compounds of nitrogen, will serve to bring out the wonderful development which our knowledge of the chemistry of this element has undergone within the last few years. I might be tempted here into a digression on the general bearing of the very striking fact that an element comparatively inactive in the free state should be so remarkably active in combination, but I must keep to the main topic, as by means of these compounds it is possible to illustrate still further both the strength and the weakness of our modern conceptions of chemical structure. Consider some of the undiscovered compounds which are foreshadowed by the process of ideal abstraction of types. The azoxy-compounds contain the complex $-N-N-\diagdown O\diagup$ or $-N=N-\ \overset{..}{O}$ . The types would be

$HN-NH\diagdown O\diagup$ or $HN=NH\ \overset{..}{O}$ . The first of these formulæ represents the unknown dihydro-nitrous oxide. The azo-compounds are derivatives of the hypothetical diimide $HN:NH$. An attempt to prepare this compound from azodicarbonic acid[2] resulted in the formation of hydrazine. The diethyl-derivative may have been obtained by Harries,[3] but this is doubtful. It is at present inexplicable why compounds in which the group $\cdot N:N\cdot$ is in combination with aromatic radicles should be so remarkably stable, while the parent compound appears to be incapable of existence. The addition of two atoms of hydrogen converts this type again into a stable compound. There is nothing in the structural formulæ to indicate these facts. The amidines are stable compounds, and the so-called 'anhydro-bases,' or imidazoles, are remarkably stable; the parent compound, $HC\diagdown{\substack{NH \\ NH_2}}$, has not been obtained, while its amido-derivative, $H_2N.C\diagdown{\substack{NH \\ NH_2}}$, is the well-known substance guanidine. The isodiazo-compounds recently discovered by Schraube and Schmidt and by Bamberger[4] are possibly derivatives of the hypothetical substance $O:N.NH_2$, which might be named nitrosamide. Why this compound should not exist as well as nitramide is another question raised by the principle of abstract types. The carbizines were formerly regarded as derivatives of the compounds $CO\diagdown{\substack{NH \\ NH}}$ and $CS\diagdown{\substack{NH \\ NH_2}}$.[5] Although this structure has now been disproved the possible existence of the types has been suggested. Carbizine and thiocarbizine differ from urea and thiocarbamide only by two atoms of hydrogen. These types have not been isolated; if they are incapable of existence the current views of molecular structure give no suggestion of a reason. The diazoamides are derivatives of the hypothetical $H_2N.NH.NH_2$ or $HN\ N.NH_2$, compounds which Curtius speaks of as the propane and propylene of the nitrogen series. The latter complex was at one time thought to exist in diazohippuramide,[6] and a biacidyl derivative of the former type has also been obtained.[7] Both these types await isolation if they are capable of existence. I may add that several attempts to convert diazoamides into dihydro-derivatives by mild alkaline reduction have led me to doubt whether this nitrogen chain can

[1] Curtius, *Ber.*, **27**, 2684.    [2] Thiele, *Ann.*, **271**, 130.    [3] *Ber*, **27**, 2276.
[4] *Ibid*, **27**, 514, 679, &c
[5] Fischer, *Ann*, **212**, 326; Freund and Goldsmith, *Ber*, **21**, 2456.
[6] *Ber* **24**, 3342  This has since been shown to be hippuramide, i e., a derivative of $N_3H$ (*Ber.*, **27**, 779).    [7] *Ibid*, 3314.

exist in combination with hydrocarbon radicles. The bisdiazoamides of H. v. Pechmann and Frobenius [1] are derivatives of the 5-atom chain $H_2N.NH.NH.NH.NH_2$ or $HN:N.NH.N:NH$, a type which hardly seems likely to be of sufficient stability to exist. The tetrazones of Emil Fischer have for their type the 4-atom chain $H_2N.N:N.NH_2$ or $H_2N.NH.NH.NH_3$, of which the free existence is equally problematical, although a derivative containing the chain $-N:N.NH.NH-$ has been obtained by Curtius.[2] Hydrazoic acid may be regarded as a derivative of triimide,

$$HN\left\langle\begin{matrix}NH\\|\\NH\end{matrix}\right.$$

, but this type appears to be also incapable of isolation.[3] The hydrazidines or formazyls of Pinner [4] and of H. v. Pechmann,[5] have for their parent compound the hypothetical substance $H_2N.N:CH.N:NH$. In 1888 Limpricht described certain azo-compounds[6] which, if possessing the structure assigned by that author, must be regarded as derivatives of diamidotetrimide:

$$\begin{matrix}HN - NH\\|\quad|\\HN - NH\end{matrix}\qquad\qquad\begin{matrix}H_2N\ N - N\\\\H_2N\ N - N\end{matrix}$$

Both these types are at present imaginary; whether it is possible for cyclic nitrogen systems to exist we have no means of knowing—all that can be said is that they have never yet been obtained. It is possible, as I pointed out in 1890 at the Leeds meeting of the British Association, that mixed diazoamides may be derivatives of such a 4-atom ring.

Any chemist who has followed the later developments of the chemistry of nitrogen could supply numerous other instances of undiscovered types. A chapter on the unknown compounds of this element would furnish quite an exciting addition to many of those books which are turned out at the present time in such profusion to meet the requirements of this or that examining body. I have selected my examples from these compounds simply because I can claim some of them as personal acquaintances. It would be easy to make use of carbon compounds for the same purpose, but it is unnecessary to multiply details. It has frequently happened in the history of science that a well-considered statement of the shortcomings of a theory has led to its much-desired extension. This is my hope in venturing to point out one of the chief deficiencies in the structural chemistry of the present time. I am afraid that I have handled the case badly, but I am bound to confess that I am influenced by the same feelings as those which prevent us from judging an old and well-tried friend too severely.

The theory of types to which we have reverted as the outcome of the study of molecular structure is capable of almost indefinite extension if, as there is good reason for doing, we replace atoms or groups by their valency analogues in the way of other atoms or groups of atoms. The facts that in cyclic systems N can replace CH (benzene and pyridine), that O, S, and NH are analogues in furfurane, thiophene, and pyrrole, are among the most familiar examples. The remarkable iodo- and iodoso-compounds recently discovered by Victor Meyer and his colleagues are the first known instances in which the trivalent atom of iodine has been shown to be the valency analogue of nitrogen in organic combination. Pushing this principle to the extreme we get further suggestions for new groupings, but, as before, no certainty of prevision. Thus, if nitrogen formed the oxide $N_2O_2$ the series might be written:

$$\begin{matrix}N\\\cdot\cdot\\N\end{matrix}\Big\rangle O\qquad O\Big\langle\begin{matrix}N\\N\end{matrix}\Big\rangle O\qquad \cdot\begin{matrix}N\cdot\\N.\end{matrix}\qquad O\Big\langle\begin{matrix}N-O\\N-O\end{matrix}\qquad or\ O\Big\langle\begin{matrix}N:O\\N:O\end{matrix}\ \&c.$$
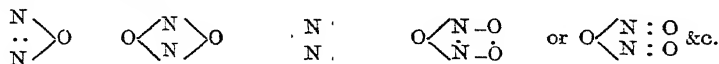
Of course these formulæ are more or less conjectural, being based on valency only. But since nitrous oxide is the analogue of hydrazoic acid, they hint at the
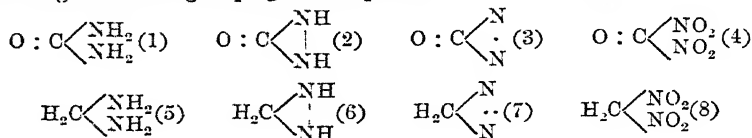
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[1] *Ber.*, **27**, 898.      [2] *Ibid*, **26**, 1263     [3] Curtius, *Ber.*, **26**, 407.
[4] *Ber.*, **17**, 182.     [5] *Ibid*, **25**, 3175.     [6] *Ibid.*, **21**, 3422

possibility of such compounds as $HN\langle\stackrel{N}{\stackrel{\text{ }}{N}}\rangle NH$, &c. If a student produced a set of formulæ corresponding to the above, in which NH had been substituted for O, and asked whether they did not indicate the existence of a whole series of unknown hydrogen compounds of nitrogen, we should probably tell him that his notions of chemical structure had run wild. At the same time I am bound to admit that it would be very difficult, if not impossible, to furnish him with satisfactory reasons for believing that such groupings are improbable. Compare again the series:

$$O : C\langle\stackrel{NH_2}{NH_2}(1) \qquad O : C\langle\stackrel{NH}{\underset{NH}{|}}(2) \qquad O : C\langle\stackrel{N}{\underset{N}{\cdot}}(3) \qquad O : C\langle\stackrel{NO_2}{NO_2}(4)$$

$$H_2C\langle\stackrel{NH_2}{NH_2}(5) \qquad H_2C\langle\stackrel{NH}{\underset{NH}{|}}(6) \qquad H_2C\langle\stackrel{N}{\underset{N}{\cdot\cdot}}(7) \qquad H_2C\langle\stackrel{NO_2}{NO_2}(8)$$

The first is urea; the second, third, fourth, fifth (methylene diamine), and sixth are unknown; the seventh is the remarkably interesting diazomethane discovered last year by H. v. Pechmann.[1] The last compound, dinitromethane, is known in the form of its salts, but appears to be incapable of existence in the free state. There is nothing expressed or implied in the existing theory of chemical structure to explain why dinitromethane is unstable while trinitromethane is stable, and mono- and tetranitromethane so stable as to admit of being distilled without decomposition. Chemists will form their own views as to the possibility or impossibility of such a series as this being completed. Whether there would be a concordance of opinion I will not venture to say; but any chemist who expressed either belief or disbelief with regard to any special member would, I imagine, have great difficulty in giving a scientific reason for the faith which is in him. At the most, he would have only the very unsafe guide of analogy to fall back upon. Perhaps by the time the British Association holds its next meeting at Ipswich it will have become possible to prove that one particular configuration of certain atoms is possible and another configuration impossible. Then will have been achieved that great advance for which we are waiting—the reunion of the two streams into which our science began to diverge shortly after the last Ipswich meeting.

The present position of structural chemistry may be summed up in the statement that we have gained an enormous insight into the anatomy of molecules, while our knowledge of their physiology is as yet in a rudimentary condition In the course of the foregoing remarks I have endeavoured to indicate the direction in which our theoretical conceptions are most urgently pressing for extension It is, perhaps, as yet premature to pronounce an opinion as to whether the next development is to be looked for from the stereochemical side; but it is not going too far to express once again the hope that the geometrical representation of valency will give us a deeper insight into the conditions which determine the stability of atomic configurations. The speculations of A. v. Baeyer, Wislicenus, Victor Meyer, Wunderlich, Bischoff, and others have certainly turned the attention of chemists towards a quarter from which a new light may eventually dawn.

### THE PROGRESS OF SYNTHETICAL CHEMISTRY.

If, in my earnest desire to see the foundations of structural chemistry made more secure, I may have unwittingly given rise to the impression that I am depreciating its services as a scientific weapon, let me at once hasten to make amends by directing attention to the greatest of its triumphs, the synthesis of natural products, i e., of compounds which are known to be produced by the vital processes of animals and plants.

Having been unable to find any recent list of the natural compounds which have been synthesised, I have compiled a set of tables which will, I hope, see the

[1] *Ber*, 27, 1888.

light at no very distant period. According to this census we have now realised about 180 such syntheses. The products of Bacteria have been included in the list because these compounds are the results of vital activity in the same sense that alcohol is a product of the vital activity of the yeast plant. On the other hand the various uro-compounds resulting from the transformation in the animal economy of definite chemical substances administered for experimental purposes have been excluded, because I am confining my attention to natural products. Of course the importance of tracing the action of the living organism on compounds of known constitution from the physiological point of view cannot be overestimated. Such experiments will, without doubt, in time shed much light on the working of the vital laboratory.

The history of chemical synthesis has been so thoroughly dealt with from time to time that I should not have ventured to obtrude any further notice of this subject upon your patience were it not for a certain point which appeared to me of sufficient interest to merit reconsideration. It is generally stated that the formation of urea from ammonium cyanate by Wöhler in 1828 was the first synthesis of an organic compound. There can be no doubt that this discovery, which attracted much attention at the time, gave a serious blow to the current conceptions of organic chemistry, because urea was so obviously a product of the living animal It will be found, however, that about the same time Henry Hennell, of Apothecaries' Hall, had really effected the synthesis of alcohol—that is to say, had synthesised this compound in the same sense that Wöhler had synthesised urea. The history is soon told. In 1826 Hennell (through Brande) communicated a paper to the Royal Society which appears in the ' Philosophical Transactions ' for that year.[1] In studying the compounds produced by the action of sulphuric acid on alcohol, and known as ' oil of wine,' he obtained sulphovinic acid, which had long been known, and gave fairly good analyses of this acid and of some of its salts, while expressing in the same paper very clear notions as to its chemical nature. Having satisfied himself that sulphovinic acid is a product of the action in question, he then proceeded to examine some sulphuric acid which had absorbed eighty times its volume of olefiant gas, and which had been placed at his disposal for this purpose by Michael Faraday. From this he also isolated sulphovinic acid. In another paper, communicated to the Royal Society in 1828,[2] he proves quantitatively that when sulphovinic acid is distilled with sulphuric acid and water the whole of the alcohol and sulphuric acid which united to form the sulphovinic acid are recovered. In the same paper he shows that he had very clear views as to the process of etherification. Hennell's work appears to have been somewhat dimmed by the brilliancy of his contemporaries who were labouring in the same field; but it is not too much to claim for him, after the lapse of nearly seventy years, the position of one of the pioneers of chemical synthesis. Of course in his time the synthesis was not complete, because he did not start from inorganic materials. The olefiant gas used by Faraday had been obtained from coal-gas or oil-gas. Moreover, in 1826–1828 alcohol was not generally regarded as a product of vital activity, and this is, no doubt, the reason why the discovery failed to produce the same excitement as the formation of urea But the synthesis of alcohol from ethylene had, nevertheless, been accomplished, and this hydrocarbon occupied at that time precisely the same position as ammonium cyanate. The latter salt had not then been synthesised from inorganic materials, and the formation of urea, as Schorlemmer points out,[3] was also not a complete synthesis. The reputation of Wöhler, the illustrious friend and colleague of the more illustrious Liebig, will lose not a fraction of its brilliancy by the raising of this historical question. Science recognises no distinction of nationality, and the future historian of synthetical chemistry will not begrudge the small niche in the temple of Fame to which Hennell is entitled.

[1] ' On the Mutual Action of Sulphuric Acid and Alcohol, with Observations on the Composition and Properties of the resulting compound,' *Phil. Trans*, 1826, p 240
[2] ' On the Mutual Action of Sulphuric Acid and Alcohol, and on the Nature of the Process by which Ether is formed,' *Phil. Trans*, 1828, p 365
[3] *The Rise and Development of Organic Chemistry*. p 195.

Like many other great discoveries in science, the artificial formation of natural products began, as in the case of alcohol and urea, with observations arising from experiments not primarily directed to this end. It was not till the theory of chemical structure had risen to the rank of a scientific guide that the more complicated syntheses were rendered possible by more exact methods. We justly credit structural chemistry with these triumphant achievements. In arriving at such results any defects in the theory of structure are put out of consideration because—and this point must never be lost sight of—all doubt as to the possibility of this or that atomic grouping being stable is set aside at the outset by the actual occurrence of the compound in nature. The investigator starts with the best of all assurances. From the time of Wöhler and Hennell the course of discovery in this field has gone steadily on. The announcement of a new synthesis has ceased to produce that excitement which it did in the early days when the so-called 'organic' compounds were regarded as products of a special vital force. The interest among the uninitiated now rises in proportion to the technical value of the compound. The present list of 180 odd synthetical products comprises, among the latest discoveries, gentisin. the colouring-matter of the gentian root (*Gentiana lutea*), which has been prepared by Kostanecki and Tambor, and caffeine, synthesised by Emil Fischer and Lorenz Ach, starting from dimethylurea and malonic acid.

I have allowed myself no time for those prophetic flights of the imagination which writers on this subject generally indulge in. When we know more about the structure of highly complex molecules, such as starch and albumin, we shall probably be able to synthesise these compounds. It seems to me more important just at present to come to an understanding as to what is meant by an organic synthesis. There appears to be an impression among many chemists that a synthesis is only effected when a compound is built up from simpler molecules. If the simpler molecules can be formed directly from their elements, then the synthesis is considered to be complete. Thus urea is a complete synthetical product, because we can make hydrogen cyanide from its elements: from this we can prepare a cyanate, and finally urea. In dictionaries and text-books we find synthetical processes generally separated from modes of formation, and the latter in their turn kept distinct from methods of preparation. The distinction between formation and preparation is obviously a good one, because the latter has a practical significance for the investigator. But the experience gained in drawing up the tables of synthesised compounds, to which I have referred, has resulted in the conclusion that the terms 'synthesis' and 'mode of formation' have been either unnecessarily confused or kept distinct without sufficient reason, and that it is impossible now to draw a hard-and-fast line between them. Some recent writers, such, for example, as Dr. Karl Elbs, in his admirable work on this subject,[1] have expanded the meaning of the word synthesis so as to comprise generally the building up of organic molecules by the combination of carbon with carbon, without reference to the circumstance whether the compound occurs as a natural product or not. But although this definition is sufficiently wide to cover the whole field of the production of carbon compounds from less complex molecules, it is in some respects too restricted, because it excludes such well-known cases as the formation of hydrogen cyanide from its elements, or of urea from ammonium cyanate. I should not consider the discussion of a mere question of terminology of sufficient importance to occupy the attention of this Section were it not for a matter of principle, and that a principle of the very greatest importance, which I believe to be associated with a clear conception of chemical synthesis. The great interest of all work in this field arises from our being able, by laboratory processes, to obtain compounds which are also manufactured in nature's laboratory—the living organism. It is in this direction that our science encroaches upon biology through physiology. Now, if we confine the notion of synthesis to the building up of molecules from simpler molecules or from atoms, we exclude one of nature's

[1] *Die synthetischen Darstellungsmethoden der Kohlenstoffverbindungen.* Leipzig, 1889.

methods of producing many of these very compounds which we claim to have synthesised. There can be no manner of doubt that a large proportion, if not a majority, of the natural products which have been prepared artificially are not synthesised by the animal or plant in the sense of building up at all. They are the results of the breaking down—of the degradation—of complex molecules into simpler ones   I urge, therefore, that if in the laboratory we can arrive at one of these products by decomposing a more complex molecule by means of suitable reagents, we have a perfect right to call this a synthesis, provided always that the more complex molecule, which gives us our compound, can be in its turn synthesised, by no matter how many steps, from its constituent atoms.   Thus oxalic acid has been directly synthesised from carbon dioxide by Kolbe and Drechsel by passing this gas over potassium or sodium amalgam heated to 360°.   Whether the plant makes oxalic acid directly out of carbon dioxide we cannot at present state, if it does it certainly does not employ Kolbe and Dreschel's process.   On the other hand this acid may, for all that is known, exist in the plant as a product of degradation.   Many more complex acids, such as citric and tartaric, break down into oxalic acid when fused with potash.   Both citric and tartaric acids can now be completely synthesised; therefore the formation of oxalic acid from these by potash fusion is a true synthesis.

The illustration given will make clear the point which I am urging.   The distinction between a synthesis and a mode of formation vanishes when we can obtain a compound by the breaking down of a more complex molecule in all those cases where the latter can be completely built up.   If we do not expand the meaning of synthesis so as to comprise such cases we are simply shutting the door in Nature's face.   It must be borne in mind that the actual yield of the compound furnished by the laboratory process does not come into consideration, because it may be generally asserted that in most cases the artificial processes are not the same as those which go on in the animal or plant   The information of real value to the physiologist which these syntheses give is the suggestion that such or such a compound may possibly result from the degradation of this or that antecedent compound, and not from a process of building up from simpler molecules.

### The Bearing of Chemical Synthesis on Vital Chemistry.

With these views—the outcome of structural chemistry—the chemist and physiologist may join hands and move fearlessly onwards towards the great mystery of vital chemistry.   In considering the results of organic synthesis two questions always arise as it were spontaneously : How does nature produce these complicated molecules without the use of strong reagents and at ordinary temperatures ?   What bearing have our laboratory achievements on the mechanism of vitality ?   The light shed upon these questions by experimental investigation has as yet flickered only in fitful gleams.   We are but dwellers in the outer gates, waiting for the guide who is to show us the bearing of modern research on the great problem which confronts alike the physicist, the chemist, and the biologist. The chemical processes that go on in the living organism are complex to an extent that is difficult to realise.   Of the various compounds of animal or vegetable origin that have been produced synthetically some are of the nature of waste products, resulting from metabolic degradation ; others are the result of zymolytic action within the organism, and others, again, are secondary products arising from the action of associated Bacteria, the relationship between the Bacteria and their host being as yet imperfectly understood.   The answer to the question how nature produces complicated organic molecules will be much facilitated when the physiologist, by experiment and observation, shall have made possible a sound classification of these synthetical products based on their mode of origination in the organism.

The enlargement of the definition of organic synthesis which I have advocated has been rendered necessary by the consideration of certain questions which have arisen in connection with the present condition of chemical discovery in this field. What evidence is there that any one of the 180 compounds which have been pre-

pared artificially is produced in the organism by a direct process of building up ? Is not the opposite view quite as probable ?  May they not, from the simplest to the most complex, be products of the degradation of still more complex molecules ? I venture to suggest—not without some temerity lest our colleagues of Sections I and K should treat me as an intruder—that this view should be given a fair trial. I am aware that the opposite view, especially as regards plant assimilation, has long been held, and especially since 1870, when v. Baeyer advanced his celebrated theory of the formic aldehyde origin of carbohydrates.  It is but natural to consider that the formation of a complex molecule is the result of a building-up process.  It must be remembered, however, that in the living organism there is always present a compound or mixture, or whatever we like to call it, of a highly complex proteid nature, which, although at present indefinite from the purely chemical point of view, is the essence of the vitality.  Of course I refer to what biologists have called protoplasm.  Moreover, it is perhaps necessary to state what is really nothing more than a truism, viz , that protoplasm is present in and forms a part of the organism from the very beginning of its existence—from the germ to the adult, and onwards to the end of life.  Any special chemical properties pertaining to protoplasm are inseparable from the animal or plant until that period arrives which Kekulé has hinted at when we shall be able to 'build up the formative elements of living organisms' in the laboratory.[1]  But here I am afraid I am allowing the imagination to take a flight which I told you a few minutes ago that time would not admit of.

The view that requires pushing forward into a more prominent position than it has hitherto occupied is that all the chemical transformations in the organism—at any rate all the primary changes—are made possible only by the antecedent combination of the substances concerned with living protoplasmic materials.   The carbon dioxide, water, &c., which the plant absorbs must have formed a compound or compounds with the protoplasmic material of the chloroplasts before starch, or sugar, or cellulose can be prepared.  There is, on this view, no such process as *the direct combination* of dead molecules to build up a complex substance.  Everything must pass through the vital mill.  The protoplasmic molecule is vastly more complex than any of the compounds which we have hitherto succeeded in synthesising.  It might take up and form new and unstable compounds with carbon dioxide or formic aldehyde, or sugar, or anything else, and our present methods of investigation would fail to reveal the process.  If this previous combination and, so to speak, vitalisation of dead matter actually occurs, the appearance of starch as the first visible product of assimilation, as taught by Sachs, or the formation of a 12-carbon-atom sugar as the first carbohydrate, as shown by the recent researches of Horace Brown and G. H Morris, is no longer matter for wonderment.  The chemical equations given in physiological works are too purely chemical ; the physiologists have, I am afraid, credited the chemists with too much knowledge—it would appear as though their intimate familiarity with vital processes had led them to undervalue the importance of their prime agent.  In giving expression to these thoughts I cannot but feel that I am treating you to the strange spectacle of a chemist pleading from the physiologists for a little more vitality in the chemical functions of living organisms.  The future development of vital chemistry rests, however, with the chemist and physiologist conjointly ; the isolation, identification, and analysis of the products of vital activity, which has hitherto been the task of the chemist, is only the preliminary work of physiological chemistry leading up to chemical physiology.

## PROTOPLASMIC THEORY OF VITAL SYNTHESIS.

The supposition that chemical synthesis in the organism is the result of the combination of highly complex molecules with simpler molecules, and that the unstable compounds thus formed then undergo decomposition with the formation of new products, may be provisionally called the protoplasmic theory of vital synthesis.  From this standpoint many of the prevailing doctrines will have to

[1] *Nature*, vol. xviii p 212.

be inverted, and the formation of the more complex molecules will be considered to precede the synthesis of the less complex. It may be urged that this view simply throws back the process of vital synthesis one stage and leaves the question of the origin of the most complex molecules still unexplained. I grant this at once; but in doing so I am simply acknowledging that we have not yet solved the enigma of life. We are in precisely the same position as is the biologist with respect to abiogenesis, or the so-called ' spontaneous generation.' To avoid possible misconception let me here state that the protoplasmic theory in no way necessitates the assumption of a special ' vital force.' All that is claimed is a peculiar, and at present to us mysterious, power of forming high-grade chemical combinations with appropriate molecules. It is not altogether absurd to suppose that this power is a special property of nitrogen in certain forms of combination. The theory is but an extension of the views of Kuhne, Hoppe-Seyler, and others respecting the mode of action of enzymes. Neither is the view of the degradational origin of synthetical products in any way new.[1] I merely have thought it desirable to push it to its extreme limit in order that chemists may realise that there is a special chemistry of protoplasmic action, while the physiologists may exercise more caution in representing vital chemical transformations by equations which are in many cases purely hypothetical, or are based on laboratory experiments which do not run parallel with the natural process. The chemical transformations which go on in the living organism are thus referred back to a peculiarity of protoplasmic matter, the explanation of which is bound up with the inner mechanism of the process of assimilation. If, as the protoplasmic theory implies, there must be combination of living protoplasm with appropriate compounds before synthesis is possible, then the problem resolves itself into a determination of the conditions which render such combination possible—i.e., the conditions of assimilation. It may be that here also light will come from the stereochemical hypothesis. The first step was taken when Pasteur found that organised ferments had the power of discriminating between physical isomerides; a similar selective power has been shown to reside in enzymes by the researches of Emil Fischer and his coadjutors. Fischer has quite recently expressed the view that the synthesis of sugars in the plant is preceded by the formation of a compound of carbon dioxide, or of formic aldehyde, with the protoplasmic material of the chloroplast, and similar views have been enunciated by Stohmann. The question has further been raised by van 't Hoff, as well as by Fischer, whether a stereochemical relationship between the living and dead compounds entering into combination is not an absolutely essential condition of all assimilation. The settlement of this question cannot but lead us onwards one stage towards the solution of the mystery that still surrounds the chemistry of the living organism.

### RECENT DISCOVERIES OF GASEOUS ELEMENTS.

The past year has been such an eventful one in the way of startling discoveries that I must ask indulgence for trespassing a little further upon the time of the Section. It was only last year at the Oxford meeting of the British Association that Lord Rayleigh and Prof. Ramsay announced the discovery of a gaseous constituent of the atmosphere which had up to that time escaped detection. The complete justification of that announcement is now before the world in the paper recently published in the 'Philosophical Transactions' of the Royal Society. The history of this brilliant piece of work is too recent to require much recapitulation. I need only remind you how, as the result of many years' patient determinations of the density of the gases oxygen and nitrogen, Lord Rayleigh established the fact that atmospheric nitrogen was heavier than nitrogen from chemical sources, and

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[1] See, e.g., Vines' *Lectures on the Physiology of Plants*, pp 145, 218. 227, 233, and 234 Practically all the great classes of synthetical products are regarded as the results of the destructive metabolism of protoplasm A special plea for protoplasmic action has also been urged, from the biological side, by W T Thiselton-Dyer, *Journ. Chem. Soc.*, 1893 ; *Trans.*, pp 680–681.

was then led to suspect the existence of a heavier gas in the atmosphere. He set to work to isolate this substance, and succeeded in doing so by the method of Cavendish. In the meantime Prof. Ramsay, quite independently, isolated the gas by removing the nitrogen by means of red-hot magnesium, and the two investigators then combining their labours, followed up the subject, and have given us a memoir which will go down to posterity among the greatest achievements of an age renowned for its scientific activity

The case in favour of argon being an element seems to be now settled by the discovery that the molecule of the gas is monatomic, as well as by the distinctness of its electric spark spectrum. The suggestion put forward soon after the discovery was announced, that the gas was an oxide of nitrogen, must have been made in complete ignorance of the methods by which it was prepared. The possibility of its being $N$, has been considered by the discoverers and rejected on very good grounds. Moreover, Peratoner and Oddo have been recently making some experiments in the laboratory of the University of Palermo with the object of examining the products of the electrolysis of hydrazoic acid and its salts They obtained only ordinary nitrogen, not argon, and have come to the conclusion that the anhydride $N_3.N_3$ is incapable of existence, and that no allotropic form of nitrogen is given off. It has been urged that the physical evidence in support of the monatomic nature of the argon molecule, viz., the ratio of the specific heats, is capable of another interpretation—that argon is in fact an element of such extraordinary energy that its atoms cannot be separated, but are bound together as a rigid system which transmits the vibrational energy of a sound-wave as motion of translation only. If this be the state of affairs we must look to the physicists for more light. So far as chemistry is concerned, this conception introduces an entirely new set of ideas, and raises the question of the monatomic character of the mercury molecule which is in the same category with respect to the physical evidence. It seems unreasonable to invoke a special power of atomic linkage to explain the monatomic character of argon, and to refuse such a power in the case of other monatomic molecules, like mercury or cadmium. The chemical inertness of argon has been referred also to this same power of self-combination of its atoms. If this explanation be adopted it carries with it the admission that those elements of which the atoms composing the molecule are the more easily dissociated should be the more chemically active. The reverse appears to be the case if we bear in mind Victor Meyer's researches on the dissociation of the halogens, which prove that under the influence of heat the least active element, iodine, is the most easily dissociated. On the whole, the attempts to make out that argon is polyatomic by such forced hypotheses cannot at present be considered to have been successful, and the contention of the discoverers that its molecule is monatomic must be accepted as established.

In searching for a natural source of combined argon Professor Ramsay was led to examine the gases contained in certain uranium and other minerals, and by steps which are now well known he has been able to isolate helium, a gas which was discovered by means of the spectroscope in the solar chromosphere during the eclipse of 1868 by Professors Norman Lockyer and E. Frankland In his address to the British Association in 1872 [1] the late Dr. W. B. Carpenter said:—

'But when Frankland and Lockyer, seeing in the spectrum of the yellow solar prominences a certain bright line not identifiable with that of any known terrestrial flame, attribute this to a hypothetical new substance which they propose to call helium, it is obvious that their assumption rests on a far less secure foundation, until it shall have received that verification which, in the case of Mr. Crookes' researches on thallium, was afforded by the actual discovery of the new metal, whose presence had been indicated to him by a line in the spectrum not attributable to any substance then known.'

It must be as gratifying to Professors Lockyer and Frankland as it is to the chemical world at large to know that helium may now be removed from the category of solar myths and enrolled among the elements of terrestrial matter. The

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[1] *Reports*, 1872, p. lxxiv.

sources, mode of isolation, and properties of this gas have been described in the papers recently published by Professor Ramsay and his colleagues. Not the least interesting fact is the occurrence of helium and argon in meteoric iron from Virginia, as announced by Professor Ramsay in July.[1] Like argon, helium is monatomic and chemically inert so far as the present evidence goes. The conditions under which this element exists in cleveite, uraninite, and the other minerals have yet to be determined.

Taking a general survey of the results thus far obtained, it seems that two representatives of a new group of monatomic elements characterised by chemical inertness have been brought to light. Their inertness obviously interposes great difficulties in the way of their further study from the chemical side; the future development of our knowledge of these elements may be looked for from the physicist and spectroscopist. Professor Ramsay has not yet succeeded in effecting a combination between argon or helium and any of the other chemical elements. M. Moissan finds that fluorine is without action on argon. M Berthelot claims to have brought about a combination of argon with carbon disulphide and mercury, and with 'the elements of benzene, . . . with the help of mercury,' under the influence of the silent electric discharge. Some experiments which I made last spring with Mr. R. J. Strutt with argon and moist acetylene submitted to the electric discharge, both silent and disruptive, gave very little hope of a combination between argon and carbon being possible by this means. The coincidence of the helium yellow line with the D₃ line of the solar chromosphere has been challenged, but the recent accurate measurements of the wave-length of the chromospheric line by Prof. G E Hale, and of the line of terrestrial helium by Mr. Crookes, leave no doubt as to their identity. Both the solar and terrestrial lines have now been shown to be double. The isolation of helium has not only furnished another link proving community of matter, and, by inference, of origin between the earth and sun, but an extension of the work by Professor Norman Lockyer, M. Deslandres, and Mr Crookes, has resulted in the most interesting discovery that a large number of the lines in the chromospheric spectrum, as well as in certain stellar spectra, which had up to the present time found no counterparts in the spectra of terrestrial elements can now be accounted for by the spectra of gases contained with helium in these rare minerals. The question now confronts us, Are these gases members of the same monatomic inert group as argon and helium ? Whether, and by what mechanism, a monatomic gas can give a complicated spectrum is a physical question of supreme interest to chemists, and I hope that a discussion of this subject with our colleagues of Section A will be held during the present meeting. That mercury is capable under different conditions of giving a series of highly complex spectra can be seen from the memoir by J M. Eder and E. Valenta, presented to the Imperial Academy of Sciences of Vienna in July 1894. With respect to the position of argon and helium in the periodic system of chemical elements, it is, as Professor Ramsay points out, premature to speculate until we are quite sure that these gases are homogeneous. It is possible that they may be mixtures of monatomic gases, and in fact the spectroscope has already given an indication that they contain some constituent in common. The question whether these gases are mixtures or not presses for an immediate answer I will venture to suggest that an attack should be made by the method of diffusion If argon or helium were allowed to diffuse fractionally through a long porous plug into an exhausted vessel there might be some separation into gases of different densities, and showing modifications in their spectra, on the assumption that we are dealing with mixtures composed of molecules of different weights.

[1] *Nature*, vol. lii p 224

# British Association for the Advancement of Science

## IPSWICH 1895.

## UNDERGROUND IN SUFFOLK AND ITS BORDERS.

# ADDRESS

### TO THE

# GEOLOGICAL SECTION

BY

### W. WHITAKER, B.A., F.R.S., F.G.S.,

#### PRESIDENT OF THE SECTION.

WHEN the British Association revisits a town it is not unusual for the Sectional Presidents to refer to the addresses of their local predecessors, and to allude to the advance of their science since the former meeting. I have at all events tried to follow this course, with the sad result of having to chronicle a falling back rather than an advance in our methods of procedure; for at the meeting of 1851 all the Sectional Presidents had the wisdom not to give an address, and of all the inventions of later years I look upon the presidential address as perhaps the worst.

Had I the courage of my opinion I should not now trouble you; but an official life of over thirty-eight years has led me to do what I am told to do, and to suppress my own ideas of what is right. After all it is the fault of the Sections themselves that they should suffer the evil of addresses. They could disestablish the institution without difficulty.

On these occasions it is not usual to allude to the personal losses our science has had in the past year; but there are times when the lack of a familiar presence can hardly be passed over, and since we last met we have lost one of our most constant friends, who had served us long and well, and had been our Secretary for a far longer time than any other holder of that office. When we were at Oxford last summer none of us could have thought that it was our last meeting with William Topley.

I do not now mean to say anything on the origin or on the classification of the various divisions of the Crag and of the Drift that occur so plentifully around us, and form the staple interest of East Anglian geology These subjects, which are the more interesting from being controversial, I leave to my brother-hammerers, and without claiming the credit of magnanimity in so doing, having said what I had to say on them in sundry Geological Survey Memoirs. The object of this address is to carry you below the surface, and to point out how much our knowledge of the geology of the county in which we meet has been advanced by workers in another field, by engineers and others in their search for water. As far as possible allusion will be made only to work in Suffolk; but we must occasionally invade the neighbouring counties.

This kind of evidence has chiefly accumulated since the meeting of the Association at Ipswich, in 1851; for of the 476 Suffolk wells of which an account, with

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some geologic information, has been published, only 68 were noticed before that year, all but two of these being in a single paper. The notes on all these wells are now to be found in twelve Geological Survey Memoirs that refer to the county. Number alone, however, is not the only point, and many of the later records are marked by a precision and a detail rarely approached in the older ones. It should be stated that in the above and in the following numbers strict accuracy is not professed, nor is it material. A slight error in the number of the wells, one way or the other, would make practically no difference to the general conclusions.

Now let us see how these records affect our knowledge of the various geologic formations, beginning with the newest and working downward.

## The Drift.

Under this head, as a matter of convenience for the present purpose, we will include everything above the Chillesford Clay. There is no need for refinement of classification, and the thin beds that come in between that clay and the Drift in some parts do not affect the evidence we have to deal with.

As a matter of fact it is only from wells that we can tell the thickness of the Drift over most of the great plateau that this formation chiefly forms; open sections through a great thickness of Drift, to its base, are rare, except on the coast.

There is often some doubt in classifying the beds, the division between Drift and Crag being sometimes hard to make in sections of wells and borings; but from an examination of the records of these Suffolk sections that pass through any part of the Drift Series (as defined above) we find that no less than 173 show a thickness of 50 feet and upward, whilst of these 34 prove no less than 100 feet of Drift, many reaching to much more. Of the two that are said to show a thickness of over 200 feet and the one other said to be more than 300 feet deep in Drift, we can hardly feel certain; but such amounts have been recorded with certainty as occurring in the neighbouring county of Essex.

These great thicknesses (chiefly consisting of Boulder Clay) show the importance of the Drift, and the impossibility of mapping the formations beneath with any approach to accuracy, on the supposition that the Drift is stripped off, as is the case in the ordinary geologic map. The records also show the varying thickness of the Drift, and how difficult it often is therefore to estimate the thickness at a given spot. Sometimes the sections seem to point to the existence of channels filled with Drift, such as are found also in Essex and in Norfolk; and it may be noted that in the northern inland part of the former county, one of these channels has been traced, though of course not continuously, for some 11 miles along the valley of the Cam, and at one place to the depth of 340 feet (or nearly 140 below sea-level), the bottom of the Drift moreover not having been reached even then. A channel of this sort seems to occur close to us, in the midst of the town of Ipswich, where, by St. Peter's, one boring has pierced 70 feet of Drift, and another 127, in ground but little above the sea-level.

As the Drift sands and gravels, that in many places occur below the Boulder Clay, often yield a fair amount of water, the proof of their occurrence and of the thickness of the overlying clay is of some practical good.

## The Crag.

On this geologic division we have a less amount of information, as would be expected from the fact that it is not nearly so widespread as the Drift, and this information is confined to the Upper, or Red, Crag, the Lower, or Coralline, Crag occurring only over a very small area, and no evidence of its underground extension being given by wells.

What we learn of the Red Crag, however, is of interest, several wells having proved that it is far thicker underground than would have been supposed from what is seen where its base crops out. One characteristic, indeed, of this sandy deposit, in the many parts where it can be seen from top to bottom, is its thinness,

as in such places it rarely reaches a thickness of 40 feet. But. on the other hand, wells at Hoxne seem to prove more than 60 feet of Crag, whilst at Saxmundham the formation is 100 feet thick, and at Leiston and Southwold over 140. Further north, just within the border of Suffolk, there is, at Beccles, a thickness of 80 feet of sand, or, with the overlying Chillesford Clay, a total of 95. Our underground information has, then, trebled the known thickness of the Upper Crag of Suffolk.

It has also shown that at some depth underground the colour-name is a misnomer, the shelly sands being light-coloured and not red. This is the case too with some other deposits, which owe their reddish-brown colour at the surface to peroxide of iron. Presumably the iron-salt is in a lower state of oxidation until it comes within reach of surface-actions. This seems to point to the risk of taking colour as the mark of a geologic formation.

### Eocene Tertiaries.

Below the Crag there is a great gap in the geologic series, and we come to some of the lower of the Tertiary formations, about which little had been published, as regards Suffolk, before the work of the Geological Survey in the county. It seems as if the special interest in the more local Crag had led observers to neglect these beds, which had been amply noticed in other parts.

We have records of more than forty wells in Suffolk that are partly in these deposits, and of these thirty-six reach down to the Chalk, twenty giving good sections from the London Clay to the Chalk. The thickness of the Lower London Tertiaries (between those formations) thus proved varies from 30 to $79\frac{1}{2}$ feet, the higher figure being much greater than anything shown at the outcrop. The greatest recorded thickness is at Leiston, where, moreover, the top 26 feet of the $79\frac{1}{2}$ may belong to the uppermost and most local of the three divisions of the Series, the Oldhaven Beds, of very rare occurrence in the county. The next greatest thickness is at Southwold, where the whole has been classed as Reading Beds (the persistent division), though here and elsewhere it is possible that the underlying Thanet Beds are thinly represented. It is noteworthy that at both these places, where the Lower London Tertiaries are thick, they are also at a great depth, beginning at $252\frac{1}{2}$ and 218 feet respectively, which looks as if, like the Crag, they thickened in their underground course away from the outcrop.

The important evidence given by these wells, however, is not as regards thickness; it is to show the underground extent of the older Tertiary beds, beneath the great sheet of Crag and Drift that prevents them from coming to the surface north-eastward from the neighbourhood of Woodbridge. It is clear that over this large tract we can know nothing of the beds beneath the Crag otherwise than from wells and borings; and, until these were made, our older geologic maps cut off the older Tertiary beds far south of the parts to which we now know that they reach, though hidden from our sight. No one, for instance, would have imagined many years ago that at Southwold the Chalk would not be touched till a boring had reached the depth of 323 feet, or some 280 below sea-level, nor that at Leiston those figures would have been about 297 and 240.

It is from calculations based on the levels of the junction of the Chalk and the Tertiary beds in many wells that the line engraved on the Geological Survey map as the probable boundary of the latter beds under the Crag and Drift has been drawn. From what has gone before, however, as to the great irregularity in the thickness of the Drift, it is clear that this line must be taken only as approximate, and open to correction as further evidence is got; albeit the junction of the Chalk and the Tertiary beds is found to be here, as elsewhere, fairly even, along an inclined plane that sinks toward the coast.

### Cretaceous Beds.

Though the *Chalk* is reached by very many wells, yet we get less information about it, by reason of its great thickness. Moreover, the great amount of overlying beds in many cases is a bar to deep exploration.

Of our Suffolk wells there are forty which go through 100 feet or more of Chalk. Of these twenty go through 200 feet or more, half of these to 300 or more, and again half of the ten to 400 or more, a very exact piece of geometric progression, or more strictly, retrogression. Although two wells pass through the great thickness of more than 800 feet of chalk, yet neither of them gives us the full thickness of the formation; for the 816 feet at Landguard Fort do not reach to the base, whilst the 843 (or 817) feet at Combs, near Stowmarket, do not begin at the top.

As in no case yet recorded has the Chalk been pierced from top to bottom in Suffolk (a defect that will be supplied during this meeting by the description of the Stutton boring), that is to say, no boring has gone from the overlying older Tertiary beds to the underlying Gault, we must now, therefore, cross the border of the county to get full information as to the thickness of the Chalk; and we have not far to go, for the well-known Harwich boring passes through the whole of the Chalk, proving a thickness of 890 feet. It is almost certain, indeed, that this should be given as a few feet more, for the 22 feet next beneath, which have been described as Gault mixed with Greensand, is probably in part the green clayey glauconitic base of the Chalk Marl. We may fairly add for this another 5 feet (as also in the case of the Combs boring), and may say that, in round numbers, the Chalk reaches a thickness of about 900 feet in the south-eastern part of Suffolk. Toward the northern border of the county it is probably more, as the deep boring at Norwich passes through nearly 1,160 feet of Chalk, and that without beginning at the top of the formation.

Of our recorded Suffolk wells only three reach the base of the Chalk, at Mildenhall, Culford and Combs; consequently we have little knowledge of the divisions of the Chalk. These divisions, indeed, are of comparatively late invention, having been evolved since the publication of many of the deep sections that have been referred to.

If the Upper Chalk at Harwich goes as far down as the flints, then we must allow it to be 690 feet thick, leaving little more than 200 for the Middle and Lower Chalk together. At Landguard Fort, from the same point of view, the Upper Chalk would certainly be 500 feet thick, and one can't say how much more.

At Combs, on the other hand, flints have been recorded as present only in the top 27 feet of the Chalk; but whilst this may have been owing in part to the boring having passed between fairly scattered nodules, and in part perhaps to insufficient care in observation, at Harwich it is possible that some flints may have been carried down in the process of boring.

What evidence we have tends to show, however, that the Upper Chalk forms a good deal more than half, and perhaps about two-thirds, of the formation, the Middle and Lower Chalk being rather thin. This agrees with what is found in other parts where the Chalk is thick, extra thickness being chiefly due to the highest division. The glauconitic marly bed at the base seems to be well developed and to be underlain by the Gault clay; so that we have no good evidence of the occurrence of Upper Greensand. This division may be thinly represented at Mildenhall, but it is difficult to classify some of the beds passed through in the old boring there.

As far as the *Gault* is concerned little of course is known; but that little points to this formation being unusually thin, presumably only 73 feet from top to bottom at Culford, and probably not more than between 50 and 60 at and near Harwich. In the north-western part of the neighbouring county of Norfolk it is well known to be still less, the clay thinning out northward along the outcrop, until at last there is nothing but a few feet of Red Chalk between the carstone of the Lower Greensand and the Chalk. The Gault being of much greater thickness around and under other parts of the London Basin, this thinning in Norfolk and Suffolk is noteworthy. The absence of the more inconstant Upper Greensand is to be expected in most places, and calls for no remark; it may, however, be noted that geologists are coming to the conclusion that these two divisions are really parts of one formation, and one result of this geologic wedding is for the in-

constancy of one partner to be greatly compensated by the constancy of the other.

The *Lower Greensand* has been found in one deep boring only, at Culford, in the western part of the county, where it is represented by 32½ feet of somewhat exceptional beds. This slight thickness prepares us for underground thinning, and in the far east of the county the formation is presumably absent, there being no trace of it at Harwich or at Stutton.

With the Cretaceous beds we pass from the regular orderly succession of geologic formations; indeed it may be said that when we reach the base of the Gault we pass out of the region of facts into the realm of speculation.

We have come then to perhaps the most interesting problem in the geology of the Eastern Counties, to the consideration of the question, What rocks underlie the Cretaceous beds at great depths? In dealing with this I must ask your patience for frequent excursions outside our special district, and sometimes indeed far away from it

Beyond the outcrop of the lower beds of the Cretaceous Series in Cambridgeshire and Norfolk, we find of course a powerful development of the great Jurassic Series; but the only two recorded deep borings in and near Suffolk that have pierced through the Cretaceous base, at Culford on the north-west and at Harwich on the south-east, show not a trace of anything Jurassic; they pass suddenly from Cretaceous into far older rocks. And here a paper that is to be brought before you must be anticipated, to a slight extent, by adding that the trial-boring at Stutton shows just the same thing, the Gault resting directly on a much older rock, which cannot be classed as of Secondary age.

There is no need now to discuss the literature of the old rocks underground in South-Eastern England, that has often been done. We may take the knowledge of what has been shown by the various deep borings as common property, and may use it freely, without troubling to state the source of each piece of information, and I will not therefore burden this address with references. I had indeed thought of supplementing a former account by noticing the later literature of the subject; but decided to spare you from the infliction, and myself from the trouble of inflicting; though it may be convenient to add, in the form of an Appendix, a list of the chief papers on the subject that have been published since the question was discussed at length in 1889, in an official memoir on the geology of London, and to supply some omissions in that work. Nor do I propose to make any special criticism of papers on the subject that have appeared of late years; this is hardly the occasion for controversy, which may well be put off to a more convenient season. Some general remarks, however, I shall have to make after putting the facts before you.

There are 10 deep borings reaching to old rocks in the London Basin, of which accounts have been published. We find that in 4 of these (Meux's, Streatham, Richmond, and Dover) Jurassic beds separate those rocks from the Cretaceous beds; so that there are 6 in which these last rest direct on old rocks (Ware, Cheshunt, Kentish Town, Crossness, Culford, and Harwich). Stutton of course makes a seventh. The Jurassic rocks occur only in the southern borings, either in London or still further southward, and in one case only (Dover) is there any considerable thickness of these: in the other 3 they are from 38½ to 87½ feet thick. As far as regards Suffolk and its borders we may therefore disregard them, except in the far west, near their outcrop, and we may pass on to consider the older rocks that have been found.

So far the occurrence, next beneath the Cretaceous or Jurassic beds, of Silurian, Devonian, and Carboniferous rocks has been proved, whilst in some cases we are still doubtful as to the age of the old rocks found. In 5 cases distinctive fossils have been found (Ware, Cheshunt, Meux's, Dover, and Harwich), but in 5 others they have not (Kentish Town, Crossness, Richmond, Streatham, and Culford), and it is in the latter group too that the character of the beds leaves their age in doubt.

So far another must be added to these, as no fossil has yet been found in the old rocks at Stutton.

Of the above 10 deep borings in the London Basin (using that term in the widest sense, as including the Chalk tract that everywhere surrounds the Tertiary beds) we owe 9 to endeavours to get water from deep-seated rocks, and in addition to these 9 we have several other deep borings, which though not carried through to the base of the Secondary rocks, yet give us much information concerning those beds (at Holkham, Norwich, Combs, Winkfield, London, Loughton, Chatham, and Dover). In one case only, that of Dover, has the work been done for the purpose of exploration, but now, after a few years' interval, a second trial has been made at Stutton.

Now both of these borings were started for a much more definite object than merely to prove the depth to older rocks, or the thickness of the Cretaceous and Jurassic Series. There is one particular division of those older rocks that has a distinct fascination for others than geologists. We, happily, are content to find anything and to increase our knowledge in any direction, but naturally those who are not geologists, as well as many who are, like to find something of immediate practical value. As already shown, we owe much knowledge of the underground extension of formations to explorations for water; it has now become the turn of geologists to help those who would like to find that much less general, though nearly as needful and certainly more valuable thing, *coal.*

The first place to suggest itself to those geologists who had worked at this question, as a good site for trial, was the neighbourhood of Dover, and for various good reasons. The trial has been made, and successfully, several hundred feet of Coal Measures having been found, without reaching their base, but with several beds of workable coal.

Beyond that neighbourhood, however, geologists are not in such accord, and generally speaking, fairly good reasons can be given both for and against the selection of many tracts for trial, except in and near London, where no geologists would recommend it, from the evidence in our hands.

Let us then shortly review the evidence that we have on the underground extension of the older rocks in South-Eastern England, with a view of considering the question of the possibility of finding Coal Measures in any of the folds into which those rocks have probably, nay almost certainly, been thrown.

The area within which the borings that reach older rocks in the London Basin is enclosed is an irregular pentagon, from near Dover, on the south-east, to Richmond on the west, thence to Ware, thence to Culford on the north, thence to Harwich, and thence southward to Dover, the greatest distance between any borings being from Dover to Culford, about eighty-six miles. It is therefore over a large tract, extending of course beyond the boundaries sketched above, that we have good reason to infer that older rocks are within reasonable distance of the surface, nowhere probably as much as 1,600 feet, and mostly a good deal less.

We must now consider some evidence outside the tract hitherto dealt with. Southward of the central and eastern parts of the London Basin we have evidence that the Lower Cretaceous beds thicken greatly, from what is seen over their broad outcrop between the North and South Downs. We know also, from the Dover and Chatham borings, that the Upper and Middle Jurassic beds come in to the south-east, whilst the Sub-Wealden Exploration, near Battle, proves that those divisions thicken greatly southward, the latter not having been bottomed at the depth of over 1,900 feet, at that trial-boring.

Westward, however, near Burford in Oxfordshire, and some miles northward of the nearest part of the London Basin, Carboniferous rocks have been found at the depth of about 1,180 feet, these being separated from the thick Jurassic beds (including therein the Liassic and Rhætic) by perhaps 420 of Trias. They consist of Coal Measures, which were pierced to the depth of about 230 feet.

In and near Northampton, north-eastward of the last site, and still further from the northern edge of the London Basin, the like occurs; but the beds found are older than the Coal Measures, and the Trias is thin, not reaching indeed to

90 feet in thickness, and being absent in one case. At one place, too, the Carboniferous beds have been pierced through, with a thickness of only 222 feet, when Old Red Sandstone was found, and in another place still older rock seems to have been found next beneath the Trias. The depth to the rocks older than the Trias, where they were reached, was 677, 733. and 790 feet, or respectively 395, 460, and 316 below sea-level. Some of these figures must be taken as somewhat approximate, though they are near enough to the truth for practical purposes

A boring at Bletchley, to the south, reached granitic rocks at the depths of 378½ and 401 feet: but these rocks seem to be only boulders in a Jurassic clay: their occurrence, however, is suggestive of the presence of older rocks at the surface no great way off, in Middle Jurassic times.

Much further northward, at Scarle, south-west of Lincoln, the older rocks have been reached at the depth of about 1,500 feet, all but 141 of which are Trias, and they begin with the Permian (which crops out some eighteen miles westward), the Carboniferous occurring after another 400 feet, and having been pierced to 130.

We have then evidence that over a large part of South-Eastern England, reaching northward and westward of the London Basin, though the older rocks are hidden by a thick mantle of Jurassic, Cretaceous, and Tertiary beds, yet they seem to be rarely at a depth that would be called very great by the coalminer They are distinctly within workable depths wherever they have been reached.

There is no area of old rocks at the surface in our island, south of the Forth, in which Coal Measures are not a constituent formation. Truly, further north, in the great tract of Central and Northern Scotland there are no Carboniferous rocks; but we can hardly say that none ever occurred, at all events in the more southern parts. We know, though, that on the west and north Jurassic and Triassic beds rest on formations older than the Carboniferous.

It is not, however, to this more northern and distant tract that we should look for analogy to our underground plain of old rocks; rather should we look to more southern parts, to Wales and to Central and Northern England, where Coal Measures are of frequent occurrence. On the principle of reasoning from the known to the unknown, I cannot see why we should expect anything but a like occurrence of Coal Measures, in detached basins, in our vast underground tract of old rocks.

What, then, is the evident conclusion from what we know and from what we may reasonably infer? Surely that trials should be made to see if such hidden coal-basins can be found.

One trial has been made, and it has succeeded; the Dover boring has proved the presence of coal underground in Eastern Kent, along the line between the coal-fields of South Wales and of Bristol on the west, and those of Northern France and of Belgium on the east.

The long gap between the distant outcrops of the Coal Measures near Bristol and Calais has been lessened very slightly by the working of coal under the Triassic and Jurassic beds near the former place, but much more by our brethren across the narrow sea, the extent of the Coal Measures beneath the Jurassic and Cretaceous beds, having not only been proved by the French and the Belgians along their borders, but the coal having been largely worked. At last, we too have still further decreased the gap, by the Dover boring, a work that I trust is to be followed by other work along the same line.

But is this the only line along which we are to search? Are we to conclude that the only coal-fields under our great tract of Cretaceous beds (where these are either at the surface or covered by Tertiary beds) are in Kent, Surrey, and other counties to the west? Have we no coal-fields but those of Bristol and of South Wales? The bounds of our midland and northern coal-fields have been extended by exploration beneath the New Red Series; are we to stop here and to assume that there can be no further underground extension of the Coal Measures south-eastward? This seems hardly a wise course, and is certainly a very unenter-

prising one. It seems to me rather that the right thing to be done is to try to find out the real state of things, by means of borings.

There are of course objectors in this as in other matters. Some may say that it is silly to try in Suffolk, and that Essex gives a better chance of success. Others again may prefer Norfolk. And yet others may argue that there is no chance of finding Coal Measures in any of those three counties. But I must confess my inability to understand this line of reasoning; the fact is that the data we have are few and far between, and that we want more. It is really of little use to bandy words, and I do not now mean to take up the matter in detail. We cannot get at the truth except by actual work; justification by faith will not hold in this case, still less justification by unfaith.

Let us hark back a little and call to mind what has happened in the past. I remember the time when certain geologists disbelieved in the possibility of the occurrence of Coal Measures anywhere in South-Eastern England, it being argued that the formation thinned out before it could get so far eastward. Then this view was somewhat varied, and it was inferred, from certain observed facts, that even if Coal Measures did reach underground into these benighted parts, they would be without workable coal, and so practically useless.

Now for some years nothing occurred to upset the prophets of evil, that is to say, no fact came to light. There were not wanting inferences to the contrary, but it remained practically a matter of opinion. One day, however, the needful fact came, and the first boring made specially to test the question (at Dover) disproved both the above negative theories by finding Coal Measures with workable coal. Let us hope that a like result may happen in East Anglia, and that the pessimists may again be in the wrong.

We should not, however, fall into the opposite error, that of optimism. We must not expect an immediate success like that at Dover. We are here much further from any known coal-field. Advertisements of various wares sometimes tell us that ' one trial will suffice,' but it is not so in this case. We should not be content until many borings have been made, and we should not be despondent if, after sites have been selected to the best of our judgment, we begin with a set of borings that are unsuccessful in finding coal.

At the time of writing I cannot say that the Stutton boring is a success or a failure as far as coal is concerned, but I am quite ready to accept the latter without being discouraged. Whatever it is you may know during our meeting; it is certainly a success in the matter of reaching the old rocks at a depth of less than 1000 feet. We should remember that every boring is almost certain to give us some knowledge that may help in future work.

There is a further point, however, to be taken into account. A boring that may at first seem to be a failure, from striking beds older than the Coal Measures, may some day turn out otherwise. The coal-field along the borders of France and Belgium is sometimes affected by powerful and peculiar disturbances, by faults of comparatively gentle inclination (far removed from the usual more or less vertical displacements) which have thrown Coal Measures beneath older beds in large tracts. This is no mere theory, though advanced as such at first by some Continental geologists, who have had the great satisfaction of seeing their theory adopted by practical men, and proved to be true, much coal being worked below the older beds that have been pushed above the Coal Measures by the overthrust faults.

Our trial-work, of course, does not yet lead us to consider such disturbances as those alluded to. We have at first to assume a normal succession of formations, and not to carry on explorations in beds that can be proved to be older than the Coal Measures; but the time may come when it will be otherwise.

Another matter to which attention has been drawn by our foreign friends is an apparent general persistence of disturbances along certain lines, or in other words, the recurrence of disturbances in newer beds in those parts where earlier movements had affected older beds; so that, reasoning backward, where we see marked signs of disturbance for long distances in beds at or near the surface, there we may expect to find pre-existing disturbances of the older beds

beneath. This, however. is a somewhat controversial question, and much remains to be done on it ; but should it be proved as a general rule it may have much effect on our underground coal.

Finally, the question of the possibility of finding and of working coal in various parts of South-Eastern England is not merely of local interest ; it is of national importance. The time must come when the coal-fields that we have worked for years will be more or less exhausted, and we ought certainly to look out ahead for others, so as to be ready for the lessening yield of those that have served us so well. It is on our coal that our national prosperity largely, one may say chiefly, depends, and, as far as we can see, will depend. Let us not neglect any of the bounteous gifts of nature, but let us show rather that we are ready to search for the treasures that may be hidden under our feet, and the finding of which will result in the continued welfare of our native land.

APPENDIX.—*List of the Chief Papers on the Old Rocks Underground in South-Eastern England since* 1889, *when the literature of the subject was treated of in the Memoir on the Geology of London, &c.*

**Bertrand, Professor M.** Sur le Raccordement des Bassins houillers du Nord de la France et du Sud de l'Angleterre. *Annales des Mines* and *Trans Fed Inst Min Eng* , vol v (1893).

**Brady, F.** Dover Coal Boring. Observations on the Correlation of the Franco-Belgian, Dover and Somerset Coal Fields, 8vo. 1892. Second Issue, with Additions, 1893 Notice by E Lorieux in *Annales des Mines*, 1892.

**Dawkins, Professor W. B** The Discovery of Coal near Dover, *Nature*, vol. 41, pp 418, 419 ; *Iron and Coal Trades Gazette* ; *Contemporary Review*, vol. lvii., pp 470–478. The Search for Coal in the South of England, *Proc Roy. Inst.* (nine pages); *Nature*, vol 42, pp 319–322 The Discovery of Coal Measures near Dover, *Trans Manchester Geol Soc* , vol. xx , pp 502–517 (1890).

The Further Discovery of Coal at Dover and its Bearing on the Coal Question *Trans. Manchester Geol. Soc* . vol xxi , pp 456–474 (1892)

On the South-Eastern Coalfield at Dover, *Trans Manchester Geol. Soc* , vol. xxii , pp 488–510; The Probable Range of the Coal-Measures in Southern England, *Trans Fed Inst. Min Eng.*, vol. vii., 13 pages and plate (1894).

**Harrison, W. J** On the Search for Coal in the South-East of England ; With Special Reference to the Probability of the Existence of a Coal-field beneath Essex, 28 pages and plate 8vo. Birmingham (1894)

**Irving, Rev. Dr A** The Question of Workable Coal Measures beneath Essex *Herts and Essex Observer*, July 14, 1894

**Martin, E A.** On the Underground Geology of London. *Science Gossip*, no 335, pp 251–254 ; no 337, pp. 11–15 (1892, 1893).

**Rucker, Professor A W** , and **Professor T. E Thorpe.** Magnetic Survey of the British Isles, *Phil Trans*, vol 181, see pp 280 &c., and plate 14 (1891); A popular account by **Professor Rucker** under the title Underground Mountains, *Good Words*, January to March 1890

**Topley, W** Coal in Kent *Trans Fed Inst Min Eng* , vol. i , pp. 376–387 (1892)

**Whitaker, W** Coal in the South East of England, *Journ Soc Arts.*, vol xxxviii , pp 543-557 ; Suggestions on Sites for Coal-search in the South East of England, *Geol Mag* , dec iii , vol. vii , pp 514–516 (1890)

**Whitaker, W**, and **A. J Jukes-Browne** On Deep Borings at Culford and Winkfield, with Notes on those at Ware and Cheshunt. *Quart Journ Geol. Soc.*, vol. 1, pp. 488–514 (1894)

The Eastern Counties' Coal Boring and Development Syndicate . . . Geological Reports by T V Holmes, J. E Taylor and W. Whitaker, 15 pages, 8vo Ipswich (1893) Partly reprinted in *Essex Naturalist*

*Omitted from Notice in 1889.*

**Drew, F.** Is there Coal under London? *Science for All,* vol v. pp. 324–328

**Firket, A** Sur l'Extension en Angleterre du Bassin houiller Franco-Belge *Ann. Soc. Géol Belg* t. x. *Bulletin,* pp xcii–xciv (1883)

**Taylor, W.** On the Probability of Finding Coal in the South-east of England, pp. ii , 22, 8vo Reigate (1886).

**Topley, W.** On the Correspondence between some Areas of Apparent Upheaval and the Thickening of Subjacent Beds. *Quart Journ Geol Soc.* vol xxx see pp 186, 190–195 (1874). See also Memoir ' The Geology of the Weald,' pp 241, 242, pl. vi. (1875)

# 𝔅ritish 𝔄ssociation for the 𝔄dvancement of 𝔖cience.

## IPSWICH, 1895.

# ADDRESS

TO THE

# ZOOLOGICAL SECTION

BY

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PRESIDENT OF THE SECTION.

THIS year, for the first time in the history of the British Association, Section D meets without including in the range of its subject-matter the Science of Botany. Zoology now remains as the sole occupant of Section D—that 'Fourth Committee of Sciences,' as it was at first called, more than sixty years ago, when our subject was one of that group of biological sciences, the others being Botany, Physiology, and Anatomy These allied sciences have successively left us Like a prolific mother our Section has given rise one after another to the now independent Sections of Anthropology, Physiology, and Botany. Our subject-matter has been greatly restricted in scope, but it is still very wide—this year, when Section I devoted to the more special physiology of the medical physiologist does not meet, perhaps a little wider than it may be in other years, since we are on this occasion credited with the subject 'Animal Physiology'—surely *always* an integral part of Zoology! It is to be hoped that this section will always retain that general and comparative physiology which is inseparable from the study of animal form and structure. The late Waynflete Professor of Physiology at Oxford, in his Newcastle Address to this Section, said 'that every appreciable difference in structure corresponds to a difference of function,'[1] and his successor, the present Waynflete Professor, has shown us 'how pointless is structure apart from function, and how baseless and unstable is function apart from structure'[2]—the 'argument for the simultaneous examination of both' in that science of Zoology which we profess is, to my mind, irresistible.

We include also in our subject-matter, besides the adult structure and the embryonic development of animals, their distribution both in space and time, the history and structure of extinct forms, speciography and classification, the study of the habits of animals and all that mass of lore and philosophy which has gathered around inquiries into instinct, breeding, and heredity. I trust that the discussion of matters connected with Evolution will always, to a large extent, remain with this Section D, which has witnessed in the past the addresses, papers, discussions, and triumphs of Darwin, Huxley, and Wallace.

When the British Association last met in Ipswich, in 1851, Section D, under the Presidency of Professor Henslow, still included Zoology, Botany, and Physiology, and a glance through the volumes of reports for that and neighbouring years

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[1] Burdon-Sanderson—*British Association Report* for 1889
[2] Gotch—Presidential Address to *Liverpool Biological Society*, vol. ix 1894

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results to us that our subject has undergone great and striking developments in the forty-four years that have elapsed  Zoology was still *pre-Darwinian* (though Charles Darwin was then in the thick of his epoch-making work—both what he calls his 'plain barnacle work' and his 'theoretic species work.')[1]  Although the cell-theory had been launched a decade before, zoologists were not yet greatly concerned with those minute structural details which have since built up the science of Histology.  The heroes of our science were then chiefly those glorious field naturalists, observers, and systematists who founded and established on a firm basis British Marine Zoology.  Edward Forbes, Joshua Alder, Albany Hancock were then in active work.  George Johnston was at his zoophytes, Bowerbank at sponges, Busk at polyzoa  Forbes' short brilliant career was nearly run.  He probably did more than any of his contemporaries to advance marine zoology  In the previous year, at the Edinburgh meeting of the Association, he and his friend McAndrew, had read their classic reports,[2] 'On the Investigation of British Marine Zoology by means of the Dredge,' and 'On South European Marine Invertebrata,' which mark the high water level reached at that date, and for some time afterwards, in the exploration of our coasts and the explanation of the distribution of our marine animals.  At the Belfast meeting, which followed Ipswich, Forbes exhibited his great map of the distribution of marine life in 'Homoiozoic Belts.'  In November, 1854, he was dead, six months after his appointment to the goal of his ambition, the professorship at Edinburgh, where, had he lived, there can be no doubt he would, with his brilliant ability and unique personality, have founded a great school of Marine Zoology

To return to the early fifties, Huxley—whose recent loss to science, to philosophy, to culture, we, in common with the civilised world, now deplore—at that time just returned from the memorable voyage of the 'Rattlesnake,' was opening out his newly acquired treasures of comparative anatomy with papers on Siphonophora and on Sagitta, and one on the structure of Ascidians, in which he urged—fourteen years before Kowalevsky established it on embryological evidence in 1866—that their relations were with Amphioxus, as we now believe, rather than with the Polyzoa or the Lamellibranchiata, as had formerly been supposed  Bates was then on the Amazons, Wallace was just going out to the Malay Archipelago, Wyville Thomson, Hincks, and Carpenter, the successors of Forbes, Johnston, and Alder, were beginning their life-work.  Abroad that great teacher and investigator, Johannes Müller, was training amongst his pupils the most eminent zoologists, anatomists, and physiologists of the succeeding quarter century.  In this country, as we have seen, Huxley was just beginning to publish that splendid series of researches into the structure of nearly all groups in the animal kingdom, to which comparative anatomy owes so much

In fact, the few years before and after the last Ipswich meeting witnessed the activity of some of the greatest of our British zoologists—the time was pregnant with work which has since advanced, and in some respects revolutionised our subject.  It was then still usual for the naturalist to have a competent knowledge of the whole range of the natural sciences.  Edward Forbes, for example, was a botanist and a geologist, as well as a zoologist.  He occupied the chair of Botany at King's College, London, and the presidential chair of the Geological Section of the British Association at Liverpool in 1854  That excessive specialisation, from which most of us suffer in the present day, had not yet arisen ; and in the comprehensive, but perhaps not very detailed, survey of his subject taken by one of the field naturalists of that time, we find the beginnings of different lines of work, which have since developed into some half-dozen distinct departments of zoology, are now often studied independently, and are in some real danger of losing touch with one another (see diagram)

The splendid anatomical and 'morphological' researches of Huxley and Johannes Müller have been continued by the more minute histological or cellular work rendered possible by improvements of the microtome and the microscope,

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[1] See *Life and Letters*, vol. i. p 380.
[2] *British Association Report* for 1850, p. 192—*et seq.*

until at last in these latter years we investigate not merely the cellular anatomy of the body, but *the anatomy of the cell*—if indeed we are permitted to talk of 'cell' at all, and are not rather constrained to express our results in terms of 'cytomicrosomes,' 'somacules,' or 'idiosomes,' and to regard our morphological unit, the cell, as a symbiotic community containing two colonies of totally dissimilar organisms.[1] To such cytological investigations may well be applied Lord Macaulay's aphorism, 'A point which yesterday was invisible is its goal to-day, and will be its starting point to-morrow.'

Somewhat similar advances in methods have led us from the life-histories studied of old to the new and fascinating science of embryology. The elder Milne-Edwards and Van Beneden knew that in their life-histories Ascidians produced tadpole-like young. Kowalevsky (1866) showed that in their embryonic stages these Ascidian tadpoles have the beginnings of their chief systems of organs formed in essentially the same manner and from the same embryonic layers as in the case of the frog's tadpole or any other typical young vertebrate, and now we are not content with less than tracing what is called the 'cell-lineage' of such Ascidian embryos, so as to show the ancestry and descendants, the traditions, peculiarities of, and influences at work upon each of the embryonic cells—or areas of protoplasm—throughout many complicated stages. And there is now opening
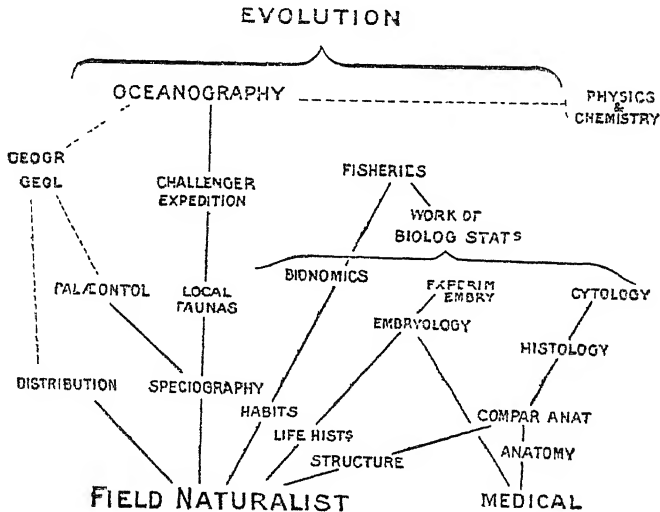


up from this a great new field of experimental and 'mechanical' embryology, in which we seek the clue to the explanation of particular processes and changes by determining under what conditions they take place, and how they are affected by altered conditions. We are brought face to face with such curious problems as, Why does a frog's egg, in the two-celled stage, of which one half has been destroyed, develop into half an embryo when it is kept with one (the black) surface uppermost, and into—not half an embryo, but—a whole embryo of *half the usual size* if kept with the other (the white) surface upwards. Apparently, according to the conditions of the experiment, we may get half embryos or whole embryos of half size from one of the first two cells of the frog's egg.[2]

One of the most characteristic studies of the older field naturalists, the observation of habits, has now become, under the influence of Darwinism, the 'Biono-

[1] See Watasé in *Wood's Holl Biological Lectures*, 1893

[2] See Morgan, *Anat Anzeig*, 1895, x. Bd. p 623, and recent papers by Roux, Hertwig, Born, and O. Schultze.

lines of the present day, the study of the relations between habit and structure and environment—a most fascinating and promising field of investigation, which may be confidently expected to tell us much in the future in regard to the competition between species, and the useful or indifferent nature of specific characters.

Other distinct lines of zoological investigation, upon which I shall not dwell, are geographical distribution and palæontology—subjects in which the zoologist comes into contact with, and may be of some service to his fellow-workers in geology And there still remains the central avenue of the wide zoological domain—that of speciography and systematic zoology—which has been cultivated by the great classifiers and monographers from Linnæus to Hæckel, and has culminated in our times in the magnificent series of fifty quarto volumes, setting forth the scientific results of the 'Challenger' Expedition , a voyage of discovery comparable only in its important and wide-reaching results with the voyages of Columbus, Gama, and Magellan at the end of the fifteenth century. It is now so long since the 'Challenger' investigations commenced that few I suppose outside the range of professional zoologists are aware that although the expedition took place in 1872 to 1876, the work resulting therefrom has been going on actively until now—for nearly a quarter of a century in all—and in a sense, and a very real one, will never cease, for the 'Challenger' has left an indelible mark upon science, and will remain through the ages exercising its powerful, guiding influence, like the work of Aristotle, Newton, and Darwin.

Most of the authors of the special memoirs on the sea and its various kinds of inhabitants, have interpreted in a liberal spirit the instructions they received to examine and describe the collections entrusted to them, and have given us very valuable summaries of the condition of our knowledge of the animals in question, while some of the reports are little less than complete monographs of the groups. I desire to pay a tribute of respect to my former teacher and scientific chief, Sir Wyville Thomson, to whose initiative, along with Dr. W. B Carpenter, we owe the first inception of our now celebrated deep-sea dredging expeditions, and to whose scientific enthusiasm, combined with administrative skill, is due in great part the successful accomplishment of the 'Lightning,' the 'Porcupine,' and the 'Challenger' Expeditions. Wyville Thomson lived long enough to superintend the first examination of the collections brought home, their division into groups, and the allotment of these to specialists for description. He enlisted the services of his many scientific friends at home and abroad, he arranged the general plan of the work, decided upon the form of publication, and died in 1882 after seeing the first ten or twelve zoological reports through the press.

Within the last few months have been issued the two concluding volumes of this noble series, dealing with a summary of the results, conceived and written in a masterly manner by the eminent editor of the reports, Dr John Murray. An event of such first-rate importance in zoology as the completion of this great work ought not to pass unnoticed at this zoological gathering. I desire to express my appreciation and admiration of Dr. Murray's work, and I do not doubt that the Section will permit me to convey to Dr Murray the congratulations of the zoologists present, and their thanks for his splendid services to science Murray, in these 'Summary' volumes, has given definiteness of scope and purpose, and a tremendous impulse, to that branch of science—mainly zoological—which is coming to be called

## OCEANOGRAPHY.

Oceanography is the meeting ground of most of the sciences It deals with botany and zoology, ' including animal physiology ; ' chemistry, physics, mechanics, meteorology, and geology all contribute, and the subject is of course intimately connected with geography, and has an incalculable influence upon mankind, his distribution, characteristics, commerce, and economics. Thus oceanography, one of the latest developments of marine zoology, extends into the domain of, and ought to find a place in, every one of the sections of the British Association.

Along with the intense specialisation of certain lines of zoology in the last quarter of the nineteenth century, it is important to notice that there are also lines

of investigation which require an extended knowledge of, or at least make use of the results obtained from, various distinct subjects. One of these is oceanography, another is bionomics, which I have referred to above, a third is the philosophy of zoology, or all those studies which bear upon the theory of evolution, and a fourth is the investigation of practical fishery problems—which is chiefly an application of marine zoology. Of these four subjects—which while analytic enough in the detailed investigation of any particular problem, are synthetic in drawing together and making use of the various divergent branches of zoology and the neighbouring sciences—oceanography, bionomics, and the fisheries' investigation, are most closely related, and I desire to devote the remainder of this Address to the consideration of some points in connection with their present position.

Dr. Murray, in a few only too brief paragraphs at the end of his detailed summary of the results of the 'Challenger' Expedition, which I have alluded to above, states some of the views, highly suggestive and original, at which he has himself arrived from his unique experience. Some of his conclusions are very valuable contributions to knowledge, which will no doubt be adopted by marine zoologists Others, I venture to think, are less sound and well founded, and will scarcely stand the test of time and further experience. But for all such statements, or even suggestions, we should be thankful. They do much to stimulate further research, they serve, if they can neither be refuted nor established, as working hypotheses; and even if they have to be eventually abandoned, we should bear in mind what Darwin has said as to the difference in their influence on science between erroneous facts and erroneous theories. 'False facts are highly injurious to the progress of science, for they often endure long; but false views, if supported by some evidence, do little harm, for everyone takes a salutary pleasure in proving their falseness, and when this is done, one path towards error is closed, and the road to truth is often at the same time opened'[1]

With all respect for Murray's work, and fully conscious of my own temerity in venturing to differ from one who has had such an extended experience of the sea and its problems, I am constrained to express my disagreement with some of his conclusions. And I am encouraged to do so by the belief that Murray will rightly feel that the best compliment which zoologists can pay to his work is to give it careful, detailed consideration, and discuss it critically. He will, I am sure, join me in the hope that, whether his views or mine prove the false ones, we may be able, by their discussion, to close a 'path towards error,' and possibly open 'the road to truth.'

One of the points upon which Murray lays considerable stress, and to the elaboration of which he devotes a prominent position in his 'General Observations on the Distribution of Marine Organisms,' is the presence of what he has called a 'mud-line' around coasts at a depth of about one hundred fathoms. It is the point 'at which minute particles of organic and detrital matters in the form of mud begin to settle on the bottom of the ocean.' He regards it as the great feeding ground, and a place where the fauna is most abundant, and from which there have hived off, so to speak, the successive swarms or migrations which have peopled other regions—the deep waters, the open sea, the shallow waters and the estuaries, fresh waters, and land Murray thus gives to his mud-line both a present and an historic importance which can scarcely be surpassed in the economy of life on this globe. I take it that the historic and the present importance stand or fall together—that the evidence as to the origin of faunas in the past is derived from their distribution at the present day, and I am inclined to think that Murray's opinion as to the distribution of animals in regard to the mud-line is not entirely in accord with the experience of specialists, and is not based upon reliable statistics Murray's own statement is[2]:—'A depth is reached along the Continental shores facing the great oceans immediately below which the conditions become nearly uniform in all parts of the world, and where the fauna likewise presents a great uniformity. This depth is usually not far above nor far below the 100-fathom

[1] Darwin, *The Descent of Man*, second edit 1882, p 606
[2] *Chall Exped*, Summary, vol ii., p 1,433.

line, and is marked out by what I have elsewhere designated as the *Mud-line* . . .'
' Here is situated the great feeding ground in the ocean . . .' and he then goes on
(page 1.434) to enumerate the Crustaceans, such as species of *Calanus, Euchæta,
Pasiphæa, Crangon, Calocaris, Pandalus, Hippolyte,* many amphipods, isopods,
and immense numbers of schizopods, which swarm, with fishes and cephalopods,
immediately over this mud deposit. Now I venture to think that the experience of
some of those who have studied the marine zoology of our own coasts does not bear out
this statement. In the first place, our experience in the Irish Sea is that mud may
be found at almost any depth, but is very varied in its nature and in its source.
There may even be mud laid down between tide marks in an estuary where a very
considerable current runs  A deposit of mud may be due to the presence of an eddy
or a sheltered corner in which the finer particles suspended in the water are able to
sink, or it may be due to the wearing away of a limestone beach, or to quantities
of alluvium brought down by a stream from the land, or to the presence of a sub-
merged bed of boulder clay, or even, in some places, to the sewage and refuse from
coast towns  Finally, there is the deep water mud, a very stiff blue-grey substance
which sets, when dried, into a firm clay, and this is, I take it, the mud of which
Dr. Murray writes  But in none of these cases, and certainly not in the last men-
tioned, is there in my experience or in that of several other naturalists I have
consulted. any rich fauna associated with the mud  In fact, I would regard mud
as supporting a comparatively poor fauna as compared with other shallow water
deposits.

For practical purposes, round our own British coasts, it is still convenient to
make use of the zones of depth marked out by Forbes.  The first of these is the
' Littoral zone,' the space between tide marks, characterised by the abundance of sea-
weeds, belonging to the genera *Lichina, Fucus, Enteromorpha, Polysiphonia,* and
others, and by large numbers of individuals belonging to common species of
*Balanus, Mytilus, Littorina, Purpura,* and *Patella* amongst animals  The second
zone is the ' Laminarian,' which extends from low water mark to a depth of a few
fathoms, characterised by the abundant growth of large sea-weeds belonging to
the genera *Laminaria, Alaria,* and *Himanthalia,* and by the presence of the
beautiful red sea-weeds (Florideæ).  There is abundance of vegetable food, and
animals of all groups swarm in this zone, the numbers both of species and of
individuals being very great.  The genera *Helcion, Trochus,* and *Lacuna* are
characteristic molluscan forms in our seas.  Next comes Forbes' ' Coralline' zone,
badly so named, extending from about ten to forty or fifty fathoms or so.  Here
we are beyond the range of the ordinary sea-weeds, but the calcareous, coral-like
Nullipores are present in places in such abundance as to make up deposits covering
the floor of the sea for miles.  Hydroid zoophytes and polyzoa are also abundant,
and it is in this zone that we find the shell-beds lying off our coasts, produced by
great accumulations of species of *Pecten, Ostrea, Pectunculus, Fusus* and *Buccinum,*
and forming rich feeding grounds for many of our larger fishes  All groups of
marine animals are well represented in this zone, and *Antedon, Ophiothrix, Ophio-
glypha. Ebalia, Inachus,* and *Eurynome,* may be mentioned as characteristic genera.
Lastly, there is what may be appropriately called the zone of deep mud (although
Forbes did not call it so), extending from some fifty fathoms down to (in our seas)
one hundred or so.  The upper limit of this zone is Murray's mud-line  We come
upon it in the deep fjord-like sea-lochs on the west of Scotland, and in the Irish
Sea to the west of the Isle of Man.

Now of these four zones, my experience is that the last—that of the deep mud
—has by far the poorest fauna both in species and in individuals  The mud has a
*peculiar* fauna and one of great interest to the zoologist, but it is not a *rich* fauna.
It contains some rare and remarkable animals not found elsewhere, such as *Calo-
caris macandreæ, Panthalis oerstedi, Lipobranchius jeffreysi, Brissopsis lyrifera,
Amphiura chiajii, Isocardia cor,* and *Sagartia herdmani*; and a few striking novel-
ties have been described from it of late years, but we have no reason to believe
that the number of these is great compared with the number of animals obtained
from shallower waters

Dr. Murray not only insists upon the abundance of animals on the mud, and its

importance as the great feeding ground and place of origin of life in the ocean, but he also (p 1,432) draws conclusions as to the relative numbers of animals taken by a single haul of the trawl in deep and shallow waters which can scarcely be received, I think, by marine zoologists without a protest. His statement runs (p. 1,432): 'It is interesting to compare single hauls made in the deep sea and in shallow water with respect to the number of different species obtained. For instance, at station 146 in the Southern Ocean, at a depth of 1,375 fathoms the 200 specimens captured belonged to 59 genera and 78 species' That was with a 10 ft trawl dragged for at most two miles during at most two hours Murray then goes on to say: 'In depths less than 50 fathoms, on the other hand, I cannot find in all my experiments any record of such a variety of organisms in any single haul even when using much larger trawls and dragging over much greater distances.' He quotes the statistics of the Scottish Fishery Board's trawlings in the North Sea, with a 25 ft trawl, to show that the average catch is 7 3 species of invertebrata and 8 3 species of fish, the greatest number of both together recorded in one haul being 29 species Murray's own trawlings in the West of Scotland gave a much greater number of species, sometimes as many as 50, 'still not such a great variety of animals as was procured in many instances by the " Challenger's " small trawl in great depths.'

Now, in the first place, it is curious that Murray's own table on p 1,437, in which he shows that the ' terrigenous' deposits lying along the shore-lines yield many more animals, both specimens and species, per haul, than do the ' pelagic ' deposits[1] at greater depths, such as red clays and globigerina oozes, seems directly opposed to the conclusion quoted above. In the second place, I am afraid that Dr Murray has misunderstood the statistics of the Scottish Fishery Board when he quotes them as showing that only 7 3 or so species of invertebrates are brought up, on the average, in the trawl net 1 happen to know from Mr. Thomas Scott, F.L.S., the naturalist who has compiled the statistics in question, and also from my own observations when on board the ' Garland ' on one of her ordinary trawling expeditions, that the invertebrata noted down on the station sheet are merely a few of the more conspicuous or in other ways noteworthy animals No attempt is made—nor could possibly be made in the time—by the one naturalist who has to attend to tow-nets, water bottle, the kinds, condition, food, &c., of the fish caught and other matters—to give anything like a complete or even approximate list of the species, still less the number of individuals, brought up in the trawl I submit, therefore, that it is entirely misleading to compare those Scottish Fishery Board statistics, which were not meant for such a purpose but only to give a rough idea of the fauna associated with the fish upon certain grounds, with the carefully elaborated results, worked out at leisure by many specialists in their laboratories, of a haul of the ' Challenger's' trawl. Of Dr Murray's own trawlings in the West of Scotland I cannot, of course, speak so positively, but I shall be surprised to learn that the results of each haul were as carefully preserved and as fully worked out by specialists as were the ' Challenger ' collections

Lastly, on the next L.M.B.C.[2] dredging expedition in the Irish Sea after the appearance of Dr Murray's volumes, I set myself to determine the species taken in a haul of the trawl for comparison with the ' Challenger' numbers The haul was taken on June 23, at 7 miles west from Peel, on the north bank, bottom sand and shells, depth 21 fathoms, with a trawl of only 4 ft beam, less than half the size

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[1] One of the earliest of the · Challenger' oceanographic results, the classification of the submarine deposits into · terrigenous' and ' pelagic,' seems inadequate to represent fully the facts in regard to sea-bottoms, so I am proposing elsewhere (*Report of Irish Sea Committee*) the following amended classification ·—(1) Terrigenous (Murray), where the deposit is formed chiefly of mineral particles derived from the waste of the land ; (2) Neritic, where the deposit is chiefly of organic origin, and is derived from the shells and other hard parts of the animals and plants living on the bottom ; (3) Planktonic (Murray's ' pelagic'), where the greater part of the deposit is formed of the remains of free-swimming animals and plants which lived in the sea over the deposit

[2] Liverpool Marine Biology Committee

of the 'Challenger' one, and it was not down for more than twenty minutes I noted down the species observed, and I filled two bottles with undetermined stuff which my assistant. Mr. Andrew Scott, and I examined the following day in the laboratory. Our list comes to at least 112 species, belonging to at least 103 genera.[1]   I counted 120 duplicate specimens which, added to 112, gives 232 individuals, but there may well have been 100 more   This experience, then. is very different from Murray's, and gives far larger numbers in every respect—specimens, species, and genera—than even the 'Challenger' deep-water haul quoted.   I append my list of species,[2] and practised marine zoologists will, I think, see at a glance that it is nothing out of the way, that it is a fairly ordinary assemblage of not uncommon animals such as is frequently met with when dredging in the 'coralline' zone.   I am sure that I have taken better netfuls than this both in the Irish Sea and on the West of Scotland.

In order to get another case on different ground, not of my own choosing, on the first occasion after the publication of Dr Murray's volumes when I was out witnessing the trawling ob-ervations of the Lancashire Sea Fisheries steamer ' John Fell,' I counted, with the help of my assistant, Mr. Andrew Scott and the men on board, the results of the first haul of the shrimp trawl. It was taken at the mouth of the Mersey estuary, inside the Liverpool bar, on what the naturalist would consider very unfavourable ground, with a bottom of muddy sand, at a depth of 6 fathoms. The shrimp trawl (1½ in mesh) was down for one hour, and it brought up over seventeen thousand specimens referable to at least 39 species,[3] belonging to 34 genera. These numbers have been exceeded on many other hauls taken in the ordinary course of work by the Fisheries steamer in Liverpool Bay—for example, on this occasion the fish numbered 5,943, and I have records of hauls on which the fish numbered over 20,000, and the total catch of individual animals must have been nearly 50,000.   Can any of Dr. Murray's hauls on the deep mud beat these figures?

The conclusion, then, at which I arrive in regard to the distribution of animals in deep water and in water shallower than 50 fathoms, from my own experience and an examination of the 'Challenger' results, is in some respects the reverse of Murray's.   I consider that there are more species and more individuals in the shallower waters, that the deep mud as dredged has a poor fauna, that the ' Coralline ' zone has a much richer one, and that the ' Laminarian ' zone, where there is vegetable as well as animal food, has probably the richest of all.

In order to come to as correct a conclusion as possible on the matter I have consulted several other naturalists in regard to the smaller groups of more or less free-swimming Crustacea, such as Copepoda and Ostracoda, which I thought might possibly be in considerable numbers over the mud.   I have asked three well-known specialists on such Crustaceans—viz., Professor G. S. Brady, F.R.S., Mr. Thomas

[1] It is interesting, in connection with Darwin's opinion that an animal's most formidable competitors in the struggle for existence are those of its own kind or closely allied forms, to notice the large proportion of genera to species in such hauls I have noticed this in many lists, and it certainly suggests that closely related forms are comparatively rarely taken together

[2] See *Appendix*, p 16.

| | | |
|---|---|---|
| *Solea vulgaris* | *Mytilus edulis* | *Dactylopus rostratus* |
| *Pleuronectes platessa* | *Tellina tenuis* | *Cletodes limicola* |
| *P limanda* | *Mactra stultorum* | *Caligus*, sp |
| *Gadus morrhua* | *Fusus antiquus* | *Flustra foliacea* |
| *G. œglefinus* | *Carcinus mœnas* | *Aphrodite aculeata* |
| *G merlangus* | *Portunus*, sp | *Pectinaria belgica* |
| *Clupea spratta* | *Eupagurus bernhardus* | *Nereis*, sp |
| *C. harengus* | *Crangon vulgaris* | *Asterias rubens* |
| *Trachinus vipera* | *Sacculina*, sp | *Hydractinia echinata* |
| *Agonus cataphractus* | Some Amphipoda | *Sertularia abietina* |
| *Gobius minutus* | *Longipedia coronata* | *Hyarallmania falcata* |
| *Raia clavata* | *Ectinosoma spinipes* | *Aurelia aurita* |
| *R maculata* | *Sunaristes paguri* | *Cyanæa*, sp. |

Scott, F.L S., and Mr. I. C. Thompson, F.L.S.—and they all agree in stating that, although interesting and peculiar, the Copepoda and Ostracoda from the deep mud are not abundant either in species or in individuals. In answer to the question which of the three regions (1) the littoral zone, (2) from low water to 20 fathoms, and (3) from 20 fathoms onwards, is richest in small free-swimming, but bottom-haunting, Crustacea, they all replied the middle region from 0 to 20 fathoms, which is the Laminarian zone and the upper edge of the Coralline. Professor Brady assures me that nearly every other kind of bottom and locality is better than mud for obtaining Ostracoda. Mr. T. Scott considers that Ostracoda are most abundant in shallow water, from 5 to 20 fathoms. He tells me that as the result of his experience in Loch Fyne, where a great part of the loch is deep, the richest fauna is always where banks occur, coming up to about 20 fathoms, and having the bottom formed of sand, gravel, and shells. The fauna on and over such banks, which are in the Coralline zone, is much richer than on the deeper mud around them. On an ordinary shelving shore on the west coast of Scotland Mr Scott, who has had great experience in collecting, considers that the richest fauna is usually at about 20 fathoms. My own experience in dredging in Norway is the same. In the centre of the fjords in deep water on the mud there are rare forms, but very few of them, while in shallower water at the sides, above the mud, on gravel, shells, rock, and other bottoms, there is a very abundant fauna.

Probably no group of animals in the sea is of so much importance from the point of view of food as the Copepoda. They form a great part of the food of whales, and of herrings and many other useful fish, both in the adult and in the larval state, as well as of innumerable other animals, large and small. Consequently, I have inquired somewhat carefully into their distribution in the sea, with the assistance of Professor Brady, Mr. Scott, and Mr. Thompson. These experienced collectors all agree that Copepoda are most abundant, both as to species and individuals, close round the shore, amongst seaweeds, or in shallow water in the Laminarian zone over a weedy bottom    Individuals are sometimes extremely abundant on the surface of the sea amongst the plankton, or in shore pools near high water, where, amongst *Enteromorpha,* the Harpacticidæ swarm in immense profusion , but, for a gathering rich in individuals, species, and genera, the experienced collector goes to the shallow waters of the Laminarian zone. In regard to the remaining, higher, groups of the Crustacea my friend, Mr. Alfred O. Walker, tells me that he considers them most abundant at depths of 0 to 20 fathoms.

I hope no one will think that these are detailed matters interesting only to the collector, and having no particular bearing upon the great problems of biology. The sea is admittedly the starting-point of life on this earth, and the conclusions we come to as to the distribution of life in the different zones must form and modify our views as to the origin of the faunas—as to the peopling of the deep sea, the shallow waters, and the land. Murray supposes that life started in Pre-Cambrian times on the mud, and from there spread upwards into shallower waters, outwards on to the surface, and, a good deal later, downwards to the abysses by means of the cold Polar waters. The late Professor Moseley considered the pelagic, or surface life of the ocean to be the primitive life from which all the others have been derived. Professor W. K Brooks [1] considers that there was a primitive pelagic fauna, consisting of the simplest microscopic plants and animals, and 'that pelagic life was abundant for a long period during which the bottom was uninhabited.'

I, on the other hand, for the reasons given fully above, consider that the Laminarian zone close to low-water mark is at present the richest in life, that it probably has been so in the past, and that if one has to express a more definite opinion as to where, in Pre-Cambrian times, life in its simplest forms first appeared, I see no reason why any other zone should be considered as having a better claim than what is now the Laminarian to this distinction. It is there, at present at any rate, in the upper edge of the Laminarian zone, at the point of junction of sea, land, and air, where there is a profusion of food, where the materials brought down by streams or worn away from the land are first deposited, where the animals are

[1] *The genus Salpa* 1893, p. 156, etc

able to receive the greatest amount of light and heat, oxygen and food, without being exposed periodically to the air, rain, frost, sun, and other adverse conditions of the littoral zone, it is there that life—it seems to me—is most abundant, growth most active, competition most severe.    It is there, probably, that the surrounding conditions are most favourable to animal life; and, therefore, it seems likely that it is from this region that, as the result of over-crowding, migrations have taken place downwards to the abysses, outwards on the surface, and upwards on to the shore.    Finally, it is in this Laminarian zone, probably, that under the stress of competition between individuals and between allied species evolution of new forms by means of natural selection has been most active.    Here, at any rate, we find, along with some of the most primitive of animals, some of the most remarkably modified forms, and some of the most curious cases of minute adaptation to environment.    This brings us to the subject of

## BIONOMICS,

which deals with the habits and variations of animals, their modifications, and the relations of these modifications to the surrounding conditions of existence.

It is remarkable that the great impetus given by Darwin's work to biological investigation has been chiefly directed to problems of structure and development, and not so much to bionomics until lately.    Variation amongst animals in a state of nature is, however, at last beginning to receive the attention it deserves.    Bateson has collected together, and classified in a most useful book of reference, the numerous scattered observations on variation made by many investigators, and has drawn from some of these cases a conclusion in regard to the discontinuity of variation which many field zoologists find it hard to accept.

Weldon and Karl Pearson have recently applied the methods of statistics and mathematics to the study of individual variation.    This method of investigation, in Professor Weldon's hands, may be expected to yield results of great interest in regard to the influence of variations in the young animal upon the chance of survival, and so upon the adult characteristics of the species.    But while acknowledging the value of these methods, and admiring the skill and care with which they have been devised and applied, I must emphatically protest against the idea which has been suggested, that only by such mathematical and statistical methods of study can we successfully determine the influence of the environment on species, gauge the utility of specific characters, and throw further light upon the origin of species.    For my part, I believe we shall gain a truer insight into those mysteries which still involve variations and species by a study of the characteristic features of individuals, varieties, and species in a living state in relation to their environment and habits.    The mode of work of the old field naturalists, supplemented by the apparatus and methods of the modern laboratory, is, I believe, not only one of the most fascinating, but also one of the most profitable fields of investigation for the philosophical zoologist.    Such studies must be made in that modern outcome of the growing needs of our science, the Zoological Station, where marine animals can be kept in captivity under natural conditions, so that their habits may be closely observed, and where we can follow out the old precept—first, Observation and Reflection ; then Experiment.

The biological stations of the present day represent, then, a happy union of the field work of the older naturalists with the laboratory work of the comparative anatomist, histologist, and embryologist.    They are the culmination of the ' Aquarium ' studies of Kingsley and Gosse, and of the feeling in both scientific men and amateurs, which was expressed by Herbert Spencer when he said : ' Whoever at the seaside has not had a microscope and an aquarium has yet to learn what the highest pleasures of the seaside are.'    Moreover, I feel that the biological station has come to the rescue, at a critical moment, of our laboratory worker who, without its healthy, refreshing influence, is often in these latter days in peril of losing his intellectual life in the weary maze of microtome methods and transcendental cytology.    The old Greek myth of the Libyan giant, Antæus, who wrestled with Hercules and regained his strength each time he touched his mother

earth, is true at least of the zoologist. I am sure he derives fresh vigour from every direct contact with living nature.

In our tanks and artificial pools we can reproduce the Littoral and the Laminarian zones, we can see the methods of feeding and breeding—the two most powerful factors in influencing an animal. We can study mimicry, and test theories of protective and warning colouration

The explanations given by these theories of the varied forms and colours of animals were first applied by such leaders in our science as Bates, Wallace, and Darwin, chiefly to insects and birds, but have lately been extended, by the investigations of Giard, Garstang, Clubb, and others, to the case of marine animals. I may mention very briefly one or two examples. Amongst the Nudibranchiate Mollusca—familiar animals around most parts of our British coasts—we meet with various forms which are edible, and, so far as we know, unprotected by any defensive or offensive apparatus Such forms are usually shaped or coloured so as to resemble more or less their surroundings, and so become inconspicuous in their natural haunts *Dendronotus arborescens*, one of the largest and most handsome of our British Nudibranchs, is such a case The large, branched processes on its back, and its rich purple-brown and yellow markings, tone in so well with the masses of brown and yellow zoophytes and purplish-red seaweeds, amongst which we usually find *Dendronotus*, that it becomes very completely protected from observation, and, as I know from my own experience, the practised eye of the naturalist may fail to detect it lying before him in the tangled forests of a shore-pool.

Other Nudibranchs, however, belonging to the genus *Eolis*, for example, are coloured in such a brilliant and seemingly crude manner, that they do not tone in with any natural surroundings, and so are always conspicuous They are active in their habits, and seem rather to court observation than to shun it. When we remember that such species of *Eolis* are protected by the numerous stinging cells in the cnidophorous sacs placed on the tips of all the dorsal processes, and that they do not seem to be eaten by other animals, we have at once an explanation of their fearless habits and of their conspicuous appearance The brilliant colours are in this case of a warning nature for the purpose of rendering the animal provided with the stinging cells noticeable and recognisable. But it must be remembered that in a museum jar, or in a laboratory dish, or as an illustration in a book or on the wall, *Dendronotus* is quite as conspicuous and striking an animal as *Eolis*. In order to interpret correctly the effect of their forms and colours, we must see them alive and at home, and we must experiment upon their edibility or otherwise in the tanks of our biological stations.[1]

Let me give you one more example of a somewhat different kind. The soft, unprotected mollusc, *Lamellaria perspicua*, is not uncommonly found associated (as Giard first pointed out) with colonies of the compound Ascidian *Leptoclinum maculatum*, and in these cases the *Lamellaria* is found to be eating the *Leptoclinum*, and lies in a slight cavity which it has excavated in the Ascidian colony, so as to be about flush with the general surface The integument of the mollusc is, both in general tint and also in surface markings, very like the Ascidian colony with its scattered ascidiozooids This is clearly a good case of protective colouring Presumably the *Lamellaria* escapes the observation of its enemies through being mistaken for a part of the *Leptoclinum* colony, and the *Leptoclinum*, being crowded like a sponge with minute sharp-pointed spicules, is, I suppose, avoided as inedible by carnivorous animals, which might devour such things as the soft unprotected mollusc. But the presence of the spicules evidently does not protect the *Leptoclinum* from *Lamellaria*, so that we have, if the above interpretation is correct, the curious result that the *Lamellaria* profits by a protective characteristic of the *Leptoclinum*, for which it has itself no respect, or, to put it another way, the *Leptoclinum* is protected against enemies to some extent for the benefit of the *Lamellaria* which preys upon its vitals.

It is, to my mind, no sufficient objection to theories of protective and warning

[1] See my experiments on Fishes with Nudibranchs in *Trans Biol. Soc*, Liverpool, vol. iv., p 150, and *Nature* for June 26, 1890

colouration that careful investigation may from time to time reveal cases where a disguise is penetrated, a protection frustrated, an offensive device supposed to confer inedibility apparently ignored. We must bear in mind that the enemies, as well as their prey, are exposed to competition, are subject to natural selection, are undergoing evolution; that the pursuers and the pursued, the eaters and the eaten, have been evolved together; and that it may be of great advantage to be protected from *some*, even if not from all enemies. Just as on land some animals can browse upon thistles whose 'nemo me impune lacessit' spines are supposed to confer immunity from attack, so it is quite in accord with our ideas of evolution by means of natural selection to suppose that some marine animals have evolved an indifference to the noxious sponge or to the bristling Ascidian, which are able by their defensive characteristics, like the thistle, to repel the majority of invaders.

Although we can keep and study the Littoral and Laminarian animals at ease in our zoological stations, it may perhaps be questioned how far we can reproduce in our experimental and observational tanks the conditions of the ' Coralline ' and the ' Deep-mud ' zones. One might suppose that the pressure—which we have no means as yet for supplying [1]—and which at 30 fathoms amounts to nearly 100 lbs. on the square inch, and at 80 fathoms to about 240 lbs., or over 2 cwt. on the square inch, would be an essential factor in the life conditions of the inhabitants of such depths, and yet we have kept half a dozen specimens of *Calocaris macandreæ*, dredged from 70 to 80 fathoms, alive at the Port Erin Biological Station for several weeks, we have had both the red and the yellow forms of *Sarcodictyon catenata*, dredged from 30 to 40 fathoms, in a healthy condition with the polypes freely expanded for an indefinite period; and Mr Arnold Watson has kept the Polynoid worm, *Panthalis oerstedi*, from the deep mud at over 50 fathoms, alive, healthy, and building its tube under observation, first for a week at the Port Erin Station, and then for many months at Sheffield in a comparatively small tank with no depth of water. Consequently it seems clear that, with ordinary care, almost any marine animals from such depths as are found within the British area may be kept under observation and submitted to experiment in healthy and fairly natural conditions. The Biological Station, with its tanks, is in fact an arrangement whereby we bring a portion of the sea with its rocks and bottom deposits and seaweeds, with its inhabitants and their associates, their food and their enemies, and place it for continuous study on our laboratory table. It enables us to carry on the bionomical investigations to which we look for information as to the methods and progress of evolution, in it lie centred our hopes of a comparative physiology of the invertebrates—a physiology not wholly medical—and finally to the Biological Station we confidently look for help in connection with our coast fisheries  This brings me to the last subject which I shall touch upon, a subject closely related both to Oceanography and Bionomics, and one which depends much for its future advance upon our Biological Stations—that is the subject of

### AQUICULTURE,

or industrial Ichthyology, the scientific treatment of fishery investigations, a subject to which Professor M'Intosh has first in this country directed the attention of zoologists, and in which he has been guiding us for the last decade by his admirable researches  What chemistry is to the aniline, the alkali, and some other manufactures, marine zoology is to our fishing industries.

Although zoology has never appealed to popular estimation as a directly useful science having industrial applications in the same way that Chemistry and Physics have done, and consequently has never had its claims as a subject of technical education sufficiently recognised; still, as we in this Section are well aware, our

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[1] Following up M. Regnard's experiments, some mechanical arrangement whereby water could be kept circulating and aerated under pressure in closed tanks might be devised, and ought to be tried at some zoological station  I learn from the Director at the Plymouth Station that some of the animals from deep water, such as Polyzoa, do not expand in their tanks.

subject has many technical applications to the arts and industries. Biological principles dominate medicine and surgery. Bacteriology, brewing, and many allied subjects are based upon the study of microscopic organisms. Economic entomology is making its value felt in agriculture. Along all these and other lines there is a great future opening up before biology, a future of extended usefulness, of popular appreciation, and of value to the nation—and not the least important of these technical applications will, I am convinced, be that of zoology to our fishing industries. When we consider their enormous annual value—about eight millions sterling at first hand to the fisherman, and a great deal more than that by the time the products reach the British public, when we remember the very large proportion of our population who make their living directly or indirectly (as boatbuilders, net-makers, &c ) from the fisheries, and the still larger proportion who depend for an important element in their food supply upon these industries; when we think of what we pay other counties—France, Holland, Norway—for oysters, mussels, lobsters, &c., which we could rear in this country if our sea-shores and our sea-bottom were properly cultivated ; and when we remember that fishery cultivation or aquiculture is applied zoology, we can readily realise the enormous value to the nation which this direct application of our science will one day have—perhaps I ought rather to say, we can *scarcely* realise the extent to which zoology may be made the guiding science of a great national industry  The flourishing shell-fish industries of France, the oyster culture at Arcachon and Marennes, and the mussel culture by bouchots in the Bay of Aiguillon, show what can be done as the result of encouragement and wise assistance from Government, with constant industry on the part of the people, directed by scientific knowledge  In another direction the successful hatching of large numbers (hundreds of millions) of cod and plaice by Captain Dannevig in Norway, and by the Scottish Fishery Board at Dunbar, opens up possibilities of immense practical value in the way of restocking our exhausted bays and fishing banks—depleted by the over-trawling of the last few decades.

The demand for the produce of our seas is very great, and would probably pay well for an increased supply. Our choicer fish and shellfish are becoming rarer, and the market prices are rising. The great majority of our oysters are imported from France, Holland, and America. Even in mussels we are far from being able to meet the demand. In Scotland alone the long line fishermen use nearly a hundred millions of mussels to bait their hooks every time all the lines are set, and they have to import annually many tons of these mussels at a cost of from 3*l.* to 3*l.* 10*s.* a ton. If 'squid' (cuttlefish) could be obtained in sufficient quantity, it would probably be even more valuable than mussels as bait, but its price is usually prohibitive. I happen to know that a fishing firm in Aberdeen paid during this last winter over 200*l.* for squid bait for a single boat's lines for the three months October to December, and there are fifty to sixty of such boats north of the Tyne. Here is a nice little industry ready for anyone who can capture or cultivate the common squid in quantity.

Whether the wholesale introduction of the French method of mussel culture, by means of bouchots, on to our shores would be a financial success is doubtful. Material and labour are dearer here, and beds, scars, or scalps seem, on the whole, better fitted to our local conditions ; but as innumerable young mussels all round our coasts perish miserably every year for want of suitable objects to attach to, there can be no reasonable doubt that the judicious erection of simple stakes or plain bouchots would serve a useful purpose, at any rate in the collection of seed, even if the further rearing be carried on by means of the bed system.

All such aquicultural processes require, however, in addition to the scientific knowledge, sufficient capital. They cannot be successfully carried out on a small scale. When the zoologist has once shown as a laboratory experiment, in the zoological station, that a particular thing can be done—that this fish can be hatched or that shellfish reared under certain conditions which promise to be an industrial success, then the matter should be carried out by the Government [1] or by capitalists

[1] We require in England a Central Board or Government Department of Fisheries, composed in part of scientific experts, and that not merely for the purpose of

on a sufficiently large scale to remove the risk of results being vitiated by temporary accident or local variation in the conditions. It is contrary, however, to our English traditions for Government to help in such a matter, and if our local Sea-Fisheries Committees have not the necessary powers nor the available funds, there remains a splendid opportunity for opulent landowners to erect sea-fish hatcheries on the shores of their estates, and for the rich merchants of our great cities to establish aquiculture in their neighbouring estuaries, and by so doing instruct the fishing populations, resuscitate the declining industries, and cultivate the barren shores—in all reasonable probability to their own ultimate profit.

In addition to the farming of our shores there is a great deal to be done in promoting the fishing industries on the inshore and offshore grounds along our coast, and in connection with such work the first necessity is a thorough scientific exploration of our British seas by means of a completely equipped dredging and trawling expedition. Such exploration can only be done in little bits, spasmodically, by private enterprise. From the time of Edward Forbes it has been the delight of British marine zoologists to explore, by means of dredging from yachts or hired vessels during their holidays, whatever areas of the neighbouring seas were open to them. Some of the greatest names in the roll of our zoologists, and some of the most creditable work in British zoology, will always be associated with dredging expeditions. Forbes, Wyville Thomson, Carpenter, Gwyn Jeffreys, M'Intosh, and Norman—one can scarcely think of them without recalling—

> 'Hurrah for the dredge, with its iron edge,
>   And its mystical triangle,
> And its hided net, with meshes set,
>   Odd fishes to entangle ! ' [1]

Much good pioneer work in exploration has been done in the past by these and other naturalists, and much is now being done locally by committees or associations—by the Dublin Royal Society on the West of Ireland, by the Marine Biological Association at Plymouth, by the Fishery Board in Scotland, and by the Liverpool Marine Biology Committee in the Irish Sea; but few zoologists or zoological committees have the means, the opportunity, the time to devote—along with their professional duties—to that detailed systematic survey of our whole British sea-area which is really required. Those who have not had experience of it can scarcely realise how much time, energy, and money it requires to keep up a series of dredging expeditions, how many delays, disappointments, expensive accidents and real hardships there are, and how often the naturalist is tempted to leave unprofitable ground, which ought to be carefully worked over, for some more favoured spot where he knows he can count upon good spoil. And yet it is very necessary that the whole ground—good or bad though it may be from the zoological point of view—should be thoroughly surveyed, physically and biologically, in order that we may know the conditions of existence which environ our fishes, on their feeding grounds, their spawning grounds, their ' nurseries,' or wherever they may be.

The British Government has done a noble piece of work which will redound to its everlasting credit in providing for, and carrying out, the 'Challenger' expedition. Now that that great enterprise is completed, and that the whole scientific world is united in appreciation of the results obtained, it would be a glorious consequence, and surely a very wise action in the interests of the national fisheries, for the Government to fit out an expedition, in charge of two or three zoologists and fisheries experts, to spend a couple of years in exploring more systematically than has yet been done, or can otherwise be done, our British coasts from the Laminarian zone down to the deep mud. No one could be better fitted to organise and direct such an expedition than Dr. John Murray.

Such a detailed survey of the bottom and of the surface waters, of their condition and their contents, at all times of the year for a couple of years, would give

imposing and enforcing regulations, but still more, in order that research into Fisheries problems may be instituted and aquicultural experiments carried out.
    [1] The dredging song (see *Memoir of Edward Forbes*, p 247).

us the kind of information we require for the solution of some of the more difficult fishery problems—such as, the extent and causes of the wanderings of our fishes, which 'nurseries' are supplied by particular spawning grounds, the reason of the sudden disappearance of a fish such as the haddock from a locality, and in general the history of our food fishes throughout the year. It is creditable to our Government to have done the pioneer work in exploring the great oceans, but surely it would be at least equally creditable to them—and perhaps more directly and immediately profitable, if they look for some such return from scientific work—to explore our own seas and our own sea-fisheries.

There is still another subject connected with the fisheries which the biologist can do much to elucidate—I mean the diseases of edible animals and the effect upon man of the various diseased conditions. It is well known that the consumption of mussels taken from stagnant or impure water is sometimes followed by severe symptoms of irritant poisoning which may result in rapid death. This 'musselling' is due to the presence of an organic alkaloid or ptomaine, in the liver of the mollusc, formed doubtless by a micro-organism in the impure water. It is clearly of the greatest importance to determine accurately under what conditions the mussel can become infected by the micro-organism, in what stage it is injurious to man, and whether, as is supposed, steeping in pure water with or without the addition of carbonate of soda will render poisonous mussels fit for food.

During this last year there has been an outcry, almost amounting to a scare, and seriously affecting the market,[1] as to the supposed connection between oysters taken from contaminated water and typhoid fever. This, like the musselling, is clearly a case for scientific investigation, and, with my colleague Professor Boyce, I have commenced a series of experiments and observations, partly at the Port Erin Biological Station, where we have oysters laid down on different parts of the shore under very different conditions, as well as in dishes and tanks, and partly at University College, Liverpool.

Our object is to determine the effect of various conditions of water and bottom upon the life and health of the oyster, the effect of the addition of various impurities to the water, the conditions under which the oyster becomes infected with the typhoid Bacillus, and the resulting effect upon the oyster, the period during which the oyster remains infectious, and lastly, whether any simple practicable measures can be taken (1) to determine whether an oyster is infected with typhoid, and (2) to render such an oyster innocuous to man. As Professor Boyce and I propose to lay a paper upon this subject before the Section, I shall not occupy further time now by a statement of our methods and results.

I have probably already sufficiently indicated to you the extent and importance of the applications of our science to practical questions connected with our fishing industries. But if the zoologist has great opportunities for usefulness, he ought always to bear in mind that he has also grave responsibilities in connection with Fisheries investigations. Much depends upon the results of his work. Private enterprise, public opinion, local regulations, and even imperial legislation may all be affected by his decisions. He ought not lightly to come to conclusions upon weighty matters. I am convinced that of all the varied lines of research in modern zoology, none contains problems more interesting and intricate than those of Bionomics, Oceanography, and the Fisheries, and of these three series the problems connected with our Fisheries are certainly not the least interesting, not the least intricate, and not the least important in their bearing upon the welfare of mankind.

[1] I am told that between December and March the oyster trade decreased 75 per cent.

APPENDIX

List of Species taken in one haul, on June 23, 1895 (see p 8)

SPONGES
- Renieri, sp.
- Halichondria, sp
- Cliona celata
- Suberites domuncula
- Chalina oculata

COELENTERATA
- Dicoryne conferta
- Halecium halecinum
- Sertularia abietina
- Coppinia arcta
- Hydrallmania falcata
- Campanularia verticillata
- Lafoëa dumosa
- Antennularia ramosa
- Alcyonium digitatum
- Virgularia mirabilis
- Sarcodictyon catenata
- Sagartia sp
- Adamsia palliata

ECHINODERMATA
- Cucumaria, sp.
- Thyone fusus
- Asterias rubens
- Solaster papposus
- Stichaster roseus
- Porania pulvillus
- Palmipes placenta
- Ophiocoma nigra
- Ophiothrix fragilis
- Amphiura chiajii
- Ophioglypha ciliata
- O. albida
- Echinus sphæra
- Spatangus purpureus
- Echinocardium cordatum
- Brissopsis lyrifera
- Echinocyamus pusillus

VERMES.
- Nemertes neesii
- Chætopterus, sp.
- Spirorbis, sp.
- Serpula, sp.
- Sabella, sp
- Owenia filiformis
- Aphrodite aculeata
- Polynoe, sp

CRUSTACEA
- Scalpellum vulgare
- Balanus, sp
- Cyclopicera nigripes
- Acontiophorus elongatus
- Artotrogus magniceps
- Dyspontius striatus
- Zaus goodsiri
- Laophonte thoracica
- Stenhelia reflexa
- Lichomolgus forficula
- Anonyx, sp.

- Galathea intermedia
- Munida bamffica
- Crangon spinosus
- Stenorhynchus rostratus
- Inachus dorsettensis
- Hyas coarctatus
- Xantho tuberculatus
- Portunus pusillus
- Eupagurus bernhardus
- E prideauxii
- E. cuanensis
- Eurynome aspera
- Ebalia tuberosa

POLYZOA
- Pedicellina cernua
- Tubulipora, sp.
- Crisia cornuta
- Cellepora pumicosa, and three or four undetermined species of Lepralids
- Flustra securifrons
- Scrupocellaria reptans
- Cellularia fistulosa

MOLLUSCA.
- Anomia ephippium
- Ostrea edulis
- Pecten maximus
- P. opercularis
- P tigrinus
- P pusio
- Mytilus modiolus
- Nucula nucleus
- Cardium echinatum
- Lissocardium norvegicum
- Cyprina islandica
- Solen pellucidus
- Venus gallina
- Lyonsia norvegica
- Scrobicularia prismatica
- Astarte sulcata
- Modiolaria marmorata
- Saxicara rugosa
- Chiton, sp.
- Dentalium entale
- Emarginula fissura
- Velutina lævigata
- Turritella terebra
- Natica alderi
- Fusus antiquus
- Aporrhais pespelicani
- Oscanius membranaceus
- Doris, sp.
- Eolis coronata
- Tritonia plebeia

TUNICATA.
- Ascidiella virginea
- Styelopsis grossularia
- Eugyra glutinans
- Botryllus, sp.
- B., sp.

# British Association for the Advancement of Science.

## IPSWICH, 1895.

# ADDRESS

### TO THE

# GEOGRAPHY SECTION,

### BY

### H. J. MACKINDER, M.A., F.R.G.S.,

#### PRESIDENT OF THE SECTION.

HIS is a memorable year for English students of geography. We have entertained in London for the first time a great gathering of our foreign colleagues, and have presented to the British public the unfamiliar spectacle of a geographical meeting, in which scholars and professors were as prominent as explorers. As a nation we may justly claim that for several generations we have been foremost in the work of the pioneer; nor need we view with dissatisfaction our contributions to precise survey, to hydrography, to climatology, and to biogeography. It is rather on the synthetic and philosophical, and therefore on the educational, side of our subject that we fall so markedly below the foreign and especially the German standard, and it is for this reason that we may regard the Sixth International Congress as a noteworthy object lesson for English geographers and teachers. The time seems, moreover, to have been ripe for some such stimulating influence. To indicate a few signs only of rising courage among our geographers, and of sympathy on the part of the public, I would draw your attention to the institution of afternoon meetings in Savile Row for the discussion of technical questions, to the success of the new Geographical Journal, notwithstanding its geographical as opposed to merely 'adventuring' flavour, to the recent formation of a geographical association of Public Schoolmasters, and to the demand for addresses on the teaching of geography on the part of the local branches of the Teachers' Guild. Facts are reminding us once more that the lapse of a certain time is essential to the rooting of a new idea, and we may thank the geographical veterans of 1869 for sowing seed the fruit of which we are now harvesting. That I am not alone in my interpretation of present tendencies is clear from the emphatic opinion of the President of the Royal Geographical Society expressed in his last annual address, that 'the time is approaching for a reconsideration of the educational policy of the Society.' It would almost seem that we are nearing a development of geographical education not unlike that which nine years ago followed on the publication of Mr. Keltie's valuable Report. At that time two of my predecessors in this chair, Sir Frederick Goldsmid and Sir Charles Warren, thought it not unfit to make education the chief theme of their addresses, and encouraged by their example I venture, under present circumstances, to call your attention once more to that subject. Since 1886 and 1887, however, much has happened, and we no longer need to discuss the more elementary teaching of geography. I propose, therefore, to treat of comparative and philosophical geography in relation especially to secondary and university education, and it seems to me that an historical rather than an *a priori* discussion gives best promise of result.

E

The middle of the 18th century marks an important epoch in the history of geography. In ancient times Ptolemy and Strabo grasped the system and possibilities of our science, but they failed to build high from lack of a broad foundation of precisely recorded facts. Subsequently, geography had its Dark Ages and its Renascence in harmony with the general trend of human affairs. By the end of the 16th century Mercator and Ortelius had somewhat more than recovered the Greek position, but still, for another century and a half, geographers wrestled with essentially the same problems as had presented themselves to the ancients. The observers ascertained latitudes and longitudes with ever-increasing precision, the cartographers projected the observed positions on their maps with growing happiness of compromise, and the scholars sought, with the prodigious industry characteristic of the age, to identify the sites mentioned by the ancient authorities. Three names—Harrison, D'Anville, and Varenius—in the several fields of observation, cartography, and scholarship, may be taken as completing this stage of development, although, as is always the case, the new and the old overlapped. In 1761 the chronometer was added by Harrison to the magnetic compass, the log-line, the sextant, and the theodolite, and thus was completed the observer's equipment. In the same year D'Anville published his *Atlas Moderne*, in which (besides a fidelity of outline greater than that of his predecessors Delisle and Homann) he brought to bear a mechanical finish and a criticism of data that were new to cartography. Only a few years earlier, in 1755, there appeared in Paris a French translation of the *Geographia Generalis* of Varenius, first published at Amsterdam in 1650, edited for Cambridge in 1681 by Sir Isaac Newton, and reprinted again and again for three generations as the masterpiece of the 'scholarly' geographers. Thus when George III. was still young, the *horizontal* outlines of the map of the world had taken their now familiar form, and school geography consisted of 'the use of the globes' with some small attention to classical topography.

What made the 18th century a transition age of such importance to geography was the realisation of new problems, which both Antiquity and the Renascence had either neglected or utterly failed to solve. These problems allow of most general expression by the use of three convenient terms, two of them lately imported from Germany—lithosphere, hydrosphere, and atmosphere—the first implying the rock globe whose surface is both land and sea-bed, the other two denoting the external envelopes. The geographer is concerned with the atmosphere, the hydrosphere and the surface of the lithosphere. His first business is to define the *form*, or relief, of the surface of the *solid* sphere, and the *movements*, or circulation, within the two *fluid* spheres. The land-relief conditions the circulation, and this in turn gradually changes the land-relief. The circulation differentiates climates, and these, together with the relief, constitute the environments of plants, animals, and men. Shorn of complexities, this is the main line of the geographical argument. In the language of Richthofen, the earth's surface and man are the terminal links. It is clear that all depends on the accuracy of the first premises—the form of the lithosphere and the movements within the hydrosphere and atmosphere. Before last century geographers ascertained the horizontal elements in form, but neglected the vertical. In the matter of outline, the maps of D'Anville are an immense improvement on those of Ortelius, but they exhibit essentially the same almost child-like methods for the depiction of relief which had been employed by Buckinck in the 1478 edition of Ptolemy. Until this was remedied the whole superstructure of comparative and philosophical geography lacked any real basis.

Like the letters of the alphabet, conventional hill-shading was evolved from pictures rather than invented. The great atlas of Germany, published at Nuremberg in 1753 by the successors of Homann, consisting as it does of maps engraved in various years extending from 1718 to 1753, shows admirably almost every stage in the evolution. Other striking evidence may be seen in the chart of New Zealand drawn from Captain Cook's surveys, and reproduced by Admiral Wharton in his edition of Cook's Journal. Side by side on the same chart, we have the 'ant-hills' of Buckinck and Ortelius, and the 'caterpillars' of modern maps; but the latter, like degenerate animals with rudimentary organs, still retain clear marks

of their origin. The 'ant-hills,' elsewhere sown evenly over the land-surface, are in certain parts drawn into chains and foreshortened, or in modern railway parlance 'telescoped.' One step more—the confusion of the lines of slope-shading with those of hill-outline—and the pictures would be conventionalised, all signs of origin would be lost, and students who had never seen a great mountain-range would be led to think of it as a wall-like ridge. Even 'ant-hills' are preferable to the 'caterpillar' in its crudest form.

An indication of the importance attached to the new problem of relief is to be seen in the fact that, before the method of hill-shading or hatching had been perfected, the method of horizontal contouring had already been invented. In 1737 Philip Buache, a French geographer of remarkably original mind, produced a contoured chart of the English Channel. Contour lines represent what would be coast lines were the sea to rise or fall to the level indicated, and it was natural that this device should first be applied to the mapping of the sea-bed rather than the land. In 1791 Dupain Triel drew a contoured map of France. But already in 1783, as Mr. Ravenstein pointed out in his address at the Cardiff meeting, Lehmann had combined the two systems, and, by superimposing hachures upon contours, and making the depth of shading proportional to the closeness of the contours, had produced a map which, while yielding to the popular requirements, rested on a scientific basis. Contoured maps, in which names are few or absent, can now, however, be made to rival in pictorial suggestiveness those which are shaded, and such maps are the more valuable in that they are not only structurally correct, but that they can be read also with accuracy and ease. Some of the sheets of the American Geographical Survey may be cited as excellent examples of graphic effect produced by contours only.

Ptolemy's knowledge of the theory and methods of cartography far outran the positive materials at his command for the mapping of the known world. In the same way the methods of depicting relief, though so recently developed, already at the end of last century more than sufficed for the presentment of the recorded data. As was seen in the case of Ptolemy, there are peculiar dangers in the possession of an engine more powerful than is needed for the work in hand. In 1783 Frnace was the only country in the world with a completed map based on systematic and detailed surveys A relief-map like that of Dupain Triel was possible only in such a country. But in 1756 Philip Buache had already launched a general theory of relief resting on the conception of river basins, and had enriched geography with the terms 'water-parting' and 'plateau.' In the absence of positive knowledge, what more natural than that cartographers should make illegitimate use of the theory of Buache, and should assume that in the coherent system of water-partings they had the orographical skeleton of the world? Having drawn the courses of the rivers, they had only to run caterpillar-shading along the water-partings to produce a map, in parts accidentally true, which represented the land as uniformly composed of a series of flat pans. Such a method of map-drawing was advocated by Friedrich Schultz, in a paper published at Weimar as late as 1803, and is not rare in popular maps of much later date.

It is to Alexander von Humboldt that we owe the method still in use for giving a general, yet real, idea of the relief of a little known country. Following, as he himself tells us, the precedent of the canal engineers, he constructed vertical sections along his routes through Spain and Mexico It is worth noting in this connection that our knowledge of the relief of the sea-bed is mainly due to the requirements of another set of engineers—those engaged in laying telegraphic cables. Humboldt's sections were rendered possible by the daily use of the barometer and chronometer, and by Ramond's improvement of the formula for the reduction of barometric data. Before Humboldt, the barometer had been used for the determination of isolated heights, but not for the traversing of a whole country.

Turning now to the other basis of scientific geography—a knowledge of the fluid circulation in the outer envelopes of the earth—we may regard the corner-stone of climatology as laid by George Hadley in 1735, in his well-known paper before the Royal Society, 'Concerning the Cause of the General Trade Winds.'

All that was done before his time was mere digging for the foundations; yet with rare thoroughness he enunciated, at one effort, the final theory, detecting the cause both of the movement equatorwards and of the westward swerving. We can point to no such crucial utterance in the sister field of oceanography, though it is said that, about the time of the American Revolution, Benjamin Franklin suggested that wind-pressure was the cause of the surface-currents of the sea His idea was contained in a memoir on the Gulf Stream, which was suppressed by him lest it should fall into the hands of the English, and be of use to their ships in crossing the Atlantic Major Rennell also, who, by his map of India and his Herodotean identifications, presents a likeness to the best of the old school of geographers, showed his participation in the new by compiling an Atlantic current-chart. But Humboldt's invention of isotherms in 1817 first gave to climatology cartographic resources, and rendered easy and precise the correlation of climate with relief. The idea was soon applied in other departments of geography—to the expression of atmospheric pressure, of the temperature of the sea-surface, of density of population, and indeed to any similar masses of data, capable, so far as time is concerned, of reduction to averages, but varying locally. The last edition of Berghaus's Physical Atlas is, in this matter, a monument to the memory of Humboldt; yet it is strange that a method first suggested, in the seventeenth century, by the magnetic lines of the Englishman Halley, should have been left to fructify in the mind of a German of the nineteenth century.

The facts of geography are obviously capable of two kinds of treatment. The chapter-headings may be such as ' Rivers,' ' Mountains,' ' Cities,' or such as · Ireland,' ' Italy,' ' Australia ' In other words, we may consider the phenomena of a given type in all parts of the globe, or we may discuss in a given part of the globe the phenomena of all types In the former case, our book should *as a whole* observe the order of what has been called the geographical argument; in the latter case each chapter, the discussion of each country, should exhibit that order complete. For historical reasons, which will be referred to later, we English have fallen into a bad habit of describing the former treatment as ' physical geography,' and the latter as ' geography.' The Germans are more reasonable when they contrast *Allgemeine Erdkunde* with *Landerkunde,* but Chorography, our nearest English equivalent to *Landerkunde,* is a clumsy expression An alternative would be to speak of ' special geography,' thereby implying a correlative to ' general geography,' which is a precise rendering of *Allgemeine Erdkunde.* By whatever name we call it, however, it is clear that the treatment by regions is a more thorough test of the logic of the geographical argument than is the treatment by types of phenomena. Hence Humboldt's *Essai politique sur la Nouvelle-Espagne,* published in 1809, must take high rank among the efforts of the new geography as the first complete description of a land with the aid of the modern methods. Here, for the first time, we have an exhaustive attempt to relate causally relief, climate, vegetation, fauna, and the various human activities.

The services of Humboldt to our science were so great that he almost merits the title of a new founder, and yet, of late, it has been the custom to decry him. It is probable that his memory has suffered a little from the less original work of his old age, for the Humboldt who devised cross-sections and isotherms, and wrote the *Essai politique,* was divided by the distance of a whole generation from him who was responsible for the *Asien* and the *Kosmos.*

We come now to the central event in the history of modern geography. It was in the year 1820 that Karl Ritter was called to Berlin to act in the double capacity of Professor in the Military School and Professor Extraordinary in the University. Born in 1779, ten years after Humboldt, Ritter's early training and circumstances were such as admirably to fit him for the great position he was to occupy during the last thirty-nine years of his life. His schooling was at Schnepfenthal, under Salzmann, a well-known educational experimenter of the following of Rousseau. Later in life Ritter learnt to know and to love the classics, but Salzmann's hostility to them as an educational implement secured for his pupil freedom from the current intellectual moulding The peculiar opportunities of his subsequent position as tutor in the Hollweg family almost amounted to

an endowment for research, and it was then that he accumulated that vast miscellaneous knowledge so valuable to the intellectual pioneer. It is not unimportant in connection with Ritter's later theories to observe that, at this time, Cuvier and Franz Bopp were applying the comparative method to anatomy and philology. Nor did he fail to cultivate that half artistic perception of land-forms, the early exercise of which seems to be to the geographer what youthful training in pronunciation is to the linguist. While travelling with the young Hollwegs, he caused astonishment in Switzerland by the accuracy of his delineation of a mountain range. Add that fortune brought Humboldt and Pestalozzi across his path, and we understand the influences which shaped Karl Ritter into the greatest modern professor of geography.

Ritter produced both books and men. He had the personal charm of the born teacher, and the Prussian officers of 1866 and 1870 were as truly his intellectual offspring as was the *Erdkunde*, of which Schlegel said that it was the Bible of Geography. Nor did his classes fail to bring forth professed geographers, such as Guthe, and historians with the geographical eye, such as Curtius. But Ritter did not stand alone. He was one of a group of four men, who together made the geography of the nineteenth century as distinctively a German science as that of the eighteenth century had been French. One is almost tempted to draw a comparison, man for man, between Humboldt, Ritter, Berghaus, and Perthes, and that great group of later Germans—Bismarck, Moltke, von Roon, and William I. The coincidence is not quite so fortuitous as might at first sight appear; for Berghaus, the cartographer, and Perthes, the capitalist employer of cartographers, were as necessary to the earlier combination as, to the later, were von Roon, the organiser, and William, the kingly employer of statesmen and generals.

In 1827 Humboldt, who, on his mother's side, was French by descent, left Paris, which had been his home for nearly twenty years, to join the Prussian Court at Berlin. In the winter of 1827–28 he gave a course of brilliant lectures before the University, in which was contained the nucleus of the subsequent *Kosmos*. In 1829, at the invitation of the Russian Government, he spent twenty-five weeks on a rapid journey to the mines of the Ural and Altai, and received the impressions which led to the *Asien* Thence onward Humboldt and Ritter lived at Berlin, mutually appreciative, and complementing each other in mental characteristics. They died in the same year, 1859, just before those great political events which changed the whole aspect of German life.

The influence of the new school was early felt beyond Germany. Petermann, the pupil of Berghaus, came to our islands to help Keith Johnstone with the English edition of Berghaus's great Physical Atlas, whilst Arnold Guyot, the Swiss disciple of Ritter, after teaching for a time at Neuchâtel, crossed the Atlantic to lecture at Harvard, and afterwards to accept a chair at Princeton.

No sooner, however, were the two great masters at Berlin dead, than German geography passed into a new phase, a phase of which the typical representative was Oscar Peschel, the critic of both Humboldt and Ritter The facts of Peschel's life are soon told. He began as a journalist, he became a geographical writer, and died a professor of geography. From 1849 to 1854 he was assistant editor of the Augsburg *Allgemeine Zeitung*. Then until 1870 he was sole editor of the weekly *Ausland*. From 1871 until his death in 1875 he occupied a chair in Leipzig University. The titles of his books may serve as an index to his mind. The 'Age of the Discoveries' appeared in 1858, and the 'History of Geography' in 1865. He then turned his attention to physical questions, and produced in 1870 his striking 'New Problems for Comparative Geography.' Finally, in 1874, came the *Volkerkunde*, a title not easily translatable into English. After his death his pupils, acting apparently under the inspiration of Professor Kirchhoff of Halle, collected his essays and lectures, which were published in a series of volumes edited with varying degrees of merit.

Peschel's criticism of Humboldt was of the rarest kind. He appreciated the good, detected the errors, and, above all, suggested the remedies. Humboldt's later works, the *Asien* and the *Kosmos*, both exhibit striking excellences, and for a time enjoyed great vogue, yet both, like Newton's Optics, helped to delay the

advance of science.   How this happened will be manifest if we reflect that general or physical geography is the basis, not only of special geography, but also of geology , and that just when Humboldt was vitiating his description of Asia with Elie de Beaumont's speculations on the origin of mountains, and was conveying the impression that general geography was equivalent to the entirety of natural science, Lyell was shaping physical geography to the ends of the geologist, and making it a key to unlock the past.   The result, so far as geography is concerned, may be seen at the present day in the time-table of many an English girls' school.   Separate hours are set apart for ' physical geography ' and for ' geography.'   The one is studied with a text-book written from the geological standpoint, the other in a manual of mere names lit up occasionally with a few ideas drawn from Ritter or Strabo. Thus it was that geography was divorced from physical geography to be unequally yoked with history.   Peschel restored physical geography to the geographer, and made it the implement of analysis in the field of *Landerkunde*.

But while the geographers had gone astray in the wake of Humboldt, the geologists neglected that great chapter of their subject which they hold to-day in common with the geographers    Stratigraphy, palæontology, and mineralogy claimed their first attention, and it was only after a time that Ramsay and Geikie among the English geologists, and Dana among the Americans, began to study what we now call geomorphology—the causal description of the earth's present relief.   It was Peschel who asserted the claim of geography to include geomorphology, and so rendered possible a genetic, as opposed to a merely conventional classification of the features of relief   Though common to both studies, it plays a different part in each.   The geologist looks at the present that he may interpret the past ; the geographer looks at the past that he may interpret the present.   The geographer's argument begins, as we have said, with the surface of the earth, but to his almost artistic perception of land-forms he must add a causal analysis ; precisely as the artist learns anatomy the better to grasp the human outlines

Peschel's criticism of Ritter is less happy than that which he gave to Humboldt.  He complains of Ritter's use of the expression ' Comparative Geography,' and substitutes another of his own   As a matter of fact, all geography which is not merely descriptive must be comparative, and the various uses of the term made by different writers are but particular cases of one of the most general ideas in scientific method.   Varenius called all geography comparative that was not mathematical or astronomical.   Ritter compared peoples with the lands they inhabited, in order to establish the influence of environment.   Peschel compared one physical feature with another, with the object of discovering their origin.   Markham uses comparative geography to imply a comparison of historical records, with a view to showing the changing aspects of the same locality at different times.   Peschel's difference with Ritter is, in this matter, a merely verbal quibble.   Nor can we say much more with reference to his obvious dislike of Ritter's teleological views, which, though they colour every statement he makes, yet do not affect the essence ; it is easy to re-state each proposition in the most modern evolutionary terms. Where, however, Peschel questions the adequacy of particular correlations of peoples and environments, it must be admitted that he usually strikes between the joints, and this is still more evident when he has to deal with Ritter's daring follower, Buckle.  The truth of the matter is that Ritter and Buckle had taken for their field the highest and most difficult chapter in geography, and that they underrated the complexity of the problems with which they had to deal.   We are all familiar with the saying that it required the Greeks in Greece to develop the Athenian civilisation, and that neither the Greeks elsewhere, nor any other race in Greece, would have been equal to the achievement.   It would be easy for a Peschel to demonstrate the falsity of an assertion that the Greeks owed all to Greece, but, on the other hand, the Ritters and Buckles were in error in attempting so simple an explanation   What seems to have been constantly omitted from these speculations is the fact that communities can move from one environment to another ; that even a given environment alters from generation to generation ; and that an existing community is often the product of two or more communities in past generations, each of them subject to a different environment.   Now, the influences

affecting a community at a given time may be resolved into dynamic and genetic. Among the dynamic influences, geographical environment is admittedly important. But the genetic influences are the momentum from the past, and the genetic influences acting on this generation may be resolved into the dynamic and genetic of the last. If this process be repeated through many generations, it is clear that the sum total of geographical influence is always accumulating. The Normans, for instance, were exposed to successive environments in Norway and in Normandy, and much that was out of place in Normandy was due to the earlier action of Norway. The American, again, has characteristics and institutions which could hardly have been cradled in the Mississippi plain, but are explainable by a reference to the peninsulas and islands of Europe. A very striking instance of the errors involved both in Ritter's methods and Peschel's criticism is to be found in the case of China. Peschel assumes that the Chinese civilisation grew up in China, and asserts that a land of so massive outline was not fitted to stimulate such a growth. But the most modern research tends to show that the Chinese were not thus isolated in early times, and that Chinese civilisation was of Western, not home origin. Ritter erred in thinking the action simple and uniform, Peschel in underestimating its cumulative influence.

Since the war of 1870, geographical chairs have been multiplied throughout Europe, and especially in Germany, and at the present time German-speaking geographers form a little public of themselves. Some of the Professors, as von Richthofen of Berlin and Penck of Vienna, have worked mainly at geomorphology; others, such as Krummel of Kiel, at oceanography, others, again, such as Ratzel of Leipzig, at anthropogeography; while Wagner of Gottingen has been conspicuous in cartography, and Kirchhoff of Halle and Lehmann of Munster in questions of method. Davis of Harvard and Woeikof of St Petersburg may count as foreign adherents of the German school. There can be no doubt that it is especially in geomorphology that the advance has been most rapid, and here we may trace Peschel's impulse still unexhausted In 1887 Gerland of Strasburg went so far as wholly to exclude the human element from geography, and to make it a purely physical science. He probably represents the extreme swing of the pendulum. There is evidence now of a reaction towards Ritter, and as Wagner has pointed out, we owe to Gerland himself the admirable series of maps in the new edition of Berghaus's Atlas, which deals with man, and brings out with startling clearness the interdependence of relief, climate, and population.

Let us now sum up the problems and methods of modern geography as they have resulted from the last five generations of work and criticism. Merely verbal definitions may be left to the dialectician, but there are two different modes of giving practical definition to a department of knowledge. It may be considered either as a discipline, or as a field of research. As a discipline, a subject requires rough definition for the purposes of organisation. It should exhibit a central idea or a consistent chain of argument. On the other hand, no theoretical considerations can hold the investigator within set bounds, though he is none the less practically limited by the nature of the arts of investigation to which he has served his apprenticeship. The chemist should manipulate the blowpipe, the physicist should be an expert mathematician, the historian should be skilful as a palæographer, and familiar with mediæval Latin. That subject is most legitimate which admits of either definition, which exhibits both a consistent argument and also characteristic arts. The researcher will then be the writer of the text-book, and while research is fertilised by suggestions born of teaching, teaching will be illuminated by the certainty within uncertainty which comes of first hand touch with facts. Geography satisfies both requirements, it has arts and an argument.

There are three correlated arts (all concerned chiefly with maps) which may be said to characterise geography—observation, cartography, and teaching. The observer obtains the material for the maps, which are constructed by the cartographer and interpreted by the teacher. It is almost needless to say that the map is here thought of as a subtle instrument of expression applicable to many orders of facts, and not the mere depository of names which still does duty in some of the most costly

English atlases. Speaking generally, and apart from exceptions, we have had in England good observers, poor cartographers, and teachers perhaps a shade worse than cartographers. As a result, no small part of the raw material of geography is English, while the expression and interpretation are German.

The geographical argument has already been sketched. The first chapter deals with geomorphology—the half artistic, half genetic consideration of the form of the lithosphere  The second chapter might be entitled geophysiology; it postulates a knowledge of geomorphology, and may be divided into two sections—oceanography and climatology. At the head of the third and last chapter, is the word 'biogeography,' the geography of organic communities and their environments. It has three sections—phytogeography, or the geography of plants ; zoogeography, or the geography of animals; and anthropogeography, or the geography of men. This chapter postulates all that has preceded, and within the chapter itself each later section presupposes whatever has gone before. To each later section and chapter there is an appendix, dealing with the reaction of the newly-introduced element on the elements which have been considered earlier. Finally, there is a supplement to the whole volume, devoted to the history of geography, or the development of geographical concepts and nomenclature.

The anthropogeographer is in some sense the most typical and complete of geographers.  His special department requires a knowledge of all the other departments.  He must study geomorphology without becoming a geologist, geophysiology without becoming a physicist, biogeography without becoming a biologist.  It has been recognised ever since the time of Strabo that geography culminates in the human element, but the difficulties in the way of precise thought in this branch of the subject are such that, while its claims have been constantly reasserted, the other branches have hitherto made greater progress. At all times each race exhibits a great variety of initiative, the product, in the main, of its past history  In each age certain elements of this initiative are selected for success, chiefly by geographical conditions  Sometimes human genius seems to set geographical limitations at defiance, and to introduce an incalculable element into every problem of anthropogeography. Yet, as we extend our survey over wider periods, the significance even of the most vigorous initiative is seen to diminish. Temporary effects contrary to Nature may be within human possibilities, but in the long run Nature reasserts her supremacy. Celt, Roman, and Teuton successively neglected the Alpine and the Pyrenean frontiers, but modern history has vindicated their power. Probably, when it is fully recognised that the methods of anthropogeography are essentially the same as those of physical geography, advance will become more rapid. The facts of human geography, like those of all other geography, are the resultant for the moment of the conflict of two elements, the dynamic and the genetic. Geographical advantages of past times permitted a distribution and a movement of men which, by inertia, still tend to maintain themselves even in the face of new geographical disadvantages. Economic or commercial geography should probably be regarded as the basal division of the treatment. The streams of commodities over the face of the earth, considered as an element in human environments, present many analogies to the currents of the ocean or the winds of the air. Strategical opportunities, also, have a constant action on communities, in the shape of tempting or threatening possibilities. Political geography becomes reasonable when the facts are regarded as the resultant in large measure, of genetic or historical elements, and of such dynamic elements as the economic and strategic.

This being our conception of geography, it seems not without interest to sketch our ideal geographer  He is a man of trained imagination, more especially with the power of visualising forms and movements in space of three dimensions—a power difficult of attainment, if we are to judge by the frequent use of telluria and models. He has an artistic appreciation of land forms, obtained, most probably, by pencil study in the field ; he is able to depict such forms on the map, and to read them when depicted by others, as a musician can hear music when his eyes read a silent score ; he can visualise the play and the conflict of the fluids over and around the solid forms ; he can analyse an environment, the local resultant of

world-wide systems; he can picture the movements of communities driven by their past history, stopped and diverted by the solid forms, conditioned in a thousand ways by the fluid circulations, acting and reacting on the communities around; he can even visualise the movement of ideas and of words as they are carried along the lines of least resistance. In his cartographic art he possesses an instrument of thought of no mean power. It may or may not be that we can think without words, but certain it is that maps can save the mind an infinitude of words. A map may convey at one glance a whole series of generalisations, and the comparison of two or more maps of the same region, showing severally rainfall, soil, relief, density of population, and other such data, will not only bring out causal relations, but also reveal errors of record; for maps may be both suggestive and critical. With his visualising imagination and his facile hand, our ideal geographer is well equipped, whether he devote himself to a branch of geography or to other fields of energy  As a cartographer he would produce scholarly and graphic maps; as a teacher he would make maps speak; as an historian or biologist he would insist on the independent study of environment instead of accepting the mere *obiter dicta* of the introductory chapters of histories and text-books; and as a merchant, soldier, or politician he would exhibit trained grasp and initiative when dealing with practical space-problems on the earth's surface. There are many Englishmen who possess naturally these or compensating powers, but England would be richer if more of such men, and others besides, had a real geographical training.

Let us consider for a moment the methods of organisation by which the German results have been produced. There are two systems of examination important to geography—the philosophical doctorate of the Universities, and the *facultas docendi* of the State. Candidates for the doctorate present three subjects, one major and two minor, selected according to the taste or requirements of the student. Young geographers usually present themselves in geography as major, and in history and geology as minor subjects. The State examination for the *facultas docendi* is of greater severity and of more general effect, in that every secondary teacher must hold the Government qualification in the subjects he teaches. As long ago as the time of Mr. Keltie's report, a single professor, Wagner of Göttingen, had examined in geography 200 candidates for the *facultas docendi*. It is a consequence of this system that at the last meeting of the *Deutsche Geographentag* there was an attendance of 500 members, *mostly specialist teachers of geography;* and, as a further consequence, there is a market for good maps in the German-speaking lands, whereas in England, reformers are constantly daunted by the fact that the public actually *prefers* the bad to the good. English specialists are almost invariably compelled to use German maps.

In most German Universities there is now a Geographical Institute, possessed of lecture-rooms and work-rooms, with appliances and collections , and the teaching combines lecture, seminar, cartographical exercise, written thesis, and field practice. At Vienna, for instance, there are two professors of geography in joint charge of an institute founded in 1885  The institute has a yearly subvention from the State, and in 1891 had a library of 2,400 volumes, the necessary globes and telluria, and an equipment of instruments for observation and cartography, besides 131 wall maps, 27 relief models, 135 diagrams, 370 typical views (*character-bilder*), 1,200 photographs, 148 bound atlases, and about 5,000 separate maps. There were also a collection of rock-specimens, used more especially to convey the necessary geological ideas to the *Historiker* (who form a majority of the students), and a series of typical school-books and school atlases for the benefit of teachers  Professor Penck remarks that the neighbourhood of Vienna is in itself an admirable laboratory for every department of geography. It should be carefully noted that the University Institutes compete neither with geographical societies nor with public libraries, in that books and specimens of rare or unique character are excluded from the collections, which are solely for the use of the students of the institute.

In England geography has no appreciable position in degree-examinations; there are no examinations at all for the post of secondary teacher, nor is there anywhere in the land anything really comparable to the German

Geographical Institute.  Since 1869 the Royal Geographical Society has made repeated efforts to alter the situation, and it would be an error not to recognise that we are on the upward gradient.  The Society's policy has been embodied chiefly in four measures—the offer of medals to the great public schools ; the appointment of an inspector to report on foreign geographical teaching : the foundation of lecturerships in the universities, and the institution of a system of training for explorers. After sixteen years of trial the medals were discontinued on the ground that they affected only a few schools, and even in those schools only a few pupils.  Out of a total of 62 medals awarded, no fewer than 30 fell to two schools ; a noteworthy fact, as indicating at once the power and the rarity of skilled and enthusiastic geographical teaching.  The most significant result of Mr. Keltie's report. and of the exhibition of specimens collected by him and now deposited with the Teachers' Guild in Gower Street, has been a general improvement in school text-books and maps, as seen particularly in some of the better elementary schools and training colleges.  The university lecturerships have been effective only at Oxford for a sufficient time to judge of results.  There, a considerable class of historical students attend lectures in geography twice a week, but are not likely to give the time necessary for more thorough study without the stimulus of examination.   None the less, students who have heard lectures are gradually spreading geographical ideas, and the mere existence of the lecturerships is a valuable admission that the study is one of University rank.  The classes for explorers have been conspicuously successful, and are probably the best of their kind in the world.  But here we are dealing with those arts of observation in which, as already remarked, Englishmen excel.

With the example of Germany before us, with partial success to encourage us, with the interest aroused by the recent Geographical Congress to aid us, and with the reorganisation of secondary teaching impending, is not this the ripe opportunity for another, and it may be final effort, to make geography effective in English education ?  I do not deny that there may be several good roads to success, but I cannot help feeling that our most immediate need is a certain amount of centralisation.  This is so for two reasons   First, because we English geographers require, above all things, a tradition.  We vary so widely in our views, and our examiners examine so differently, that teachers are at a loss whether to keep to the old methods or venture on the new.  The old classical education still maintains its supremacy, mainly because through strong tradition it is workable without artificial syllabus , it is an organism rather than a machine.  German geography, despite its modern growth, has a tradition, for Germans are all sons in geography of the ancestral group—Humboldt, Ritter, Berhaus, and Perthes.  Secondly, we need a worthy object lesson, which is attainable under existing circumstances only by the concentration of funds, and by the co-operation of several leaders. For no single lecturer, such as the Universities at present maintain, can deal adequately with all aspects of geography.  An historical or classical student listens to a dozen different teachers at Oxford or Cambridge.  Berlin and Vienna have each of them two professors of geography, besides *Docenten.*  Moreover, a German student may pass from university to university, and thus correct the limitations of his teachers.  Yet nothing short of a considerable object lesson in England will bring general conviction as to the value and possibilities of geography.  Nor need we fear that when centralisation has done its work, independent and local initiative will not vary the general tradition   Furthermore, the centralisation should not be complete.  The work in progress at the Universities must not be abandoned. It will steadily gain importance in proportion as the central body does the work for which it is designed.

Clearly, if the policy of centralisation be agreed to, there is only one site for the central school.  It must be in London, under the immediate inspiration of that Royal Geographical Society, whose past services to the cause would be a guarantee of support during the early efforts.  But geographers must associate with themselves experts in education, if they are to avoid certain rocks which have knocked many a hole into the geographical projects of the past, and if public bodies and private individuals are to be moved to financial generosity.  The beginning

might be on a relatively small scale, but must not be too small for completeness. Theory, both on the scientific and historical sides, must be represented, and each of the three geographical arts. As regards observation nothing better could be asked than association with the admirable classes already existing. Cartography would be needed not only to supply the English map trade with an occasional Petermann, but especially that all serious students of the school might learn the ways of the geographical workshop. Teaching would naturally be associated with the various secondary and elementary training colleges. A certain number of university men might be tempted by the offer of a diploma to interpose a geographical year between the university and the master's desk; for head masters would probably be only too glad to give the teaching of geography into the hands of specialists, provided these were men of university culture, able to be of general service in school-work, and provided also there was adequate guarantee that they were experts. There would, in addition, be a system of evening classes for teachers and clerks, and thus, while the school would render obvious and direct service to six millions of people, the staff would gain strength from the sense of a generally diffused trust in them. The school would in no way duplicate the Geographical Society, while its staff would contribute an element of trained experts to the newly established afternoon meetings.

I launch this scheme, not with any fixed idea on the subject, for I would willingly abandon it in favour of another shown to be better, but because I am convinced that now is a great opportunity, and that a definite plan, even if it should prove unworkable, is more likely to provoke discussion and to produce result than mere negative criticism, which has often been anticipated. As effects of any adequate scheme, I should hope that, in a few years' time, geographical examinations would consistently test not merely memory for small detail, but clearness of apprehension, breadth of view, and power of statement, whether in word or map; that teachers would have the knowledge needed for Socratic rather than dogmatic teaching, and that students of geography would exercise the powers of analysis and composition, and not merely observe and remember. Geography would then be a subject rather for the higher than the lower parts of schools, and with the aid of a shelf of the classics of travel, sixth-form boys would write geographical essays with rapid but accurate map illustration. Then, the Universities would receive freshmen who, whether candidates for historical or scientific honours, could express themselves resourcefully in map and diagram, as well as in language and writing. I speak from experience when I say that not one undergraduate in thirty has the necessary equipment for accurate appreciation in history of space-relations, as well as time-relations. In an age of inevitable but unfortunate specialisation the organising of another correlating study should not be unwelcome.

Once more, let us emphasise the fact that geography is not the science of all things. It has been the aim of this address to bring out the specific character of geography and of the geographer. Nor is it the only important subject in education. Its devotees frequently do it harm by excessive claims. Moreover, let us admit that as geography is now too often taught, and even as it is conceived of in some circles which pass for geographical, it merits no greater mercy than it receives at the hands of educationalists. Nor let it be denied that some facts that we would see taught as geographical are already dealt with in other, and as we think, less advantageous connections. Lastly, let us beware of extolling the German example, which happens to be good in geography, to the degree of imputing inferiority to the whole system of English education. Let us do full justice to the position of our opponents, let us humbly benefit by their criticism, and then claim soberly, but with persistence, that a worthy geography is no pariah among intellectual disciplines. Amid the changes of organisation which are imminent, let us steadily maintain that the geographical is a distinct standpoint from which to view, to analyse, and to group the facts of existence, and as such entitled to rank with the theological or philosophical, the linguistic, the mathematical, the physical, and the historical standpoints. No intellectual education is complete which does not offer some real insight from each of these positions.

# British Association for the Advancement of Science.

## IPSWICH, 1895.

# ADDRESS

### TO THE

# ECONOMIC SCIENCE AND STATISTICS SECTION,

### BY

## L. L. PRICE, Esq., M.A.,

#### PRESIDENT OF THE SECTION.

AT the Oxford meeting of the British Association a report was presented on the 'Methods of Economic Training in this and other Countries,'[1] the general conclusion of which pointed to a deficiency in this country in the organisation of instruction and the recognition given by the examinations of the Universities, of the public service, and the legal profession. In the spring of the present year Mr. Goschen, presiding at a dinner of the Economic Association, commented[2] on the inopportune contempt of the practical man for economic reasoning at a time when many of the questions engaging public attention were economic in character. The phenomena thus noted may be connected, and a disregard of economic reasoning explained by a lack of systematic economic instruction. At any rate, the members of this Section will scarcely feel more certain of the fact that the questions of the day are largely economic in character than of the illumination obtained by an acquaintance with Economic Science and Statistics. They may not succeed in winning the attention of the practical man, but they are not unlikely to find solace in the flattering conviction that the loss is on his side, and not on their own. The proceedings of the Section in this and in previous years will prove beyond dispute that, whether or not the practical man troubles himself to ascertain or to follow the opinion of the professors, the professors are not seldom busy in the consideration of the practical questions of the day.

I make this assertion with the more boldness because it requires no extraordinary keenness of vision to detect signs in the practical man of a disposition hardly consistent with the scorn he is prone to bestow. I believe that, in spite of what we may regard as his worse impulses, he manifests a growing inclination to seek counsel—and even imperatively to demand guidance—on social and political problems from economic professors. I do not know how otherwise to explain the fact that a well-known firm of London publishers has issued, and, I imagine, found it profitable to issue, a series of books on social subjects which now numbers upwards of eighty volumes. Many of these books may not be scientific in character, but so large an issue, taken in conjunction with other significant circumstances, such as the recent revival of a desire for economic lectures on the part of the clients of University Extension, does afford some presumption in favour of a fresh growth of popular interest. Indeed, I have heard more than one practical man complain, not that it was unreasonable to look for guidance in economic matters from economic experts, but that, with every disposition to hear the advice of professors, it was impossible to obtain it. This complaint may or may not be founded on reality; but the professors may be pardoned

[1] See *Report of the British Association* for 1894
[2] See *Economic Journal* for June 1895, vol v. No 18, p. 301.

if they regard it as a sign of a more wholesome condition of mind. The complaint may be due to the fact that the guidance sought is not such as the professors can offer, and that the advice, which they are able and ready to give, is considered inadequate or superfluous.

I am going to address myself to the audacious task of endeavouring to indicate by actual example the guidance which the economic professor may furnish to the practical man on the questions of the day; and I have prefaced my attempt with these observations to show that I am aware of the hazard and difficulty attendant. Were I to seek for an appropriate metaphor to describe my venture, I might find it by saying that I was about to disturb a hornets' nest; and, if I am fortunate enough to escape with the scornful neglect of the practical man, I am afraid that the professor may be less compassionate, and that his sting may prove as venomous. I may, perhaps, plead in excuse that it is at once the traditional privilege and the inherited duty of occupants of presidential chairs to devote their observations especially to that part of their science with which they have been most closely connected. I have certainly endeavoured on the one hand to bestow a considerable portion of my time on the scientific study of economics as expounded in systematic treatises, and, on the other, my occupation as College Treasurer has forced me into intimate contact with the hard facts of at least one department of practical life. I would not for one moment claim that this dual experience gives me any title to speak with authority on the relations of economic science to practical affairs; but it has determined the grooves in which my thoughts have mainly run, and, so far as I may presume to a special acquaintance with any department of economic speculation, it is with that which concerns the bearing of theory on practice. Without unbecoming arrogance, I may, perhaps, think that I possess in not very disproportionate measure the failings of the practical man and the academic professor; and in this capacity I undertake the task before me

Before considering some particular questions of the day we may determine the general character of the guidance offered by economics in matters of practice. I believe that in this connection economists must disclaim a pretension to strict neutrality. Much, no doubt, may be urged in support of the claim, and considerable advantages might follow from its successful establishment. The cool examination of heated questions in the dry light of science might seem the appropriate occupation of the academic professor. From the serene heights of tranquil speculation he might complacently look down on the heat and turmoil of affairs, and, standing apart from the conflict himself, refuse to assist any combatant. But the strict maintenance of this attitude is a 'counsel of perfection' and a practical impossibility. The student must be more or less than human who, dealing with a department of knowledge so intimately related to the welfare of humanity, can avoid, as the result of his scientific inquiry, forming a favourable view of one course of conduct and an adverse opinion of another, and endeavouring to promote the former, and to hinder the latter, both by advice and by act. He cannot be content to observe the connection of cause and effect without trying to set in motion the cause or to restrain its action. He cannot acquiesce in the speculative solution of a problem without being impelled to embody his theory in practice. He cannot contemplate the misery due to bad economic arrangements without seeking to devise and apply a remedy; and, viewing the matter historically, the practical object of benefiting their fellow-creatures has been at least as powerful a motive with great economic thinkers as the speculative aim of enlarging the boundaries of knowledge. They have been reproached for hardness of heart and dulness of imagination, and the popular account is prone to regard them as dry and unfeeling; but the description is a travesty of the facts, and their errors have probably been due as often to excess as to lack of enthusiasm. The recurring contrast of wealth and poverty, of careless ease and careworn want, of lavish indulgence and narrow penury, has awakened as responsive a chord in their hearts as in that of the most ardent and generous socialist; and it is impossible to run over the conspicuous names on the roll of economic worthies without being impressed by the warmth of their zeal for social reform, and the intensity and persistence of their anxiety to remove or mitigate human suffering. The 'economic

man' of popular description, whether or not he occupy a place in economic theory, is no portrait of the economist of actual historical fact. The name of 'dismal science,' so often misapplied, was suggested not so much by the suppression of human interest as by the apparent destruction of cherished hopes. The science was 'dismal,' not, as popular usage interprets the phrase, because it was dry and uninteresting, but because it seemed to counsel despair; and even then the title partook of caricature.

Nor do I think that in this connection an attitude of strict neutrality is desirable, if it be possible. The besetting sin of the academic temper is indecision, and few errors are more mischievous in practical affairs. An obstinate regard for neutrality may easily beget indecision, and from that moment the economist becomes ineffectual for practice. I must confess to the belief that the practical man has a right to demand an opinion on economic points from the academic professor, and that the professor has a claim to take part in the guidance of economic affairs which is derived from his scientific study. He is an expert, and it is no less his duty than his privilege to discharge an expert's functions. He cannot, as it seems to me, properly evade the one or abnegate the other. He may be careful in forming his judgment. He may conscientiously endeavour to assign its due weight to every circumstance. He may remember and insist that in many practical problems other aspects besides the economic must be considered. But the economic is often of great, and sometimes of paramount, importance; and on this he cannot disown the responsibility of making up his mind without, as it seems to me, forfeiting his own self-respect and his usefulness to others. From that moment his neutrality vanishes. He may, and probably will, incur an opprobrium which he might have avoided by a refusal to adopt a decisive opinion. He may sacrifice a quiet and ease which he might have retained. But, whether our aim be the correct conduct of affairs or the due recognition of economic science, I cannot doubt that he has chosen the better part. To insist on a strict neutrality for economists in matters of practice seems to me idle and misleading. It is idle, because the economist is human, and economics is concerned with some of the most important interests of human welfare. It is misleading, because it is the duty of the economic expert to offer guidance on economic points, and there are at all times few practical questions which do not present an economic side. Certainly at the present juncture, when the pressing problems of the hour are in many cases distinctly and admittedly economic in character, to attempt a divorce between theory and practice is especially inopportune. It is an impossible endeavour to saw a man into separate quantities; and I would claim for the appropriate description of every great economist the epitaph on the tomb of the German socialist, 'Ferdinand Lassalle, thinker and fighter.' We need not abandon the thought, but it should stimulate, and not paralyse, the action; for the one is not fully complete until it is realised in the other. Economics is indeed a science, and on that ground claims a recognised place in the programme of this Association; but it is essentially, as I think, an applied, and not a pure, science, and the economist has only fulfilled part of his mission when he has solved a speculative problem. I am aware that this contention may not be admitted by many academic professors and practical men, but I believe that it is in accord with historical tradition, and admits of logical justification.

Yet, if an attitude of strict neutrality be impossible and ineffective, the opposite extreme of dogmatic assertion is as undesirable as it is dangerous. The older economists have been often charged with an error of this nature, and it cannot be denied that the accusation rests on a basis of truth, though it has sometimes been couched in exaggerated form. Certainly the modern economist is inclined to state his opinion with less assurance; and for that very reason he has lost some of his influence on practical affairs. For the practical man has a sneaking affection, and even respect, for dogmatic assertion. At any rate, he desires a plain, direct, and concise answer to his questions, and it is not easy to distinguish between an avoidance of dogmatism and an appearance of indecision. Nor can it be denied that, as a discipline of the mind, a study of the more abstract reasonings of some of the older writers, which generally presented the semblance, and sometimes offered the

reality, of a precise, defined, consistent whole, is both wholesome and stimulating. Legal authorities now pronounce inadequate Austin's 'Lectures on Jurisprudence,' but I must confess that I look back to my first acquaintance with them as an epoch in my mental history. I believe that they acted as a tonic and purgative, clearing away obscurity and stimulating intellectual effort. If I may say so, the effect of reading such an author as James Mill is not unlikely to be similar in the case of the young economic student; and for that reason, were there no other, I should personally regret the exclusion from a systematic economic course of the study of some of the more rigidly abstract reasonings of some of the more strict of the older economists. Such study may be regarded as a propædeutic, through which the student should pass; and he will lose, and not gain, by its omission. The regimen may be somewhat severe, and the diet, so far as the moment is concerned, not very nutritious; but the system is braced and the digestion strengthened.

The fact, however, is that the more famous of the older economists were themselves less abstract and precise than they are represented in common opinion. They took a keen and constant interest in the practical questions of their time. Their speculative opinions were largely influenced by the prominent facts of their day. The acumen of later, and even contemporary, criticism has discovered gaps in some of their reasonings and inconsistencies—which perhaps do them honour—in some of their arguments. Recent economic analysis certainly endeavours to bring within its range a larger number of facts, to be more explicit in stating and repeating the assumptions on which it proceeds, and to be more cautious in establishing conclusions and definite in limiting their application. But the change is largely due to the increasing complexity of the facts; and the difference in the mode of approaching and method of handling a question is one of degree rather than kind. The particular problems which confronted the older writers admitted more often of a plain dogmatic answer; and, if the deliberations of the later economist be more comprehensive and protracted, his conclusions need not on that account be indecisive. Indeed, with the lapse of time, the necessity and advantage of expert advice have grown more obvious and urgent.

What, then, is the general character of that advice? The answer may seem a truism, but it is surely this. As in other departments of study, the mission of the scientific economist is to discern, and to assist others to recognise, the unseen. He is not content with a superficial view. He endeavours to penetrate below the surface of affairs and discover the invisible forces. He employs telescope and microscope to bring within the range of vision what is distant or unnoticed. He compels the practical man to pay attention to something more than the obvious and immediate consequences of the policy he is pursuing; and the chief advantage of economics as part of a scheme of general education seems to consist in inducing a habit of mind which will not be satisfied with superficial explanation. And it induces this habit in matters with which men and women are brought into close and necessary contact in the ordinary routine of everyday life. They may flatter themselves that common-sense alone is needed to deal with such matters, and that no scientific training or aid is required. Economics dispels this subtle and dangerous illusion, and furnishes an instrument which at once controls and strengthens common-sense. Nor is this claim for economics as a discipline of the mind and as a guide in matters of practical conduct by any means novel. It was put forward with prominence by Bastiat, whose writing is sometimes regarded as an illustrative example of the application of orthodox economics to the treatment of an important practical question. It has been recently adduced by the Duke of Argyll, who, dissatisfied with what he considers orthodox economics, attempts to supply its defects by disclosing the 'Unseen Foundations of Society.' The arguments and conclusions of Bastiat may not be accepted, the criticisms of the Duke may be refuted, by contemporary economists, who may claim the title of orthodox, if they desire an epithet which seems to bring as much opprobrium as honour; but they would certainly agree with the earlier exponent and the later critic, who, curiously enough, have not a little else in common, in regarding the mission of economics as an endeavour to see, and to reveal to others, the unseen.

That such a description is no barren truism, that economics thus conceived

may shed illumination on dark or obscured problems, that it may prove, in Bacon's language, not merely *lucifera*, but also *fructifera*, may, I think, be shown by a brief consideration of some typical questions of the day.

I. Few are more prominent than that of industrial strife. We deplore its occurrence, and are ready to welcome any promising means suggested for mitigation or prevention. Nor does popular opinion refuse to economics a voice in the matter; but, on the contrary, its authority is continually invoked. What, then, in accordance with the principles we have sought to establish, is the guidance which it can offer? Are there any common beliefs which it may show to be superficially-founded? Few assertions certainly are more frequent than that the interests of employers and employed are harmonious, and that disputes involve a disturbance of this fundamental harmony. On the other hand, few facts are more obvious than that employer and employed regard their interests as essentially antagonistic, and from this antagonism the disputes have arisen. Economics is able to show that either view expresses a portion, and only a portion, of the truth; and, by the systematic mould in which its reasoning is cast, it brings into clear relief the relation of the complementary truths.

In the production of wealth the interests of the parties harmonise, for, with the modern organisation of industry they require the services of one another, and, the more efficient they respectively are, the larger is likely to be the wealth produced. It is the interest of the employer that the wages earned by the men should be adequate to maintain, and, if possible, to increase, their efficiency; and it is the interest of the employed that the profits of the *entrepreneur* should encourage enterprise and induce a sufficient supply of capital. For production—and this is a point which economics, and economics alone, can duly emphasise—is the ultimate source of the wealth distributed. The larger the amount produced, the larger, *cæteris paribus*, is likely to be the share of either party in distribution; and in any event it is certain that a decreased production must issue in effects on distribution, the burden of which will fall, though in varying measure, on either party. The influence thus exerted on distribution by production is one which workmen seem especially likely to forget, and many of the common arguments in favour of 'making work,' or providing 'employment for the unemployed,' proceed from ignorance or neglect of this consideration.

On the other hand, it may be urged that employers are not very keen to recognise the influence, whether for advantage or drawback, of distribution on production. No doubt the division of economics into separate departments tends to make even the student forget their mutual connection. We do not remember constantly that production and distribution are simultaneous, and are only distinguished for purposes of convenient analysis. Yet one of the most important advances of recent economics consists in the emphasis given to the influence of distribution on production; and we see more clearly than our predecessors how the poverty of the poor, by begetting inefficiency, may cause their poverty, and high wages may imply, not a high, but a low, cost of production. Either of these truths may be pushed to excess; but they are certainly fraught with important consequences, and have an intimate bearing on the question before us. But, like the influence of production on distribution, the telescope of the economist is needed to bring and retain them within the range of ordinary vision.

The full and constant recognition of these truths conduces to a more comprehensive conception of the possible results of industrial disputes. We can see, on the one hand, that a victory for the moment may not prevent defeat in the long run, and that loss, which is obvious at the time, may issue in ultimate gain. When we remember that to discern these distant results the naked eye of the plain observer seems incompetent without the aid of the economic organon, we are as ready to recognise the likelihood of industrial conflict as we are anxious to devise the means of preventing it. For in the distribution of wealth the apparent interests of the two parties are antagonistic, and, given the amount produced, the larger the share of the one, the less will be that of the other. The frank recognition of this possible antagonism is the first step towards the prevention of its natural consequences. The imminence of the possibility supplies the strongest motive for

removing unnecessary hindrance, and furnishing likely assistance, to a pacific agreement. And, whatever the final consequences of a dispute to the interests of either party, the existence of friction and irritation is beyond question an injury and hindrance to production. The loss thus occasioned is immediate as well as distant, and may be considerable; but, if the telescope of the economist is generally needed to bring sufficiently close the ultimate effects of industrial disputes, his microscope is sometimes required to magnify the results of friction to dimensions which will attract and retain the attention of the ordinary observer. By discovering these deeper considerations beneath the superficial appearance of affairs economics may furnish useful guidance in the prevention and adjustment of industrial disputes.

For to what conclusion do these considerations lead? To the discovery of some machinery which may prove not unacceptable, and yet, by imposing delay on the outbreak of strife, may allow the two parties to hear what either has to urge, and to consider the possible consequences of the action they are proposing to take. Such a machinery may be discovered in boards of conciliation and courts of arbitration. The fact that both sides should be organised on a sufficiently responsible basis to send accredited representatives; the fact that, thus meeting one another, they are compelled to seek and adduce reasons for their own position and to listen to the arguments in support of their opponents; the fact that delay and deliberation are recognised preliminaries to the commencement of war—these facts may not appear important in themselves, but they offer a chance of pacific adjustment, and afford opportunity for the consideration of ulterior issues. They prevent the apparent interests of the moment from winning an undisputed victory over the less obvious interests of the future; and they do not allow an advantage in distribution to be secured without thought of the effects on production. On the other hand, the antagonism of interests incident to the distribution of wealth, when the production is regarded as a given quantity, suggests that the machinery may on occasions break down, and that the arrangements should properly consist of different stages and provide supplementary resources; for arbitration may succeed in adjusting a dispute to which conciliation has proved incompetent, and conciliation may conceivably be useful where arbitration has been ineffectually tried. Such antagonism also suggests that voluntary adhesion is likely to be more abiding than compulsion, and more conducive to the permanent interests of peace, and that to prevent the occurrence, or reduce the likelihood, of industrial conflict a traditional standard of settlement, changed in grave emergencies or serious vicissitudes alone, should be established in the trade and recognised as fair.

For economics, as it seems to me, can do little more than point out those ultimate and obscure consequences which are concealed by immediate superficial appearances; and it is not in possession of a precise principle or rule, which can be definitely applied to the determination of industrial disputes. Could it, indeed, furnish such a rule, the argument in favour of the legal bestowal of compulsory powers on courts of arbitration and boards of conciliation would gain considerable strength; for it must be remembered that the questions before them are not the interpretation of past contracts, such as are habitually submitted to the Continental Conseils de Prud'hommes, but the establishment of agreements for the future, and it is difficult to force parties to agree when you do not supply a principle of agreement, nor does it on the whole seem likely to conduce to conciliatory relations to declare that, while you will not compel masters and men to agree, you will compel them to abide at all hazards by the agreement to which they may come. For these reasons, tempting as it undoubtedly is to invoke legal compulsion, I believe that the State can do little more than supply facilities for voluntary agreement, and, exercising, perhaps, some gentle persuasion, leave the pressure of public opinion to induce recourse to machinery thus provided. Such I take to be the drift of competent experienced opinion and the probable scope of effective legislation; and such, as it seems to me, is the kind of guidance which economics can offer on this practical question.

II. In a town like Ipswich we are forcibly reminded of another question of the day—I mean agricultural depression. From the Reports of the Assistant-Com-

missioners to the Royal Commission it would appear that the county of Suffolk shares with its neighbour, Essex, an unenvied pre-eminence among districts which have suffered, and that the present condition of this important industry borders here on despair In the actual words of Mr. Wilson Fox, the Assistant-Commissioner, agriculture in Suffolk 'is well-nigh strangled.'[1] Can economics throw any light on this lamentable situation? If there is one theory which is supposed to be more remote from fact than another, it is the theory of rent. It is the fashion, even with professed economists, to regard it as unduly abstract; and, in a recent address [2] to a learned society connected with this Section by no distant ties, the President selected the theory as a conspicuous example of older formulæ laid aside. The account of the theory given in that address is open to question, but the ground of rejection is worthy of note. Lord Farrer, it would seem, condemns the theory because it is a 'formula useless for practical purposes.' This criticism raises the question we are now considering; for we are trying to ascertain the guidance which economic science can furnish in practical affairs. That it has an important, and, indeed, a necessary, relation to practice we have asserted in positive terms; but the relation is not, as we think, that which Lord Farrer apparently assumes. For economics does not furnish precepts or formulæ immediately applicable to practice, but it supplies systematised knowledge, the possession and employment of which will afford assistance in the direction of practical affairs. The theory of rent is not, then, a maxim of conduct but a rational explanation of fact. Conceived thus, in my own experience as College Treasurer, I have been struck by its pertinence, not its inadequacy. It has certainly seemed to me that, on a broad view, the tenant considers the rent to be properly that which is left when, on an average of years, he has reaped a fair profit and paid his labourers the wages they command. The landlord, so far as I have been able to discover, occupies in his eyes the position—to use language differently applied [3] by General Walker—of a 'residual claimant'; and such, also, as I read the theory, is the place which he fills therein.

Nor is it difficult to interpret part of the present depression in conformity with the theory of rent. I must take leave to dissent from Lord Farrer when he asserts that the formula, even in its older shape, paid no regard to situation or to means of transport; and I am disposed to affirm that the emended statement of recent text-books, in which these considerations, with others mentioned by Lord Farrer, receive explicit recognition, is not so much a departure from the older form as a development and extension of it. But, taking the two points of fertility and situation alone, it is the agreement, and not the conflict, of what has happened with what the theory might have led us to expect that is likely to impress. It can hardly be doubted that one of the most remarkable changes of recent years has been the development of the means and reduction of the cost of transportation. This change implies a loss of the advantage derived from proximity to the market in the case of commodities which admit of conveyance from a distance. Interpreted in the language of the theory of rent, English land, as respects certain products, has forfeited part of the natural protection afforded by its situation near to the market. With the partial loss of this advantage has also disappeared part of another, for the diminution in the cost of transportation has opened European markets to the virgin soils of America and other countries; and, with regard to products which admit of conveyance from a distance, the fertility of English land, whether it be due to the skill of generations of comparatively high farming or to natural qualities of soil, has lost part of its advantage. In the language of the theory of rent, the forfeiture of these two advantages involves depression in the sense of a decrease of rental; and, as it seems to me, the adequacy rather than deficiency of the theory is evident as a rational explanation of fact

Nor is it useless for practical guidance. The fact which it establishes is a

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[1] Cf *Report*, p. 82

[2] Cf *Journal of the Royal Statistical Society*, vol. lvii. Part IV., December 1894, pp 595, &c.

[3] *I e*, to the wage-earner. Cf. *Political Economy*. by F A. Walker, Part IV.

connection between cause and effect, and not a maxim of conduct; for the laws of economics, like the laws of every science, are, as it has been aptly expressed, statements in the indicative and not the imperative mood. But the possession of the scientific knowledge of the causal connection is more likely than its absence to conduce to prudent practice In the instance before us the conclusion seems inevitable that, so far as the depression is due to foreign competition, and the virginity of competing soils, and facility of transportation, continued attention to products, which must be conveyed to their market quickly, is likely to be more profitable in an old country like England than the continued production of commodities, which can be raised to greater advantage on newer soils, and be easily transported from considerable distances. I am aware that this is a hard saying, that necessary conditions of cultivation, sometimes neglected, must be taken into account, and that such a change as is often contemplated in such discussions may mean a painful and difficult departure from traditional habit, and an apparent sacrifice of inherited or acquired skill. It is easy to talk glibly of the English farmer abandoning the cultivation of cereals, at any rate as a staple product, and turning his attention to vegetable and dairy produce, to fruit-raising and poultry-rearing and bee-keeping, and the various other modes of making a fortune which are put forward for his edification But the lesson of economic theory is plain so far as the depression is due to foreign competition and the maintenance of a Free Trade policy is assumed.

I do not discuss the latter question, because it is far too large to be adequately treated in a paragraph or two, and is excluded from practical politics by the leaders of political parties. but it is certainly a question on which economics may reveal the unseen. Among those invisible facts may perhaps be placed a circumstance often neglected in popular discussion. In many arguments on agricultural depression the landed interest is treated as strictly separable from the rest of the community, and a fall in rent is regarded as the loss to a particular class of an advantage enjoyed apart from exertion.

If I may say so in passing, some of the abundant popular use made in recent years of the conception of rent as an unearned increment seems to me to afford an example of the misapplication of theory to practice. For in not a few instances what has happened is this: A theory resting on nice distinctions has been crudely applied to practice, and the distinctions employed to prescribe a definite policy without regard to their nicety. In other words, the theory has been used as a precise maxim, which could be straightway embodied in practice, and not as a scientific conception, the knowledge of which might protect the practical man from hidden pitfalls.

In England, at least so far as agricultural land is concerned, the landlord is usually a partner with the tenant, and, whether or not the system be better than that of occupying-ownership, it is certain that part of the rent is a return for expenditure, and not a payment for natural qualities of soil or situation; and to this extent a fall in rent is likely to operate as a discouragement or preventive of the fresh and continuous expenditure needed to maintain the land and the buildings in a state of efficiency. I cannot doubt, in view of evidence given before the Commission on Agriculture, and of other signs, that the depression must have already produced deterioration in this respect, and thus have injuriously affected the economic position of the general community.

Nor is the landed interest strictly and entirely distinct. In an old country different classes are connected with one another by ties hard to disentangle, and impossible to sever without injury or danger. The educational endowments of the country, as a melancholy personal experience has taught me,[1] cannot regard agricultural depression with the complacency of disinterested observers. The effect on certain public institutions, like some of the London hospitals, is notorious; and it can scarcely be doubted that, though the prudent management by which they are characterised may have led many of the great insurance companies to write down

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[1] *Cf.* papers read by the present author to the Royal Statistical Society in February, 1892, and January, 1895.

their landed investments and withdraw from them as they are able, yet they have been, and are, largely interested in the fortunes of landed property, and perhaps especially in the rentals of landlords, on the security of which they have made advances. With the stability of the insurance companies is linked the preservation of perhaps the bulk of the savings of the professional classes. In short, in an old country a strict separation between the interests of different classes is only true with large deductions.

III. But economics also raises and solves the doubt whether depression in agriculture can be attributed to foreign competition alone. It is a significant fact that, according to authoritative accounts, many of the competitors of the English farmer have not escaped the distress from which he has suffered, and in England the depression, in spite of constant reductions and abatements, has exerted an influence on profits scarcely less grievous than that on rents. These circumstances certainly lend weight to the contention that the fall of prices, which is not peculiar, though perhaps especially discouraging, to agriculture, is partly due, to state the matter in the least controversial shape, to a change in the general relation between the supplies of gold and the monetary work that it is required to perform. To the discovery of a cause like this, hidden from superficial view, and to the indication of the manner in which it may affect the position of agriculture and other industries, economics, by virtue of its mission to discern the unseen, is peculiarly competent. I do not propose to enter now at any length on the vexed question of the currency, but it is certainly a prominent practical question of the day. It is a question on which the economist may claim to speak with authority, and the practical man may demand, as he may be expected to follow. the definitive guidance of expert opinion. On this question, perhaps, in particular, the unassisted vision of the naked eye may form erroneous conclusions, and derive no little profit from the use of the optical instruments provided by the economist.

I cannot preface what I propose to say more appropriately than by a quotation from Jevons In that pamphlet on ' A Serious Fall in the Value of Gold ' [1] which has attained the rare dignity of an economic classic, commenting on the alarmist anticipations of Chevalier and Cobden, he remarks that the alteration in the value of gold consequent on the discoveries in California and Australia would probably be ' most gradual and gentle.' ' Far from taking place with sudden and painful starts, flinging the rich headlong to a lower station, and shaking the groundwork of society, nothing is more insidious, slow, and imperceptible. It is insidious because we are accustomed to use the standard as invariable, and to measure the changes of other things by it; and a rise in the price of any article, when observed, is naturally attributed to a hundred other causes than the true one. It is slow because the total accumulations of gold in use are but little increased by the additions of any one or of several years. It is imperceptible because the slow rise of prices due to gold depreciation is disturbed by much more sudden and considerable, but temporary, fluctuations which are due to commercial causes, and are by no means a novelty.' I propose to apply briefly these remarks of Jevons to some aspects of the controversy which has arisen on the cause of the fall of prices of the last twenty years.

It is, for example, sometimes asserted that the influence of credit on prices is so considerable as to reduce to unimportance a decrease in the available supplies of gold. It may at once be admitted that the modern extensive development of credit obscures the relation between the metal and prices; but it does not destroy it, and, according to the view we have been trying to emphasise, the mission of economics is to remove this veil of obscurity. In this instance it may show that the relation is not unreal because it is indirect; that credit, expanding and contracting of itself owing to increasing or diminishing speculative activity, is yet limited and controlled in its movements by the changing dimensions in the basis of cash on which it rests; and that, through the bank reserves meeting or restricting the demands for petty cash and permitting an expansion or causing a curtailment of credit, the supplies of the standard metal exert an important influence on prices.[2]

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[1] Cf *Investigations in Currency and Finance*, pp. 78, 79.
[2] *Cf*. Giffen's *Essays in Finance*, Second Series, II.

Economics may thus furnish a rational account of the *modus operandi*, and statistics supply corroborative evidence  This evidence, indeed, may be said to amount to ocular demonstration, for no one who has studied with moderate attention the course of a curve of general prices over a period of time, drawn in accordance with the graphic method of statistics, can have failed to distinguish the different character of the fluctuations thereby shown—to have separated the more obvious and pronounced fluctuations of credit, marking the flow and ebb of confidence, from the minor passing changes due to some temporary accident of demand and supply on the one hand, and, on the other, from the general trend of the curve indicating a growing abundance or scarcity in the available supplies of the standard metal.  This is a broad influence, the operation of which is only discernible on a comprehensive view; but the graphic method of statistics brings it within the range of ordinary vision, and the reasoning of economics discloses the connection of the phenomena.  The influence of credit is apparent on the surface, but the deeper influence can be detected beneath ; and, if the general level of one credit cycle be higher or lower than another, the change points to the presence and action of some less obvious cause.  In a modern commercial society, with its development of banking and credit, we are able to observe and to measure cause and effect.  At the one end of the process we possess statistics of the production of gold, and can frame estimates of the amount and character of extraordinary demands.[1]  At the other end we can employ, in the form of index numbers, as they are called,[2] a means of measuring changes in general prices, which is certainly adequate to show the direction of the change, if it is not competent to indicate its precise amount.  For the connection between cause and effect we look to economic reasoning, which here, as elsewhere, enables us to discern the unseen.

A similar test may be applied to the adequacy of some other causes.  It is sometimes said that a complete explanation of changes in general prices can be discovered in the particular circumstances of individual commodities, without any reference to a common cause.  The answer is evident on the principles we have been endeavouring to establish.  Those particular circumstances lie on the surface, and the common cause is only apparent if we penetrate beneath.  Here, again, economics is aided by statistics.  Economics can recognise and explain the operation of a common cause in enhancing or diminishing the effect of particular circumstances, and statistics can offer corroborative evidence of the presence of such a cause. For the very meaning and intention of a statistical average is to eliminate the influence of particular causes, and therefore the testimony of those index numbers, in which an attempt is made to exhibit the average change in prices, is adequate to establish the influence of some common cause, if the basis on which they are constructed be sufficiently comprehensive and typical.  It can scarcely be doubted that this criterion is satisfied in the case of some of the best-known varieties.  The presence of that common cause, it must be remembered, does not imply the absence of other contributory or counteracting causes ; and the inquirer in the region of the moral and political sciences is always beset by difficulties arising from the plurality of causes  But, if he can establish the presence of a common cause competent to produce the effect, and can point to the effect which has been produced, the argument for the connection between the two attains that high degree of probability which is all that we can expect to reach.  In the instance before us statistics may show the presence of this common cause and the occurrence of the effect, and economics may indicate the competence of the cause to produce the effect.  We know that until recently the production of gold had declined from the level reached in the middle of the century, and we are aware that a series of extraordinary demands [3] had coincided, while various index numbers are in general agreement in

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[1] To some extent also this is true of the changes in the ordinary demands, but the stress of the argument may be laid on the extraordinary demands.

[2] *Cf.* those compiled by the *Economist*, Mr. Palgrave, Mr. Sauerbeck, Dr. Soetbeer, and Sir Robert Giffen.

[3] *E g* , on the part of Germany, the United States, the Austro-Hungarian Empire, and Russia.

exhibiting a fall in prices, though the degree of the fall shown in each case may vary.[1] The economic theory of supply and demand may, then, be used to establish the connection between cause and effect; for, if the supply of a commodity declines while the demand for it increases, a rise in its value, and a fall in the value of articles compared with it, become inevitable. Such has been the position of gold during the last twenty years.

It may be noticed that the possibility of a plurality of causes increases the likelihood of the action of some common cause; for, under the conditions, we cannot expect the apparent effects of this cause to be immediate or universal The presence of counteracting or modifying circumstance, of opposing or contributory causes, will delay in some cases a process accelerated in others, will minimise here an effect which is accentuated there. The apparent change due to the cause is only likely to be general and not universal, to be gradual and not immediate. The assertion that a fall in prices, if due to an alteration in the available supplies of the standard metal, should be immediate and universal cannot be sustained when economics, penetrating beneath superficial appearance, reveals the interaction of different causes; and, if the testimony of index numbers points to a general change, it is no sufficient answer to affirm that it is not universal On grounds of economic reasoning we should expect a slower movement of retail than of wholesale prices, of the prices of articles of minor than of those of more general consumption, of wages than of prices generally.

The mention of wages suggests another point neglected in some current discussions, but brought by economic reasoning from obscurity into prominence. It is sometimes asserted that the fact that wages have not fallen is a proof that monetary causes have not produced the fall of prices. But, apart from the known tendency of wages to move more slowly than prices, such an assertion overlooks the possibility of a simultaneous change in distribution. Economic reasoning points to the probability of such a change in the wage-earner, and to the effect that it would produce, and statistical evidence corroborates that reasoning.[2] If such a change be proceeding, we should expect wages to rise, and the fact that they are stationary tends to prove, not to disprove, the existence of a monetary cause of the fall of prices. A failure to give explicit recognition to this possibility is due to neglect of the plurality of causes, and is akin to another argument sometimes advanced. This maintains that, if it can be shown that the country has progressed, or not receded, in wealth, in the development of trade and manufacture, n the prosperity of the mass of the community, it is thereby proved that the fall of prices has wrought no injury. But it may be answered that the progress might have been greater in the absence of the fall, and other forces may have prevented the cause in question from producing its full effect. Here, again, economic reasoning may aid in discerning what is invisible to the unassisted eye.

Few truths, indeed, are slower to receive, and more likely to lose, popular recognition than those which lay stress on the mutual action of different causes. We are told, for instance, that the fall of prices is due to circumstances connected with improvements in the production and transportation of commodities; and it must be admitted that such a common cause is not, like particular causes affecting individual commodities, eliminated in the general average of the index numbers. But the one common cause—that of improvements in production—does not exclude the operation of the other—that of a change in the available supplies of gold. Taking a broad view of the whole century, it would certainly seem that the movement of improvement has set steadily in one direction, but that the movement of prices has first declined, and then advanced, and then declined again. It is possible that the movement of improvement may have been accelerated and retarded at different times; but the change in the movement of prices, which requires ex-

[1] An index number may be briefly described as a mode of showing the average change in prices by comprising in one grand total the percentages of rise or fall shown in the recorded prices of certain selected typical commodities

[2] Cf the investigations of Sir Robert Giffen in England, of M Leroy-Beaulieu in France, and of other inquirers in other countries.

planation, is not a variation of degree, but a reversal of direction. And this reversal coincides with similar changes in the available supplies of the standard metal. If the disturbances in America at the beginning of the century, with the known diminution in production, were followed by a fall, if the Californian and Australian discoveries of the middle of the century were accompanied by a rise, and if the notorious extraordinary demands since 1873, statistically computed by Sir Robert Giffen,[1] coming on a supply which until the past few years was diminishing, coincided with a fall again, it seems impossible to doubt that, although improvements in production and transportation may have been contributory causes, an important influence has been exerted by the monetary supplies. With the aid of the economic telescope and microscope forces too remote or obscure to be detected by the naked eye are thus brought within the range of ordinary vision; and the action of the standard metal on prices is one of those forces; for, in Jevons's language, it is 'insidious, slow and imperceptible.'

Such is the guidance which, as it seems to me, economics is able to offer; and in this question of the currency, as in the others of which we have treated, it is surely not destitute of practical import, for the detection of a monetary cause of the fall in prices is so far an argument for the adoption of a monetary remedy. Such guidance also, I believe, economics can furnish on many other questions coming to the front; and, in offering this, it cannot be accused of an excessive or defective estimate of its claims to popular recognition. I am convinced that, as the years elapse, its aid will be sought with increasing urgency, and that it will discharge, with a fuller consciousness of its high prerogative, its important but difficult mission of seeing for itself, and disclosing to others, the unseen.

[1] In evidence given before the Gold and Silver Commission, and, more recently, before the Commission on Agriculture.

# British Association for the Advancement of Science.

# ADDRESS

## TO THE

# MECHANICAL SCIENCE SECTION

### BY

L. F. VERNON-HARCOURT, M.A., M.Inst.C.E., PRESIDENT OF
THE SECTION.

## *The Relation of Engineering to Science.*

THE selection of a subject for an inaugural address, necessitated by the honour conferred upon me of presiding over this Section, has been rendered peculiarly difficult, both on account of the numerous able addresses delivered in past years by my eminent predecessors in this office, and also by the circumstance that the branches of engineering to which most of my professional life has been devoted have not as intimate a connection with mechanical science as some others. Moreover, whilst former Presidents of Section G have frequently dealt, in their addresses, with the progress of those special branches of engineering in which they have had most practical experience, such a course, in the present instance, would have exposed me to the danger of merely repeating information and reiterating opinions already recorded in the ' Proceedings of the Institution of Civil Engineers,' and in other publications, with reference to maritime and hydraulic engineering. It has, accordingly, appeared to me that the exceptional occasion of addressing a gathering of scientific persons, and of engineers who testify their interest in science by attending these meetings, would be best utilised by considering the relation, that engineering in general, and maritime and hydraulic engineering in particular, bear to pure science, and the means by which progress in engineering science might be best promoted, and its scope and utility increased.

In addition to the oft-quoted definition of civil engineering as 'the art of directing the great sources of power in nature for the use and convenience of man,' Thomas Tredgold also defined it, in 1828, as ' that practical application of the most important principles of natural philosophy which has, in a considerable degree, realised the anticipations of Bacon and changed the aspect and state of affairs in the whole world.' If the influence of engineering could be thus described in 1828, when railways and steamships were in their infancy, and the electric telegraph, and the various modern applications of electricity and magnetism had not come into existence, how far more true is it at the present day, when the various branches of engineering have attained such a marvellous development! Tredgold also realised, at that early date, that the resources of the engineer must be further directed so as to cope with the injurious forces of nature, such as floods, storms, and unsanitary conditions, and thus protect men from harm as well as promote their well-being. Moreover, he foresaw the great capabilities of development possessed by engineering, and its dependence on science; for he stated that ' the real extent to which civil engineering may be applied, is limited only by the progress of science; its scope and utility will be increased with every discovery in philosophy, and its resources with every invention in mechanical or chemical art,

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since its bounds are unlimited, and equally so must be the researches of its professors.' If the full significance of these statements may be accepted as correct, engineers might fairly claim to have a right to say, ' As engineers we are necessarily men of science, and no branch of science is outside our province.' It might, however, be said that no engineer, with his absorbing professional avocations, would have the time to acquire even the rudiments of the principal branches of science, with their ever-increasing developments, to the study of each of which the life-work of many earnest searchers into the secrets of nature is wholly devoted. Nevertheless, a few branches of science, such as physiology, biology, and botany, appear to be beyond the scope of practical engineering; whilst a moderate acquaintance with some others might suffice for the needs of the engineer, except in certain special branches, supplemented as it can readily be by the advice of a specialist in complicated cases.

Among the branches of science necessary for the engineer, two may be regarded as of the highest importance, namely, mathematics and physics, upon which the science of engineering mainly depends; and without an adequate knowledge of these no person should be able at the present day to enter the profession of a civil engineer. Other sciences of considerable, though of comparatively minor, importance to engineers in general, are chemistry, geology, and meteorology; but each of these assumes an enhanced value in special branches of engineering.

*Mathematics in Relation to Engineering* —The pre-eminent importance of mathematics in relation to engineering may be accepted as fully established; and a President of the Institution of Civil Engineers would not now tell a pupil, at their first interview, that he had done very well without mathematics, a remark made to me by a justly celebrated engineer over thirty years ago.

Surveying, which is the handmaid of civil engineering, depends upon the principles of geometry for its accuracy; and ordinary triangulation, geodesy, and the rapid method of surveying and taking levels in rough country, known as tacheometry, are based on trigonometry and aided by logarithms. Tacheometry, indeed, though carried out by means of a specially constructed theodolite, may be regarded as the practical application of the familiar problem in trigonometry of finding the height and distance of an inaccessible tower. A proposition of Euclid forms the basis of the simplest and speediest method of setting out circular curves for railways; whilst astronomy has been resorted to for facilitating surveying in unexplored regions. The laws of statics are involved in the design of bridges, especially those of large span, and also of masonry dams, roofs, floors, columns, and other structures; whilst torsion, internal ballistics, the trajectory of a projectile, the forces of impact, and the stoppage of a railway train are dynamical problems. Hydrostatics and hydrodynamics provide the foundation of hydraulic engineering; though, owing to the complicated nature of the flow of water, observations and experiments have been necessary for obtaining correct formulæ of discharge. Geometrical optics has been employed for determining the forms of the lenses for giving a parallel direction to the rays proceeding from the lamps of a lighthouse, in accordance with the principles laid down by Fresnel. The theory of the tides, the tide tables giving the predicted tidal rise at the principal ports, and wave motion—questions of considerable importance to the harbour engineer—depend upon mathematical and astronomical calculations; whilst the stability and rolling of ships, the lines for a vessel of least resistance in passing through water, and the dimensions and form of screw-propellers, to obtain the greatest speed with a given expenditure of power, have been determined by mathematical considerations aided by experiment. Electrical engineering depends very largely upon mathematical and physical problems, guided by the results of practical experience; and the possibility of the commercial success of the first Atlantic cable, depending upon the rate of transmission of the signals and the loss of electrical intensity in that long journey, has been shown by Dr. John Hopkinson, in his ' James Forrest ' lecture, to have been determined by Lord Kelvin by the solution of a partial differential equation.[1]

All branches of applied mathematics have, accordingly, been utilised by

[1] *Proceedings Inst. C.E.*, vol. 118, p. 339

engineers, or, as in the case of several general principles and tidal calculations, by mathematicians to their benefit; but graphic statics will probably gradually supersede analytical methods for the calculation of stresses, as more rapid in operation, and less subject to errors, which are also more easily detected in graphic diagrams. Pure mathematics, in its higher branches, appears to have a less direct connection with engineering; but applied mathematics is so largely dependent upon pure mathematics, that the latter, including the calculus and differential equations, cannot be safely neglected by the engineer, though certain branches, as, for instance, probabilities, the theory of numbers, the tracing of curves, and some of the more abstruse portions of the subject, may be dispensed with.

*Physics in Relation to Engineering.*—Physics has been placed after mathematics, as many physical problems are determined by mathematics; but in several respects physics, with its very wide scope in its relation to the various properties of matter, is of equal importance to engineers, for there are few problems in engineering in which no part is borne by physical considerations.

The surveyor avails himself of physics when heights are measured by the barometer, or by the temperature at which water boils; and the spirit-level is a physical instrument adapted by the surveyor for levelling across land. Evaporation, condensation, and latent heat are of great importance in regard to the efficiency of steam-engines; and the expansive force of the gases generated or exploded, the diminution of friction, and the retention of the heat developed are essential elements in the economical working of heat-engines. Allowance for expansion by heat and contraction by cold has to be made in all large structures; and deflections due to changes in temperature have to be taken into account. The temperature, also, which decreases with the elevation above the sea-level, and the distance from the equator, limits the height to which railways can be carried without danger of blocking by snow; whilst the temperature, by increasing about $1°$ Fahr. with every sixty feet below the surface of the earth, limits the depth at which tunnels can be driven under high mountain ranges. Congelation of the soil is employed, as will be explained by Monsieur Gobert, in excavations through water-bearing strata.

Compressed air is used by engineers for excluding the water from sub-aqueous foundations, so that excavations can be made and foundations laid, at considerable depths below the water-level, with the same certainty as on dry land. The compression of air, and its subsequent absorption of heat on being liberated and expanding in a chamber, are employed for refrigerating the chambers in which meat and other perishable supplies are preserved. Compressed air is employed for working the boring machinery in driving long tunnels through rock, and provides, at the same time, means of ventilation; and it also serves to convey parcels along pneumatic underground tubes. Moreover, the compressed-air and vacuum brakes are the most efficient systems of automatic continuous brakes, which have done so much to promote safety in railway travelling, and in reducing the loss of time in the pulling up of frequently stopping trains. The production of a more perfect vacuum than can be produced by the ordinary air-pump, might have been supposed to be merely an interesting physical result;[1] but, in fact, the preservation of the heated filament of carbon in the incandescent electric light has been rendered possible only by the far more perfect vacuum obtained by the Sprengel vacuum-pump, by which the air is exhausted down to so low a pressure as one-two hundred millionth of an atmosphere.

The illuminating power of different sources of light is of great importance in determining the distance at which the concentrated rays from a lighthouse can be rendered visible, as well as in relation to the lighting of streets and houses; and the refrangibility of the rays emitted, or the nature of their spectrum, should not be disregarded, as upon this depends the power of a light to penetrate mist and fog, which cut off the rays at the violet end of the spectrum, and have comparatively little influence on the least refrangible red rays.[2] The effect also of the

colouring of lights on their visibility is of interest in determining the shades of colour to be used for signals and ship-lights, and also the relative power of the lights required for different colours to secure equal illuminating power. Distinctions of colour are essential in these cases; but for distinguishing lighthouses the use of coloured glasses has been abandoned, on account of their impairing the light emitted; and the desired indication has been effected by varying the number and duration of the flashes and eclipses in each lighthouse. The detection of colour-blindness is of interest to engineers, as this physical infirmity incapacitates men from acting as engine-drivers, signalmen, or navigating seamen. The use of compressed oil-gas enables buoys and beacons to give a warning or guiding light for about three months without requiring attention; and the electric light has accelerated the passage through the Suez Canal from 30½ hours to 20 hours, and has greatly increased the capacity of the Canal for traffic by enabling navigation to be carried on at night. The electric light also affords an excellent, safe, and cool light in the confined cabins on board ship, in the headings of long tunnels, and in the working-chambers filled with compressed air used for sinking subaqueous foundations.

Acoustics might seem to have little relation to engineering; but the soundness of the wheels of a train are tested by the noise they give when struck with a hammer; warning notes are emitted by railway and steamship whistles, the fog-horn on board ship, and the whistling and bell buoys employed for marking shoals or the navigable channel; whilst the striking of bells, the blast of steam sirens, and the explosion of compressed gun-cotton cartridges and rockets indicate the position of lighthouses in foggy weather. The most powerful sounds that can be produced by the help of steam appear to have a very limited range as compared with light; for, under ordinary conditions, the most powerful siren ceases to be audible at a distance of six or seven miles; whilst the transmission of sound is very much affected by the wind and the condition of the atmosphere. It seems possible that loud detonations at short intervals may be more readily heard than the continuous blast of a steam trumpet.

Electrical engineering is very intimately connected with physics, for it really is the application of electricity to industrial purposes. The very close relation between electricity and magnetism, discovered by Oersted in 1820, and further established by the remarkable researches of Faraday, has led to the present system of generating electricity by the relative movement of coiled conductors and electro-magnets, in dynamo-electric machines worked by a steam-engine or other motive power. The electrical current thus generated can be transmitted to a distance with little loss of energy; and it can either be used directly for lighting by arc or incandescent lamps, or be reconverted into mechanical power by the intervention of another dynamo. Electricity is also employed for the simultaneous firing of a series of mines, at a safe distance from the site of the explosion.

The convertibility of heat and energy, indicated by Mayer, forms the basis of thermodynamics; and the mechanical equivalent of heat, a physical problem of the highest interest, determined by Joule in 1843, furnishes a measure of the amount of work that can be possibly obtained by a given expenditure of heat in heat-engines.

The above summary indicates how the discoveries of physics are applied to many branches of engineering, and a knowledge of the laws of physics, and of the results of physical researches, appears, therefore, essential for the successful prosecution of engineering works. The very intimate relation of mechanical science to mathematics and physics, and the indebtedness of engineers to men of science outside the ranks of their profession, are, indeed, evidenced by the roll of the Presidents of Section G, containing the names of Dr. Robinson, Mr. Babbage, Professor Willis, Professor Walker, and Lord Rosse.

*Chemistry in Relation to Engineering.*—Gas-making is in reality a chemical operation on a large scale, consisting in the destructive distillation of coal, the purification and collection of the resulting carburetted hydrogen, and the separation and utilisation of the residual products. Chemistry, accordingly, holds a very important place in the requirements of the gas engineer.

The manufacture of iron, steel, and other metals, and the formation of alloys, are essentially chemical operations; and the Bessemer and Gilchrist processes, by which steel is produced in large quantities directly from cast iron, by eliminating a portion of the carbon contained in it, and also the injurious impurities, silicon and phosphorus, in place of the former costly and circuitous method of removing the carbon from cast iron to form wrought iron, and then combining a smaller proportion of carbon with the wrought iron to form steel, are based on definite chemical changes, and necessitated chemical knowledge for their development.

Chemical analysis is needed for determining the purity of a supply of water, or the nature and extent of its contamination; and Dr. Clarke's process for softening hard water, by the addition of lime water, depends upon a chemical reaction. The methods also of purifying water by filtration, shaking up with scrap iron, and aeration, are chemical operations on an extensive scale; and their efficiency has to be ascertained by chemical tests.

Cements and mortars depend for their strength and tenacity, when mixed with water, upon their chemical composition and the chemical changes which occur. The value of Portland cement requires to be tested quite as much by a chemical analysis of its component parts, as by the direct tensile strength of its briquettes; for an apparently strong cement may contain the elements of its own disruption, in a moderate proportion of magnesia or in an excess of lime. The chemical change which has been found to occur in the Portland cement of very porous concrete exposed to the percolation of sea-water under considerable pressure, by the substitution of the magnesia in sea-water for the lime in the cement, if proved to take place even slowly under ordinary circumstances, would render the duration of the numerous sea works constructed with Portland cement very precarious, and necessitate the abandonment of this very convenient material by the maritime engineer.

Explosives, which have rendered such important services to engineers in the construction of works through rock and the blasting of reefs under water, as well as for purposes of attack and defence, form an important branch of chemical research. The uses of gun-cotton as an explosive agent, though not for guns, have been greatly extended by the investigations of Sir Frederick Abel, and by the discovery that it can be detonated, when wet and unconfined, by fulminate of mercury; whilst smokeless powder, a more recent chemical discovery, seems likely, by its application to firearms, to produce important modifications in the conditions of warfare. The progress achieved by chemists in other forms of explosives has been marked by their successive introduction for blasting in large engineering works. Thus the removal of the rock in driving the Mont Cenis tunnel, in 1857–71, was effected by ordinary blasting powder; whilst the excavation of the longer St. Gothard tunnel, in 1872–82, was accomplished by the more efficient explosive dynamite.[1] Moreover, the first great blast for removing the portion of Hallett's Reef which obstructed the approach to New York Harbour, was effected mainly by dynamite, together with vulcan powder and rendrock, in 1876: whereas the far larger Flood Rock, in mid-channel, was shattered in 1885 by rackarock, a mixture of potassium chlorate and nitrobenzol, and a much cheaper and a more efficient explosive under water than dynamite.[2] Rackarock is one of the series of safety explosives first investigated by Dr. Sprengel in 1870, which, consisting of a solid and a liquid, is safely and easily mixed for use; and these materials, being harmless previously to their admixture, can be stored in large quantities without risk.[3] The cost also of this large blast was greatly reduced by the sympathetic explosion of the bulk of the cartridges by the detonation of a series of primary exploders, placed at intervals along the galleries and fired simultaneously by electricity from the shore.

The utilisation of sewage belongs to agricultural chemistry; and the deodorisation of sewage, and its conversion into a commercial manure, are chemical processes.

[1] *Proceedings Inst. C E*, vol 95, p. 266
[2] *Ibid.*, vol 85, pp 267, 270
[3] *Journal of the Chemical Society*, August 1873.

The disposal of sewage by irrigation is a branch of agriculture, and the innocuous character of the effluent fluid, discharged into the nearest stream or river, has to be ascertained by chemical analysis. Chemists have the opportunity of benefiting the community, and at the same time acquiring a fortune, by discovering an economical and efficient process for converting sewage on a large scale into a profitable saleable manure, so that inland towns may not have to dispose of their sewage at a loss, and that towns situated on tidal estuaries or the sea-coast may no longer discharge their sewage into the sea, but distribute it productively on the land.

The purifying of the atmosphere from smoke, rendered increasingly expedient by the growth of population, and the prevention of the dense fogs caused by it, by some practical method for more thoroughly consuming the solid particles of the fuel, still await the combined efforts of chemists and engineers.

*Geology in Relation to Engineering.*—A knowledge of the superficial strata of the earth is important for all underground works, and essential for the success of mining operations. Geology is indispensable in directing the search for coal, iron ore, and the various metals; and the existence of faults or other disturbances may greatly modify the conditions. The value of geology to the engineer is not, however, confined to the extraction of minerals, for it extends, more or less, to all works going below the surface.

The water-supply of a district, in the absence of a suitable river or stream, is dependent on the configuration and geology of the district; and the spread of London before the extension of waterworks, as pointed out by Professor Prestwich, had to be confined to the limits of the gravel subsoil, in which shallow wells gave access to the water arrested by the stratum of underlying London clay. The sinking also of deep wells for a supply of water, and the depth to which they should be carried, are determined by the nature of the formation, the position of faults, and the situation of the outcrop of the water-bearing stratum. A geological examination, moreover, of a site proposed for a reservoir, to be formed by a reservoir dam across a valley, has to be made to ascertain the absence of fissures and the soundness of the foundation for the dam.

In the driving of long tunnels, the nature and hardness of the strata and their dip, the prospects of slips, and the possibility of the influx of large volumes of water, are geological considerations which affect the designs and the estimates of cost. The excavations also of large railway cuttings and ship-canals are considerably affected, both as regards their side slopes and cost, by the nature and condition of the strata traversed.

*Meteorology in Relation to Engineering.*—The maximum pressure that may be exerted by the wind has to be allowed for in calculating the strains which roofs, bridges, and other structures are liable to have to bear in exposed situations, and continuous records of anenometers for long periods are required for determining this pressure. The force of the wind also, and the direction, duration, and period of occurrence of severe gales, are important to the maritime engineer for estimating the effects of the waves in any special locality, for determining the quarter from which shelter is needed, and for ascertaining the seasons most suitable for the execution of harbour works, the repair of damages, and the carrying out of foundations of lighthouses and beacons on exposed rocks. The harbour engineer must, indeed, of necessity be somewhat of a meteorologist, for the changes in the wind and weather, the oscillations of the barometer, and the signs of an approaching storm are indications to him of approaching danger to his works, which he has to guard against; for the sea is an insidious enemy which soon discovers any weak spot, and may in a few hours destroy the work of months.

Continuous records of rainfall, as collected regularly by Mr Symons from numerous stations in the United Kingdom, are extremely valuable to engineers for calculating the probable average yield of water from a given catchment area, the greatest and least discharges of a river or stream, the size of drainage channel needed to secure a low-lying area from floods, and the amount of water available for storage or irrigation in a hot, arid district. The loss of water by evaporation at different periods of the year, and under different conditions of soil and climate, the effect of

percolation in reducing evaporation, and the influence of forests and vegetation in increasing the available rainfall, while equalising the flow of streams, are subjects of equal interest to hydraulic engineers and meteorologists.

Countries periodically visited by hurricanes, cyclones, or earthquakes, necessitate special precautions, and special designs for structures, and every additional information as to the force and extent of these visitations of nature is of value in enabling engineers to provide more effectually against their ravages

*Benefits conferred by Engineering upon Pure Science* —Engineering is generally concerned in the application of the researches of science for the benefit of mankind, and not in the extension of the domain of pure science, which necessitates greater concentration of attention and study than the engineer in practice is able to devote to it.   Engineers, however, though never able to repay the ever-increasing debt of gratitude which they owe to past and present investigators of science, except in rendering these abstract researches of practical utility, have, nevertheless, been able incidentally to promote the progress of science.   Thus mechanical science, by the construction of calculating machines, the planimeter, integrating machines, the tide-predictor and tidal harmonic analyser of Lord Kelvin, the self-registering tide-gauge, and various other instruments, has lightened the labours of mathematicians : whilst excavations for works, and borings have assisted the investigations of geologists.   The mechanical genius of Lord Rosse led mainly to the success of his gigantic telescope, which has revealed so many secrets of the heavens : and the rapidity of locomotion, due to the labours of engineers, has greatly facilitated astronomical observations and physical discoveries, besides promoting the concourse of scientific men and the diffusion of knowledge   Electrical engineering, moreover, is so closely allied to electrical physics that the development of the one necessarily promotes the progress of the other.   The observations also conducted by hydraulic and maritime engineers in the course of their practice aid in extending the statistics upon which the science of meteorology is based.

*Engineering as an Experimental Science.*—Engineering, so far as it is based on mathematics, is an exact science, and the strains due to given loads on a structure can be accurately determined ; but the strength of the materials employed has to be ascertained before any structure can be properly designed.   Accordingly, the resistance of materials to tension, compression, and flexure, has to be tested, and their limit of elasticity and breaking weight determined.   Thus, previously to the construction, by Robert Stephenson, of the Britannia Tubular Bridge, the first wrought-iron girder bridge of large span erected, numerous experiments on various forms of wrought iron were carried out by that eminent mathematician and mechanician Eaton Hodgkinson, who had previously indicated the proper theoretical form for cast-iron girders, and to whom the success of the bridge across the Menai Straits was in great measure due.[1]   Besides the numerous tests always now made of the materials employed during the progress of any large engineering work, railway bridges are also subjected to severe test loads before being opened for public traffic, by which the safety of the structures and their rigidity, as measured by the amount of deflection, are ascertained, serving as a guide for subsequent designs.

Numberless experiments have been made on the flow of water in open channels, over weirs, through orifices, and along pipes ; and the influences of the nature of the bed, the slope, depth, and size of channel, have been investigated by various hydraulicians.   Mr. Thomas Stevenson measured the force of waves at some places on the Scotch coast ;[2] Professor Osborne Reynolds has examined the laws of tidal flow in a model of the inner estuary of the Mersey, and in specially shaped experimental models ;[3] and I have found it possible, in small working models of the Mersey and Seine, not merely to reproduce the configuration of the bed of the estuary out to sea, but also to observe the effects of different forms of training works in modifying sandy estuaries.[4]   Mr. William Froude, after his retirement

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[1] *The Britannia and Conway Tubular Bridges*, Edwin Clark, vol. 1, p. 83.
[2] *The Design and Construction of Harbours*, Thomas Stevenson, 3rd ed pp. 52-56.
[3] *British Association Reports* for 1889, 1890, and 1891.
[4] *Proceedings of the Royal Society*, vol. 45, pp 504-524, and plates 2-4 ; vol. 47

from active practice, devoted his abilities to experiments on the motion and resistance of ships in water, which have proved of inestimable value to the naval architect, and which formed the subject of his presidential address to this Section in 1875.

Electrical engineering is specially adapted for experimental investigation ; and, in this branch, theory and practice are so closely allied that some of the most eminent exponents of the theory of the subject, such as Lord Kelvin and Dr. Hopkinson, have developed their theories into practical results. In most other branches, the investigator is generally distinct from the engineer in large practice ; but it may be safely said that an able investigator and generaliser in engineering science, as, for instance, the late Professor Rankine, accomplishes work of more value to the profession at large than the practical engineer, who, in the world's estimation, appears the more successful man.

Every branch of engineering science is more or less capable of being advanced by experimental investigations; and when it is borne in mind that the force of waves, the ebb and flow of tides in rivers, the influences of training works in estuaries, and the motion of ships at sea have been subjected to experimental research, it appears impossible to assign a limit to the range of experiments as a means of extending engineering knowledge. Problems of considerable interest, which can only be solved by experiments or by comprehensive generalisations from a number of examples, must frequently present themselves to engineers in the course of their practice, as they have to myself; and engineers would render a great service to the profession if they would follow up the lines of investigation thus suggested to them, in the true spirit of scientific inquiry.

*Failures of Works due to Neglect of Scientific Considerations.*—Before the amount and distribution of the stresses in structures were thoroughly understood, a disposition was naturally evinced to err on the side of excessive strength ; and the materials in the various parts of the structure were not suitably proportioned to the load to be borne, resulting in a waste of materials and too great an expenditure on the works. Thus some of the early high masonry reservoir dams in Spain exhibit an excessive thickness towards the top, imposing an unnecessary load on the foundations ; and in many of the earlier iron girder bridges more material was employed than was required for stability, and it was not properly distributed. Boldness engendered by increased experience, and dictated by motives of economy, has tended to make the engineers of the present day pursue an opposite course; and, under these circumstances, the correct calculation of the strains, the exact strength of the materials, and a strict appreciation of the physical laws affecting the designs become of the utmost importance.

The failures of many bridges may be explained by errors in design, defects in construction, or by economy carried beyond the limits of safety in pushing forward railways in undeveloped countries , but other failures are attributable to a disregard or underestimation of the influence of physical causes. Thus the Tay Bridge disaster, in 1879, was due to underestimating the amount and effect of the wind-pressure in an exposed situation, where it acted with a considerable leverage, owing to the height of the bridge, and was inadequately provided against by the small transverse width of the piers in proportion to their height, which were further weakened by bad workmanship in the bracing of their columns. The bursting of the Bouzey masonry dam in France this year must be attributed to an inadequate thickness at part of the cross-section, producing a tensional strain on the inner face with the reservoir full, aided by the instability resulting from a fissured foundation. The overthrow of the outer arms of the Madras breakwaters, during a cyclone in 1881, may be traced to an inadequate estimate of the force of the waves in a storm, in deep water, and with a great fetch across the Indian Ocean, beating against the portions of the breakwaters directly facing their course ; for these outer portions, running nearly parallel to the coast-line, were not made any stronger than the inner portions

p 142 ; and *Amélioration de la Partie Maritime des Fleuves, y compris leurs Embouchures*, L F Vernon-Harcourt, Paris Inland Navigation Congress, 1892, pp. 27-29, and 32, 33, and plate 3

placed at right angles to the shore and the direction of the waves, and situated for the most part in shallower water. The erosion of the bed of the Ganges Canal on the first admission of the water, necessitating the erection of weirs at intervals to check the current, resulted from an error in the calculated discharge of the channel with the given inclination, and the consequent undue velocity of the stream, producing scour. The failure of the jetty works at the outlet of the Rhone to effect any permanent deepening of the channel over the bar, was due to the unsuitable direction given to the outlet channel in view of the physical conditions of the site, and the concentration of all the discharge, and consequently all the alluvium carried down, into a single mouth, whereby the rate of deposit in front of this outlet has been considerably increased. The excessive cost, and consequent stoppage, of the Panama Canal works, though due to a variety of causes, must be partly attributed to want of due consideration of the strata to be excavated; for a cutting of 300 feet in depth, which may be possible in rock, becomes impracticable when a considerable portion has to be executed in very treacherous clay.

Occasionally failures of works may be attributed to exceptional causes or peculiarly unfavourable conditions; but in most cases, as in the instances given above, they are the result of errors or deficiencies in design, which might have been avoided by a more correct appreciation of the physical conditions involved.

*Scientific Training of Engineers.*—In most professions, preliminary training in those branches of knowledge calculated to fit a student for the exercise of his profession is considered indispensably necessary; and examinations to test the proficiency of candidates have to be passed as a necessary qualification for admission into the Army, Navy, Church, Civil Service, and both branches of the law. Special care is taken in securing an adequate preliminary training in the case of persons to whom the health of individuals is to be entrusted, not merely by experience in hospitals, but also by examinations in those branches of science and practice relating to medicine and surgery, before the medical student can become a qualified practitioner. If so much caution is exercised in protecting individuals from being attended by doctors possessing insufficient knowledge of the rudiments of their profession, how much more necessary should it be to ensure that engineers are similarly qualified, to whom the safety and well-being of the community, as well as large responsibilities in regard to expenditure, are liable to be entrusted! The duty of the engineer is to apply the resources of nature and science to the material benefit and progress of mankind; and it, therefore, seems irrational that no guarantee should be provided that persons, before becoming engineers, should acquire some knowledge of natural laws, and of the principles of those sciences which form the basis of engineering. The Institution of Civil Engineers has, indeed, of recent years required some evidence of young men having received a good education before their admission into the student class; but some of the examinations accepted as sufficient for studentship, such as a degree in any British university, afford no certainty in themselves that the persons who have passed them possess any of the qualifications requisite for an engineer, and it is quite unnecessary to become a student of the Institution in order to become an engineer. The Council of the Institution has no doubt been hitherto deterred from proposing the establishment of an examination in mathematics and natural science, as a necessary preliminary to becoming an engineer, by the remembrance that some of the most distinguished engineers of early days in this country were self-taught men; but since those days engineering and the sciences upon which it is based have made marvellous advances; and in view of these developments, and the excellent theoretical training given to foreign engineers, it is essential that British engineers, if they desire to retain their present position in the world, should arrange that the recruits to their profession may be amply qualified at their entrance in theoretical knowledge, in order to preserve the standard attained, and to be in a position to achieve further progress. No amount of preliminary training will, indeed, necessarily secure the success of an engineer, any more than the greatest proficiency would be certain to lead the medical student to renown as a physician or surgeon; but other conditions being equal, it will greatly promote his prospects of advancement in his profession, and his utility to his colleagues and the public. The engineers of the past achieved great results in the

then early dawn of engineering knowledge, by sound common sense, a ready grasp of first principles and of the essential points of a question, capacity for acquiring knowledge, power of managing men and impressing them with confidence, and shrewdness in selecting competent assistants.    These same qualities are still needed for success in the present day, coupled with an opportunity of exhibiting them ; but far more knowledge of mathematics and other sciences is required now, owing to the enormous advances effected, if the progress of engineering science is to be maintained.    Even though in some branches engineers in large practice may not have the time, or retain the requisite facility, for solving intricate mathematical problems, they should be able readily to comprehend the full bearing of the principles presented, and to understand the nature of the solutions put before them, which nothing but the scientific faculty implanted by early training in mathematics and physics can adequately secure.

A qualifying examination for engineers would usefully stop persons at the outset from entering the profession, who failed to evince the possession of the requisite preliminary knowledge : it would indicate, by the subjects selected, the kind of training best calculated to fit a person to become a useful engineer ; and it would protect the public, as far as practicable, from the injuries or waste of money that might result from the mistakes of ill-qualified engineers.

*Specialising in Engineering.*—Some branches of engineering have for a long time been kept distinct from others, such as the construction of steam-engines, locomotives, and marine engines, ship-building, heavy ordnance, hydraulic machinery, and other purely mechanical works, one or more of which have been treated as specialities by certain firms, and also gas lighting, and, more recently, electric lighting.    In the department, however, of civil engineering in its narrower signification, as distinguished from mechanical engineering, engineers of former times were regarded as equally qualified to undertake any of the branches of public works ; and the same engineer might be entrusted with the execution of roads, railways, canals, harbours, docks, sewerage works, and waterworks ; while even steamships were not excluded from the category in Brunel's practice.    The engineer of to-day, indeed, would be lacking that important factor for success, common sense, if he declined to execute any class of works which he might be asked to undertake ; and a variety of works is very useful to the engineer in enlarging his views and experience, as well as in extending the range of his practice.    The tendency, however, now in engineering, as in medicine, is for the engineer's practice to be confined to the special branch in which he had had most experience ; a result which cannot fail to be beneficial to the public, and calculated to promote the progress of each branch.    The powers of the human mind are too limited, and life is too short, for engineers to be able to acquire, in the present day, equal proficiency in the theory and practice of the several branches of engineering science, with their ever-widening scope and development ; and, as in the domain of abstract science, general progress will be best achieved in engineering science by the concentration of the energies of engineers in the advancement of their special line of practice.

*Value of Congresses on Special Branches of Engineering.*—The scope of engineering science is extending so fast that it is impossible for the Institution of Civil Engineers, which, as the parent society, embraces every branch within its range of subjects, to give more than a very limited time for the consideration and discussion of papers relating to the non-mechanical branches of the profession comprised in public works. Mechanical, electrical, and gas engineers have special societies of their own for advancing their knowledge and publishing their views and experience, while sharing equally with the other branches in the benefits of the older Institution.    Congresses accordingly afford a valuable opportunity for railway, hydraulic, and sanitary engineers of expressing their views, and enlarging their experience by consultation and discussion with engineers of various countries.    My experience of the six maritime, inland navigation, and waterworks international congresses I have attended in England and abroad, has convinced me of the very great value of such meetings in collecting information, comparing views, and obtaining some knowledge of foreign works and methods ; whilst the acquaintances formed with some of the most celebrated foreign engineers, afford opportunities of gaining further infor-

mation about works abroad, and deriving experience from their progress and results.

*Engineering Literature.*—Lawyers have been defined as persons who do not possess a knowledge of law, but who know where to find the law which they may require. It may be hoped that a similar definition is not applicable to engineers; but with the rapid increase of engineering literature, it is most desirable that engineers should be able readily to refer to the information on any special subject, or descriptions of any executed works, which may have been published. Much valuable matter, however, is buried in the proceedings of engineering and scientific societies, and in various publications: and often a considerable amount of time is expended in fruitless search. This great waste of time and energy, and the loss of available information involved, led me a few years ago to suggest that a catalogue of engineering literature ought to be made, arranging the lists of publications relating to the several branches under separate headings. There is a possibility that this arduous and costly task may be partially accomplished in separate volumes; and, at any rate, the first step has been effected by the publication, under the auspices of the Paris Inland Navigation Congress of 1892, of a catalogue of the publications on inland navigation. A start has also been made in France, Italy, and England, towards the preparation of a similar catalogue on maritime works, which it may be hoped means will one day be found to publish on the meeting of some future congress Engineers who have searched, even in the best libraries, for the published information on any special subject, will appreciate what a great boon an engineering subject catalogue would be to the profession, and indirectly to the public at large.

The occasional publication of comprehensive books on special branches of engineering, and concise papers on special subjects, by competent authorities, are extremely valuable in advancing and systematising engineering knowledge; but the time and trouble involved in the preparation of such publications must, like the organising of congresses, be regarded as a duty performed in the interests of the profession and science, and not as affording a prospect of any pecuniary benefit.

*Concluding Remarks.*—In this address, I have endeavoured, though very imperfectly, to indicate how engineering consists in the application of natural laws and the researches of science for the benefit and advancement of mankind, and to point out that increased knowledge will be constantly needed to keep pace with, and to carry on, the progress that has been made. The great advantages provided by engineering works in facilitating communications and intercourse, and consequently the diffusion of knowledge, in increasing trade, in extending civilisation to remote regions, in multiplying the comforts of life, and affording enlarged possibilities of enjoyment and change of scene, may be regarded as amply acknowledged; but the more gradual and less obvious, though not less important, benefits effected by engineering works are not so fully realised.

A comparison of engineering with the other chief branch of applied science, medicine, exhibits some similarities and differences. In both professions, the discoveries of science are utilised on behalf of mankind; but whilst physicians devote themselves mainly to individuals, engineers are concerned in promoting the well-being of the community at large. Persons reluctantly consult doctors when they are attacked by disease, or incapacitated by an accident; but they eagerly resort for enjoyment to railways, steamships, mountain tramways, piers, great wheels, and Eiffel towers; and they frequently avail themselves of the means of cheap and easy locomotion to complete their restoration to health by change of air and climate. Physicians try to cure people when they are ill: whereas engineers endeavour, by good water-supply and efficient drainage, to maintain them in health, and in this respect, the evident results of medical skill are far more readily realised than the invisible, though more widespread, preventive benefits of engineering works Statistics alone can reveal the silent operations of sanitary works; and probably no better evidence could be given of the inestimable value of good water and proper drainage on the health of the population of large towns, when aided by the progress of medical science, than the case of London, where, towards the close of the last

century, the death-rate exceeded the birth-rate, and the numbers were only kept up by constant immigrations; whereas now, in spite of the vast increase of the population and the progressive absorption of the adjacent country into the ever-widening circle of houses, the number of births exceed the deaths by nearly nine hundred a week.

In engineering, as in pure science, it is impossible to stand still; and engineers require to be ever learning, ever seeking, to appreciate more fully the laws of nature and the revelations of science, ever endeavouring to perfect their methods by the light of fresh discoveries, and ever striving to make past experience and a wider knowledge stepping-stones to greater achievements. Engineers have a noble vocation, and should aim at attaining a lofty ideal; and, in the spirit of the cele-brated scientific discoverers of the past, such as Galileo, Newton, La Place, Cavendish, Lyell, and Faraday, should regard their profession, not so much as an opportunity of gaining a pecuniary reward, as a means of advancing knowledge, health, and prosperity.

The remarkable triumphs of engineering have been due to the patient and long-continued researches of successive generations of mathematicians, physicists, and other scientific investigators; and it is by the utilisation of these stores of know-ledge and experience that engineers have acquired renown. A higher tribute of gratitude should perhaps be paid to the noble band of scientific investigators who, in pursuit of knowledge for its own sake, have rendered possible the achievements of engineering, than to those who have made use of their discoveries for the attainment of practical benefits; but they must both be regarded as co-workers in the promotion of the welfare of mankind. The advancement of science develops the intellectual faculties of nations, and enlarges their range; whilst the resulting progress in engineering increases their material comforts and prosperity. If men of science, by closer intercourse with engineers, could realise more fully the practical capabilities of their researches, and engineers, by a more complete scientific training, could gain a clearer insight into the scientific aspect of their profession, both might be able to co-operate more thoroughly in developing the resources of nature, and in furthering the intellectual and material progress of the human race.

# British Association for the Advancement of Science.

## IPSWICH, 1895

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# ADDRESS

### TO THE

# ANTHROPOLOGICAL SECTION.

#### BY

### Professor W. M. FLINDERS PETRIE, D.C.L., LL.D.,

PRESIDENT OF THE SECTION.

In a subject as yet so unmapped as anthropology there is more room for considering different points of view than in a thoroughly organised and limited science. The future structure of this science depends largely on the apprehension of the many different modes of treating it. The time has not yet come when it can be handled as a whole, and therefore at present we may frankly consider various questions from an individual standpoint, without in the least implying that other considerations should not be taken into account. It is only by the free statement, however onesided, of the various separate views of the many subjects involved in such a science, that any comprehensive scheme of its organisation can ever be built up. In remarking, therefore, on some branches at present I shall not attempt a judicial impersonality, but rather try to express some views which have not yet been brought into ordinary currency.

Elaborate definitions of anthropology have been formulated, but such are only too liable to require constant revision as fresh fields of research are added to the domain. In any new country it is far safer to define its limits than to describe all that it includes; and all that can yet be safely done in anthropology is to lay down the 'sphere of influence,' and having secured the boundaries, then develop the resources at leisure. The principal bordering subjects are zoology, metaphysics, economics, literature, and history. So far as these refer to other species, as well as to man, or to individuals rather than to the whole race, they stand apart as subjects; but their relation to the human species as such is essentially a part of anthropology. We must be prepared, therefore, to take anthropology more as the study of man in relation to various and often independent subjects, rather than as an organic and self-contained science. Human nature is greater than all formulæ; and we may as soon hope to compact its study into a logical structure, as to construct an algebraical equation for predicting its course of thought.

Two of the commonest and most delightfully elastic words in the subject may be looked at once more—'race' and 'civilisation.' The definition of the nature of race is the most requisite element for any clear ideas about man. Our present conception of the word has been modified recently more than may be supposed by our realising the antiquity of the species. When only a few thousand years had to be dealt with nothing seemed easier or more satisfactory than to map out races on the assumption that so many million people were descended from one ancestor and so many from another. Mixed races were glibly separated from pure races,

and all humanity was partitioned off into well-defined divisions. But when the long ages of man's history and the incessant mixtures that have taken place during the brief end of it that is recorded come to be realised, the meaning of 'race' must be wholly revised. And this revision has not yet taken effect on the modes of thought, though it may have demanded the assent of the judgment  The only meaning that a 'race' can have is a group of persons whose type has become unified by their rate of assimilation and affection by their conditions exceeding the rate of change produced by foreign elements. If the rate of mixture exceeds that of assimilation, then the people are a mixed race, or a mere agglomeration, like the population of the United States  The greatest problems awaiting solution are the conditions and rate of assimilation of races—namely, what period and kind of life is needed for climatic and other causes to have effect on the constitution and structure, what are the causes of permanence of type, and what relative powers of absorption one race has over another. Until these problems are reduced to something that can be reasonably estimated we shall only grope in the dark as to all racial questions.

How, then, can these essential problems be attacked ?  Not by any study of the lower races, but rather by means of those whose history is best recorded  The great mode of isolation on which we can work is religious difference, and oppressed religious minorities are the finest anthropological material. The first question is—given a mixture of various races in approximately known proportions, isolated, and kept under uniform conditions, how soon does uniformity of type prevail ? or what proportions of diversity will be found after a given number of generations ? A perfect case of this awaits study in the Copts, who have by monogamy and the fanaticism of a hostile majority been rigorously isolated during 1,200 years from any appreciable admixture, and who before this settling time were compounded of eight or ten different races, whose nature and extent of combination can be tolerably appraised. A thorough study of the present people and their forefathers, whose tombs of every age provide abundant material for examination, promises to clear up one of the greatest questions—the effect of climate and conditions on assimilating mixed peoples. The other great problem is, How far can a type resist changes of conditions, provided it be not mixed in blood, so as to disturb its equilibrium of constitution ? This is to be answered by the Jews and the Parsis. As with the Copts, an oppressed religious minority has no chance of mixture, as all mixed marriages are abhorrent to its exclusiveness, and are at once swept into the hostile majority. The study is, however, far more difficult owing to the absence of such good conditions of the preservation of material. But nothing could throw so much light on this as an excavation of some Jewish cemeteries of a thousand years or so ago in various European countries, and comparison of the skeletons with the proportions of the Jews now living. The countries least affected by the various proscriptions and emigrations of the race would be the proper ground for inquiry. When these studies have been made we shall begin to understand what the constants of a race really are.

We will now look at another word which is incessantly used—'civilisation.' Many definitions of this have been made, from that of the Turk drinking champagne, who remarked about it that 'after all, civilisation is very nice,' up to the most elaborate combinations of art and science  It is no doubt very comfortable to have a word which only implies a tendency, and to which everyone can assign his own value; but the day of reckoning comes, when it is brought into arguments as a term. Civilisation really means simply the art of living in a community, or the checks and counterchecks, the division of labour, and the conveniences that arise from common action when a group of men live in close relation to each other. This will perhaps be objected to as including all—or nearly all—mankind in its scope. Quite true; all civilisation is relative and not absolute.

We shall avoid much confusion if we distinguish high and low types of civilisation, and also perfect and imperfect civilisation. Like organisms we may have a low type of civilisation very perfect in its structure, capable of endless continuance, and of great shocks without much injury. Such are some of the civilisations

of the African races who have great orderliness and cleanliness of arrangements, and are capable of active recuperation after warfare, without any internal elements of instability. Again, some low types are very imperfect, and can exist only by destruction of others, while any severe shock destroys their polity; the governments which only exist by raids and plunder, such as that of the Zulus, illustrate this. Turning to high types of civilisation we may see them perfect or imperfect. Countries of financial stability, not undergoing any rapid organic changes, are the more perfect in type; while those deeply in debt and in continual revolution have but imperfect civilisation, of however high a type it may be. With these distinctions before us,—that all civilisation is a question of degree,—that there are types of all variety, from the highest complexity to the lowest simplicity, and of all degrees of perfection, or stability and completion, in any given level of complexity—with these distinctions some of the vagueness of verbal usage may perhaps be avoided.

Turning now from words to things, we may perhaps see some ground for further consideration in even one of the best elaborated departments.

In the much-vexed question of skull measurements, the paucity of clearly defined racial characteristics may make us look more closely as to whether we are working on an analytic or an empirical method. In any physical problem the first consideration is the disentangling of variables, and isolation of each factor for separate study. In skulls, however, the main measures are the length, which is compounded of half a dozen elements of growth, and the breadth and height, each the resultant of at least three elements. Two skulls may differ altogether in their proportions and forms, and yet yield identical measures in length, breadth, and height. How can any but empirical results be evolved from such a system of measurement alone?

A departure from this mechanical method has appeared in Italy last year by Professor Sergi. He proposes to classify skulls by their forms,—ellipsoid, pentagonoid, rhomboid, ovoid, &c. This, at least, takes account of the obvious differences which the numerous measurements wholly ignore. And if skulls were crystals, divisible into homogeneous classes, such a system would work; only, like all organic objects, they vary by infinite gradation.

What then lies behind this variety of form? The variety of action in the separate elements of growth. Sergi's ellipsoid type means slight curvatures, with plenty of frontal growth. His pentagonoid means sharper curvatures. His rhomboid means sharp curvatures with small frontal growth. And so in each class, we have not to deal with a geometrical figure, but with varying curvatures of the centre of each plate of the skull, and varying extent of growth from the centres.

The organic definition of a skull must depend on the statement of the energy and direction of each of the separate elements out of which it is built. The protuberances or eminences are the first point to notice. They record in their curves the size of the head when it attained rigidity in the centres of growth. Every person bears the fixed outline of parts of his infant skull. Little, if any, modification is made in the sharpness of the curves between infancy and full growth; perhaps the only change is made in course of the thickening of the skull. Hence the minimum radius of curvature of each plate of the skull is a most radical measurement, as implying early or late final ossification. In higher races finely rounded skulls with slight curvatures are more often found, and this agrees with the deferred fixation of the skull pointed out by the greater frequency of visible sutures remaining, both effects being probably due to the need of accommodating a more continued growth of the brain. The length of growth of each plate from its centre in different directions regulates the entire form of the skull. The maximum breadth being far back implies that the parietals grow mostly toward the frontal or *vice versâ*. The top being ridged means that the parietals grow conical and not spherically curved, and hence meet at an angle.

It seems, therefore, that looking at the question as a physical problem, we are far more likely to detect racial peculiarities in the separate data of the period of fixation of the skull, and of the amount of growth in different directions, than

by any treatment of gross quantities which are compounded out of a number of variables. The practical development of such a view is the work of the embryologist: here we only notice a principle of treatment of a most complex question, which seems to have too often been dealt with as if it were as simple as the definition of a crystal.

When we next turn to look at the works of man, it seems that the artistic side of anthropology has hardly been enough appreciated. In the first place, the theory of art has been grounded more assuredly by anthropological research than by all the speculations that have been spun. The ever-recurring question ' What is art ? ' whether in form or in literature, has been answered clearly and decidedly. When we contrast a row of uninteresting individualities with the ideal beauty and expression of a composite portrait compounded from these very elements, we are on the surest ground for knowing how such a beautiful result is obtained. In place of the photographic verity of the person we have the artistic expression of a character. Whatever is essential remains, and is strengthened ; whatever is transient and unimportant has faded away. No one can look, for instance, at the composite heads of Jewish boys and their individual components, published some years ago in the 'Anthropological Journal,' without feeling the artistic beauty of the composite and the unbalanced characters of the individuals. What the camera does mechanically by mere superposition, the artist does intelligently by selection. The unimportant, unmeaning phases of the person, the vacuities of expression, the less worthy turns of the mind are eliminated, whether in form or in words, and the essence of the character is brought out and expressed. Such is the theory of artistic expression which anthropology has established on a sure basis of experiment, and which is thus proved to be neither fanciful nor arbitrary, but to be a truly scientific process.

And as anthropology has thus aided art, the converse is also true—art is one of the most important records of a race. Each group of mankind has its own style and favourite manner, more particularly in the decorative arts. A stray fragment of carving without date or locality can be surely fixed in its place if there is any sufficient knowledge of the art from which it springs. This study of the art of a people is one of the highest branches of anthropology and one of the most important, owing to its persistent connection with each race. No physical characteristics have been more persistent than the style of decoration. When we see on the Celtic work of the period of La Téne, or on Irish carvings, the same forms as on mediæval ironwork, and on the flamboyant architecture of France, we realise how innate is the love of style, and how similar expressions will blossom out again from the same people. Even later we see the hideous C-curves, which are neither foliage nor geometry, to be identical on late Celtic bronze, on Louis XV. carvings, and even descending by imitation into modern furniture. Such long descent of one style through great changes of history is not only characteristic of Celtic art, but is seen equally in Italy. The heavy, stiff, straight-haired, staring faces of the Constantine age are generally looked on as being a mere degradation of the imported Greek art ; but they are really a native revival, returning to old Italian ideals, so soon as Greek influence waned. In the Vatican is an infant Hercules of thorough Constantinian type, yet bearing an Etruscan inscription, proving the early date of such work. Further east the long-persistent styles of Egypt, of Babylonia, of India, of China, which outlived all changes of government and history, show the same vitality of art. We must recognise, therefore, a principle of ' racial taste,' which belongs to each people as much as their language, which may be borrowed like language from one race by another, but which survives changes and long eclipses even more than language. Such a means of research deserves more systematic study than it has yet received.

But if we are to make any wide comparisons and generalisations a free study of material is essential, and the means of amassing and comparing work of every age is the first requisite. This first requisite is unhappily not to be found in England. The conception of collecting material for the study of man's history has as yet little root, and struggles to find a footing between the rival conceptions of the history of art and the life of modern man. The primary difficulty is the

character of the museum accommodation at present provided. This is all of an elaborate and expensive nature, in palatial buildings and on highly valuable sites. To house the great mass of objects of either ancient or modern peoples in such a costly manner is impracticable, and hence at present nothing is preserved but what is beautiful, strange, or rare. In short, our only subjects of study are the exceptional and not the usual products of races. The evil traditions of a ' collection of curiosities' still brood over our materials; and until we face the fact that for study the common things are generally more important than the rare ones, anthropology must remain much as chemistry would if it were restricted to the study of pretty colours and sweet scents.

Until we have an anthropological storehouse on a great scale we cannot hope to preserve the materials which are now continually being lost to study for lack of reasonable accommodation. Such a storehouse should be on the cheapest ground near London, built in the simplest weather-tight fashion, and capable of indefinite expansion, without rearrangement or alteration of existing parts It should contain no baits for burglars, all valuable objects being locked up in the security of the British Museum, to which such a storehouse would form a succursal, greatly relieving the present overcrowded state of many departments. To such a storehouse for students all that does not serve for public education, or that is not portable or of much saleable value, should be consigned. There the piles of architectural fragments which are essential for study, but are useless to show the public, should be all stacked in classified order. There the heaps of pottery of ancient and modern races should all be arranged to illustrate every variety of form and style. There the series of entire tombs of other races and of our own should be set out in their original arrangement, as in the Bologna Museum. There whole huts, boats, &c., could be placed in their proper order and sequence, while photographs of the showy educational specimens and valuables in the public museums could fill their places in the arrangement. That such a storehouse is needed may be illustrated by a collection gleaned in a few months' work this year. It represents the small products of a little village and a cemetery of a new race in Egypt. But there is no possibility of keeping such a collection together in any London museum; and but for the new Ashmolean Museum at Oxford having been lately built with a wide view to its increase, it is doubtful if in any place in England such a collection could be kept together. What happens to one excavator this year may happen to a dozen excavators *per annum* in a generation or two hence; and so long as space is not available to preserve such collections when they are obtained, invaluable material is being irrevocably wasted and destroyed.

Besides the theoretical and scientific side of anthropology there is also a very practical side to it which has not received any sufficient development as yet. Anthropology should in our nation be studied first and foremost as the art of dealing with other races. I cannot do better than quote a remark from the address of our previous President, General Pitt Rivers, a remark which has been waiting twenty-three years for further notice. He said, ' Nor is it unimportant to remember that anthropology has its practical and humanitarian aspect; and that as our race is more often brought into contact with savages than any other, a knowledge of their habits and modes of thought may be of the utmost value to us in utilising their labour, as well as in checking those inhuman practices from which they have but too often suffered at our hands.'

The foremost principle which should be always in view is that the civilisation of any race is not a system which can be changed at will. Every civilisation is the growing product of a very complex set of conditions, depending on race and character, on climate, on trade, and every minutia of the circumstances. To attempt to alter such a system apart from its conditions is impossible. For instance, whenever a total change is made in government, it breaks down altogether, and a resort to the despotism of one man is the result. When the English Constitution was swept away, Cromwell or anarchy was the alternative: when the French Constitution was swept away Napoleon was the only salvation from anarchy. And if this is the case when the externals of government alone are altered, how much more is it the case if we attempt to uproot the whole of a civilisation and social

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life ?   We may despotically force a bald and senseless imitation of our ways on
another people, but we shall only destroy their life without implanting any vitality
in its place.   No change is legitimate or beneficial to the real character of a people
except what flows from conviction and the natural growth of the mind.   And if
the imposition of a foreign system is injurious, how miserable is the forcing of a
system such as ours, which is the most complex, unnatural, and artificial that has
been known ; a system developed in a cold country, amid one of the hardest, least
sympathetic, and most self-denying and calculating of all peoples of the world.
Such a system, the product of such extreme conditions, we attempt to force on the
least developed races, and expect from them an implicit subservience to our illogical
law and our inconsistent morality.   The result is death ; we make a dead-house
and call it civilisation.   Scarcely a single race can bear the contact and the burden.
And then we talk complacently about the mysterious decay of savages before white
men.

Yet some people believe that a handful of men who have been mutilated into
conformity with civilised ideals are better worth having than a race of sturdy inde-
pendent beings.   Let us hear what becomes of the unhappy products of our notions.
On the Andaman Islands an orphanage, or training school, was started and more
than forty children were reclaimed from savagery, or torn from a healthy and
vigorous life.   These were the results.   ‘ Of all the girls two only have continued
in the Settlement, the other survivors having long since resumed the customs of
their jungle homes. . . . Physically speaking, training has a deteriorating effect,
for of all the children who have passed through the orphanage, probably not more
than ten are alive at the present time, while of those that have been married, two
or three only have become parents, and of their children not one has been reared.’ [1]
Such is the result of our attempts on a race of low but perfect civilisation, whom
we eradicate in trying to improve them.

Let us turn now to our attempts on a higher race, the degenerated and Arabised
descendants of a great people, the Egyptians.   Here there is much ability to work
on, and also a good standard of comfort and morality, conformable to our notions.
Yet the planting of another civilisation is scarcely to be borne by them.   The
Europeanised Egyptian is in most cases the mere blotting paper of civilisation,
absorbing what is most superficial and undesirable.   The overlaying of a French
or English layer on a native mind produces only a hybrid intellect, from which
no natural growth or fertility can be expected.   Far the more promising intellects
are those trained by intelligent native teachers, where as much as can be safely
assimilated has grown naturally as a development of the native mind.

Yet some will say why not plant all we can ? why not have the harm of raising
the intellect in some cases if we cannot do it in all ?   The harm is that you manu-
facture idiots.   Some of the peasantry are taught to read and write, and the result
of this burden which their fathers bore not is that they become fools.   I cannot
say this too plainly : an Egyptian who has had reading and writing thrust on him
is, in every case that I have met with, half-witted, silly, or incapable of taking care
of himself.   His intellect and his health have been undermined and crippled by the
forcing of education.   With the Copt this is quite different : his fathers have been
scribes for thousands of years, and his capacity is far greater, so that he can receive
much more without deterioration   Observation of these people leads to the view that
the average man cannot receive much more knowledge than his immediate ancestors.
Perhaps a quarter or a tenth more of ideas can be safely put into each generation
without deterioration of mind or body ; but, at the best, growth of the mind can in
the average man be but by fractional increments in each generation, and any large
increase will surely be deleterious to the average mind, always remembering that
there are exceptions both higher and lower.   Such a result is only what is to be
expected when we consider that the brain is the part of man which develops and
changes as races reach a higher level, while the body remains practically constant
through ages.   To expect the brain to make sudden changes of ability would be as
reasonable as to expect a cart-horse to breed racers, or a greyhound to tend sheep.

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[1] E. H. Man, ‘ On the Andaman Islands,’ *Anthrop. Jour.*, xiv. 265.

Man mainly develops by internal differences in his brain structure, as other animals develop by external differences in bones and muscles.

What, then, it may be asked, can be done to elevate other races? How can we benefit them? Most certainly not by Europeanising them. By real education, leading out the mind to a natural and solid growth, much can be done; but not by enforcing a mass of accomplishments and artificialities of life. The general impression in England is that reading, writing, and arithmetic are the elements of education. They might be so to us, 'in the foremost files of time,' but they assuredly are not so to other races. The complex ideas of connecting forms and sounds is far too great a step for many brains; and when we succeed, to our delight, in turning out finished readers, Nature comes in with the stern reply, 'Of their children not one has been reared.' Our bigoted belief in reading and writing is not in the least justified when we look at the mass of mankind. The exquisite art and noble architecture of Mykenæ, the undying song of Homer, the extensive trade of the Bronze Age, all belonged to people who never read or wrote. At this day some of my best friends—in Egypt—are happily ignorant of such accomplishments, and assuredly I never encourage them to any such useless waste of their brains. The great essentials of a valuable character—moderation, justice, sympathy, politeness and consideration, quick observation, shrewdness, ability to plan and pre-arrange, a keen sense of the uses and properties of things— all these are the qualities on which I value my Egyptian friends, and such qualities are what should be evolved by any education worth the name. No brain, however humble, will be the worse for such education which is hourly in use; while in the practical life of a simple community the accomplishments of reading and writing are not needed for perhaps a week or a month at a time. The keenest interest is taken by some races, and probably by all, in geography, modes of government, and social systems; and in most countries elements of hygiene and improvements in the dwellings and arts of life may be taught with the best results. There is therefore a very wide field for the education of even the lowest races, without throwing any great strain on the mental powers. And it must always be remembered that memory is far more perfect where a less burden of learning is thrown on the mind, and ideas and facts can be remembered and brought into use more readily by minds unstrained by artificial instruction.

The greatest educational influence, however, is example. This is obvious when we see how rapidly the curses of our civilisation spread among those unhappily subjected to it. The contact of Europeans with lower races is almost always a detriment, and it is the severest reflection on ourselves that such should be the case It is a subject which has given much room for thought in my own dealings with the Egyptian peasant to consider how this deleterious effect is produced, and how it is to be avoided. Firstly, it is due to carelessness in leaving temptations open to natives, which may be no temptations to ourselves. To be careless about sixpences is as demoralising to them as a man who tossed sovereigns about the street would be to us. Examples of carelessness in this point are among the worst of influences. Another injury is the inducement to natives to imitate the ways and customs of Europeans without reason. Every imitation, as mere imitation, is a direct injury to character; it teaches a man to trust to some one else instead of thinking for himself; it induces a belief in externals constituting our superiority, while foresight and self-restraint are the real roots of it; and it destroys all chance of any real and solid growth of character which can flourish independently. A native should always be discouraged from any imitation, unless he attempts it as an intelligent improvement on his own habits. Another sadly common evil is the abuse of power, which lowers that sense of self-respect, of honour, and of honesty which can be found in most races. If a man or a government defrauds, it is but natural to the sufferer to try and recompense himself by any means available; and thus an interminable system of reprisals is set up. Such is the chronic state of the East at present among the more civilised races. The Egyptians are notorious for their avarice, and are usually credited with being inveterate money-grabbers; yet no sooner do they find that this system of reprisals is abandoned and strict justice maintained, than they at once respond to it; and I may say that when confidence

has once been gained it is almost as common to find a man dispute an account against his own interest as for himself, and scarcely ever is any attempt made at false statements or impositions. Such is the healthy response to straightforward dealing with them.

It is therefore in encouraging a healthy growth of all that is worthy and good in the existing systems of lower civilisation, in repressing all mere imitations and senseless copying, and in proceeding on a rigorously just yet genial course of conduct, that the safe and true line lies for intercourse with inferior or different civilisations.

And, lastly, the question comes home to us, In what way is this practical anthropology to be fostered? It is so essentially important to us as a race that we should take good care that it is understood. Whether it be a question of interference with the customs of higher races, as the Hindu, or of lower savages, as the Australian, momentous questions may often depend on public opinion amongst a mass of people in England who have no conception at present of the race with whom they are dealing. And still more needful is it for those who take part abroad in the governing of other races to have a wide view of the character of various civilisations. Until the present generation there have been two great educative influences on the view of life taken by Englishmen, the Old Testament and the Classics. So long as a boy had his ideas formed in contact with Oriental polygamy and Greek polytheism, he was not in danger of undue narrowness in dealing with the Muslim or the Hindu; but with the pressure of modern requirements both of these excellent views of other civilisations are being crowded out, and we meet men now to whom the world's history began when they were born. There is great danger in such ignorance. All the painful and laborious experiments in social and political problems during past ages are ignored, rash trials are made on lines which have been repeatedly proved to be impossible, and real advance in any direction is thwarted by useless repetitions of the well-known failures of the past.

It is the business of anthropology to step in, and make a knowledge of other civilisations a part of all decent education. In this direction our science has a most important field before it, at least as valuable as geography or history, and far more practical in developing ideas than many of the smatterings now taught. To present a view of another civilisation, we require to give an insight into the way of looking at the world, the modes of thought, the aims in life, the checks and counter-checks on the weaknesses of man, and the construction of society and of government, in each case. The origin and utility of the various customs and habits need to be pointed out, and in what way they are reasonable and needful to the well-being of the community. And above all, we ought to impress on every boy that this civilisation in which he grows is only one of innumerable experiments in life that have been tried; that it is by no means the only successful one, or perhaps not the most successful, that there has been; that there are many other solutions of the problems of community and culture which are as good as our own, and that no one solution will fit a different race, climate, or set of conditions.

How such a sense of proportion in the world is to be attained, and what course of instruction will eradicate political fanaticism, and plant a reasonable tolerance of other forms of civilisation, is the problem before us as practical anthropologists. The highest form of this perception of other existence is reached in the best history—writing or fiction, which enables the reader to strip himself for the time of his prejudices and views of life, and reclothe the naked soul with an entirely different personality and environment. Very few writers, and those only in rare instances, can reach this level; it needs consummate knowledge, skill, sympathy, and *abandon* in the writer, and if without these, it is neither accurate nor inspiring. The safer course is to carefully select from the best literature of a civilisation, and explain and illustrate this so as to leave no feature of it outside of the reason and feelings of the reader. Here we run against the special bigotry of the purely classical scholar, who looks on ancient literature as a peculiar preserve solely belonging to those who will labour to read it in its original dress. No one limits an acquaintance with Hebrew, Egyptian, or Arabic authors to those who can deal with those

tongues; and Greek and Latin authors ought to be as familiar to the English reader as Milton or Macaulay. To say that because it is impossible in a business education to give several years to a working knowledge of ancient languages, that therefore all thought written in those languages shall be a sealed book, is pedantry run mad. A few months, or even weeks, on translations will at least open the mind, and give an intelligent sense of the variety and the standpoint of the intellect of the past. And such a course is certainly better than the total ignorance which now prevails on such lines where the classics are not taught.

What seems to be the most practical course would be the recognition of civilisation or social life as a branch of general reading to be stimulated in schools, and encouraged by subsequent inquiry as to the extent to which it is followed and understood, without making it an additional fang of the examination demon.

The books required for such reading should cover the life of Greece, Rome, Babylon, Egypt, and Mexico in ancient times; and China, India, Persia, Russia, Spain, and one or two low civilisations, such as the Andamans and the Zulus, in modern times. Neither histories nor travels are wanted for this purpose; but a selection of the literature which shall most illustrate the social life and frame of the community, with full explanation and illustrations. We need not to excite wonder, astonishment, or disgust; but rather to enable the reader to realise the daily life, and to live in the very minds of the people. Where no literature is available, a vivid study of the nature of the practical working of their civilisation should take its place.

Such is the practical scope of anthropology in our daily life, where it needs as much consideration and will exercise as great an influence as any of the other subjects dealt with by this Association.

# British Association for the Advancement of Science.

## IPSWICH, 1895.

# ADDRESS

### TO THE

# BOTANICAL SECTION

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### PRESIDENT OF THE SECTION.

THE establishment of a new Section of the British Association, devoted to Botany, cannot but be regarded by the botanists of this country as an event of the greatest importance. For it is practically the first time that they have possessed an independent organisation of their own. It is true that for some years past we have generally been strong enough to form a separate department of the old Biological Section D, on the platform of which so many of us in the past have acted in some capacity or other, and on which indeed many of us may be said to have made our first appearance. We shall not start then on our new career without the remembrance of filial affection for our parent, and the earnest hope that our work may be worthy of its great traditions.

The first meeting of the Section, or, as it was then called Committee, at Oxford was held in 1832. And though there has been from time to time some difference in the grouping of the several biological sciences, the two great branches of biology have only now for the first time formally severed the partnership into which they entered on that occasion. That this severance, if inevitable from force of circumstances, is in some respects a matter of regret, I do not deny. Specialisation is inseparable from scientific progress; but it will defeat its own end in biology if the specialist does not constantly keep in touch with those fundamental principles which are common to all organic nature. We shall have to take care that we do not drift into a position of isolation. Section D undoubtedly afforded a convenient opportunity for discussing many questions on which it was of great advantage that workers in the two different fields should compare their results and views. But I hope that by means of occasional conferences we shall still, in some measure, be able to preserve this advantage.

### RETROSPECT.

I confess I found it a great temptation to review, however imperfectly, the history and fortunes of our subject while it belonged to Section D. But to have done so would have been practically to have written the history of botany in this country since the first third of the century. Yet I cannot pass over some few striking events.

I think that the earliest of these must undoubtedly be regarded as the most epoch-making. I mean the formal publication by the Linnean Society, in 1833,

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of the first description of 'the nucleus of the cell,' by Robert Brown [1]  It seems
difficult to realise that this may be within the recollection of some who are now
living amongst us.  It is, however, of peculiar interest to me that the first person
who actually distinguished this all-important body, and indicated it in a figure,
was Francis Bauer, thirty years earlier, in 1802   This remarkable man, whose
skill in applying the resources of art to the illustration of plant anatomy has never,
I suppose, been surpassed, was 'resident draughtsman for fifty years to the Royal
Botanic Garden at Kew.'  And it was at Kew, and in a tropical orchid, *Phaius
grandifolius*, no doubt grown there, that the discovery was made.

It was, I confess, with no little admiration that, on refreshing my memory by a
reference to Robert Brown's paper, I read again the vivid account which he gives
in a footnote of the phenomena, so painfully familiar to many of us who have been
teachers, exhibited in the staminal hair of *Tradescantia*.  Sir Joseph Hooker [2] has
well remarked that 'the supreme importance of this observation, . . . leading to
undreamt-of conceptions of the fundamental phenomena of organic life, is acknow-
ledged by all investigators.'  It is singular that so profound an observer as Robert
Brown should have himself missed the significance of what he saw.  The world
had to wait for the discovery of protoplasm by Von Mohl till 1846, and till 1850
for its identification with the sarcode of zoologists by Cohn, who is still, I am
happy to say, living and at work, and to whom last year the Linnean Society did
itself the honour of presenting its medal.

The Edinburgh meeting of the Association, in 1834, was the occasion of the
announcement of another memorable discovery of Robert Brown's   I will content
myself with quoting Hofmeister's [3] account of it.  'Robert Brown was the dis-
coverer of the polyembrony of the *Coniferæ*.  In a later treatise he pointed out the
origin of the pro-embryo in large cells of the endosperm, to which he gave the
name of corpuscula.'  The period of the forties, just half a century ago, looks in the
retrospect as one of almost dazzling discovery.  To say nothing of the formal ap-
pearance of protoplasm on the scene, the foundations were being laid in all direc-
tions of our modern botanical morphology.  Yet its contemporaries viewed it with
a very philosophical calm.  Thwaites, who regarded Carpenter as his master,
described at the Oxford meeting in 1847 the conjugation of the *Diatomaceæ*, and
'distinctly indicated,' as Carpenter [4] says, 'that conjugation is the primitive phase
of sexual reproduction.'  Berkeley informed me that the announcement fell per-
fectly flat.  A year or two later Suminski came to London with his splendid
discovery (1848) of the archegonia of the fern, the antheridia having been
first seen by Nägeli in 1844.  Carpenter [5] gave me, many years after, a curious
account of its reception.  'At the Council of the Ray Society, at which,'
he said, 'I advocated the reproduction of Suminski's book on the "Ferns," I
was assured that the close resemblance of the antherozoids to spermatozoa
was quite sufficient proof that they could have nothing to do with vegetable
reproduction.  'I do not think,' he added—and the complaint is pathetic—'that
the men of the present generation, who have been brought up in the *light*, quite
apprehend (in this as in other matters) the utter darkness in which we were then
groping, or fully recognise the deserts of those who helped them to what they now
enjoy.'  This was in 1875, and I suppose is not likely to be less true now.

The Oxford Meeting in 1860 was the scene of the memorable debate on the
origin of species, at which it is interesting to remember that Henslow presided.
On that occasion Section D reached its meridian.  The battle was Homeric.  How-
ever little to the taste of its author, the launching of his great theory was, at any
rate, dignified with a not inconsiderable explosion.  It may be that it is not given
to the men of our day to ruffle the dull level of public placidity with disturbing
and far-reaching ideas.  But if it were, I doubt whether we have, or need now,
the fierce energy which inspired then either the attack or the defence.  When we
met again in Oxford last year the champion of the old conflict stood in the place of
honour, acclaimed of all men, a beautiful and venerable figure.  We did not know
then that that was to be his farewell.

[1] *Misc. Bot. Works*, i. 512          [2] *Proc. Linn. Soc*, 1887–88, 65.
[3] *Higher Cryptogamia*, 432.   [4] *Memorial Sketch*, 140.   [5] *Loc. cit.*, 141.

The battle was not in vain. Six years afterwards, at Nottingham, Sir Joseph Hooker delivered his classical lecture on Insular Floras. It implicitly accepted the new doctrine, and applied it with admirable effect to a field which had long waited for an illuminating principle. The lecture itself has since remained one of the corner-stones of that rational theory of the geographical distribution of plants which may, I think, be claimed fairly as of purely English origin.

## HENSLOW.

Addressing you as I do at Ipswich, there is one name written in the annals of our old Section which I cannot pass over—that of Henslow. He was the Secretary of the Biological Section at its first meeting in 1832, and its President at Bristol in 1836. I suppose there are few men of this century who have indirectly more influenced the current of human thought. For in great measure I think it will not be contested that we owe Darwin to him As Romanes has told us : [1] 'His letters written to Professor Henslow during his voyage round the world overflow with feelings of affection, veneration, and obligation to his accomplished master and dearest friend—feelings which throughout his life he retained with no diminished intensity. As he used himself to say, before he knew Professor Henslow the only objects he cared for were foxes and partridges.' I do not wish to overstate the facts The possession of 'the collector's instinct, strong in Darwin from his childhood, as is usually the case in great naturalists,' to use Huxley's [2] words, would have borne its usual fruit in after life, in some shape or other, even if Darwin had not fallen into Henslow's hands. But then the particular train of events which culminated in the great work of his life would never have been started. It appeared to me, then, that it would not be an altogether uninteresting investigation to ascertain something about Henslow himself. The result has been to provide me with several texts, which I think it may be not unprofitable to dwell upon on the present occasion.

In the first place, what was the secret of his influence over Darwin? 'My dear old master in Natural History' ('Life,' i. 317) he calls him ; and to have stood in this relation to Darwin [3] is no small matter. Again, he speaks of his friendship with him as 'a circumstance which influenced my whole career more than any other' (i. 52). The singular beauty of Henslow's character, to which Darwin himself bore noble testimony, would count for something, but it would not in itself be a sufficient explanation. Nor was it that intellectual fascination which often binds pupils to the master's feet; for, as Darwin tells us, 'I do not suppose that anyone would say that he possessed much original genius' (i. 52). The real attraction seems to me to be found in Henslow's possession, in an extraordinary degree, of what may be called the Natural History spirit. This resolves itself into keen observation and a lively interest in the facts observed. 'His strongest taste was to draw conclusions from long-continued minute observations' (i. 52). The old Natural History method, of which it seems to me that Henslow was so striking an embodiment, is now, and I think unhappily, almost a thing of the past. The modern university student of botany puts his elders to blush by his minute knowledge of some small point in vegetable histology. But he can tell you little of the contents of a country hedgerow; and if you put an unfamiliar plant in his hands he is pretty much at a loss how to set about recognising its affinities. Disdaining the field of nature spread at his feet in his own country, he either seeks salvation in a German laboratory or hurries off to the Tropics, convinced that he will at once immortalise himself. But '*cœlum non animum mutat*'; he puts into 'pickle' the same objects as his predecessors, never to be looked at again; or perhaps writes a paper on some obvious phenomena which he could have studied with less fatigue in the Palm House at Kew.

The secret of the right use of travel is the possession of the Natural History instinct, and to those who contemplate it I can only recommend a careful study of Darwin's 'Naturalist's Voyage.' Nothing that came in his way seems to have

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[1] *Memorial Notices*, 13          [2] *Proc. R S*, xliv vi.
[3] As I shall have frequent occasion to quote the *Life and Letters*, I shall insert the references in the text.

evaded him or to have seemed too inconsiderable for attention. No doubt some respectable travellers have lost themselves in a maze of observations that have led to nothing. But the example of Darwin, and I might add of Wallace, of Huxley, and of Moseley, show that that result is the fault of the man and not of the method. The right moment comes when the fruitful opportunity arrives to him who can seize it. The first strain of the prelude with which the 'Origin' commences are these words· 'When on board H.M S. "Beagle" as naturalist, I was much struck with certain facts in the distribution of the organic beings inhabiting South America.' But this sort of vein is not struck at hazard or by him who has not served a tolerably long apprenticeship to the work.

When one reads and re-reads the 'Voyage,' it is simply amazing to see how much could be achieved with a previous training which we now should think ludicrously inadequate. Before Henslow's time the state of the natural sciences at Cambridge was incredible. In fact, Leonard Jenyns,[1] his biographer, speaks of the 'utter disregard paid to Natural History in the University previous to his taking up his residence there.' The Professor of Botany had delivered no lectures for thirty years, and though Sir James Smith, the founder of the Linnean Society, had offered his services, they were declined on the ground of his being a Nonconformist.[2]

As to Henslow's own scientific work, I can but rely on the judgment of those who could appreciate it in relation to its time. According to Berkeley,[3] 'he was certainly one of the first, if not the very first, to see that two forms of fruit might exist in the same fungus.' And this, as we now know, was a fundamental advance in this branch of morphology. Sir Joseph Hooker tells me that his papers were all distinctly in advance of his day. Before occupying the chair of botany, he held for some years that of mineralogy. Probably he owed this to his paper on the Isle of Anglesey, published when he was only twenty-six. I learn from the same authority that this to some extent anticipated, but at any rate strongly influenced, Sedgwick's subsequent work in the same region.

## BOTANICAL TEACHING.

Henslow's method of teaching deserves study. Darwin says of his lectures 'that he liked them much for their extreme clearness.' 'But,' he adds, 'I did not study botany' (i. 48). Yet we must not take this too seriously. Darwin,[4] when at the Galapagos, 'indiscriminately collected everything in flower on the different islands, and fortunately kept my collections separate.' Fortunately indeed ; for it was the results extracted from these collections, when worked up subsequently by Sir Joseph Hooker, which determined the main work of his life. 'It was such cases as that of the Galapagos Archipelago which chiefly led me to study the origin of species' (iii. 159).

Henslow's actual method of teaching went some way to anticipate the practical methods of which we are all so proud. 'He was the first to introduce into the botanical examination for degrees in London the system of practical examination.'[5] But there was a direct simplicity about his class arrangements characteristic of the man. 'A large number of specimens . . . were placed in baskets on a side-table in the lecture-room, with a number of wooden plates and other requisites for dissecting them after a rough fashion, each student providing himself with what he wanted before taking his seat.'[6] I do not doubt that the results were, in their way, as efficient as we obtain now in more stately laboratories.

The most interesting feature about his teaching was not, however, its academic aspect, but the use he made of botany as a general educational instrument. 'He always held that a man of *no* powers of observation was quite an exception.'[7] He thought (and I think he proved) that botany might be used 'for strengthening the observant faculties and expanding the reasoning powers of children in all classes of society.'[8] The difficulty with which those who undertake now to teach our subject have to deal is that most people ask the question, What is the use of

[1] *Memoir*, 175.        [2] *Ibid.*, 37.        [3] *Ibid*, 56        [4] *Voyage*, 421.
[5] *Memoir*, 161.        [6] *Ibid.*, 39.        [7] *Ibid*, 163.        [8] *Ibid*, 99

learning botany unless one means to be a botanist? It might indeed be replied that as the vast majority of people never learn anything effectively, they might as well try botany as anything else. But Henslow looked only to the mental discipline; and it was characteristic of the man and of his belief in his methods that when he was summoned to Court to lecture to the Royal family, his lectures 'were, in all respects, identical with those he was in the habit of giving to his little Hitcham scholars';[1] and it must be added that they were not less successful.

This success naturally attracted attention. Botanical teaching in schools was taken up by the Government, and continues to receive support to the present day. But the primitive spirit has, I am afraid, evaporated. The measurement of results by means of examination has been fatal to its survival. The teacher has to keep steadily before his eyes the necessity of earning his grant. The educational problem retires into the background. 'The strengthening of the observant faculties,' and the rest of the Henslowian programme must give way to the imperious necessity of presenting to the examiner candidates equipped with at least the minimum of text-book formulas reproducible on paper. I do not speak in this matter without painful experience. The most astute examiner is defeated by the still more astute crammer. The objective basis of the study on which its whole usefulness is built up is promptly thrown aside. If you supply the apple blossom for actual description, you are as likely as not to be furnished with a detailed account of a buttercup. The training of observation has gone by the board, and the exercise of mere memory has taken its place. But a table of logarithms or a Hebrew grammar would serve this purpose equally well. Yet I do not despair of Henslow's work still bearing fruit. The examination system will collapse from the sheer impossibility of carrying it on beyond a certain point. Freed from its trammels, the teacher will have greater scope for individuality, and the result of his labours will be rewarded after some intelligent system of inspection. And here I may claim support from an unexpected quarter. Mr. Gladstone has recently written to a correspondent:—'I think that the neglect of natural history, in all its multitude of branches, was the grossest defect of our old system of training for the young; and, further, that little or nothing has been done by way of remedy for that defect in the attempts made to alter or reform that system.' I am sure that the importance and weight of this testimony, coming as it does from one whose training and sympathies have always been literary, cannot be denied. That there is already some revival of Henslow's methods, I judge from the fact that I have received applications from Board schools, amounting to some hundreds, for surplus specimens from the Kew museums. Without a special machinery for the purpose I cannot do much, and perhaps it is well. But my staff have willingly done what was possible, and from the letters I have received I gather that the labour has not been wholly misspent.

<center>MUSEUM ARRANGEMENT.</center>

This leads me to the last branch of Henslow's scientific work on which I am able to touch, that of the arrangement of museums, especially those which being local have little meaning unless their purpose is strictly educational. I think it is now generally admitted that, both in the larger and narrower aspects of the question, his ideas, which were shared in some measure by Edward Forbes, were not merely far in advance of his time, but were essentially sound. And here I cannot help remarking that the zoologists have perhaps profited more by his teaching than the botanists. I do not know how far Sir William Flower and Professor Lankester would admit the influence of Henslow's ideas. But, so far as my knowledge goes, I am not aware that, at any rate in Europe, there is anything to be seen in public museums comparable to the educational work accomplished by the one at the College of Surgeons and the Natural History Museum, and by the other at Oxford.

I have often thought it singular that in botany we have not kept pace in this matter with our brother naturalists. I do not doubt that vegetable morphology and a vast number of important facts in evolution, as illustrated from the

vegetable kingdom, might be presented to the eye in a fascinating way in a carefully arranged museum. The most successful and, indeed, almost the only attempt which has been made in this direction is that at Cambridge, which, I believe, is due to Mr. Gardiner. But our technical methods for preserving specimens still leave much to desire. Something more satisfactory will, it may be hoped, some day be devised, and the whole subject is one which is well worth the careful consideration of our Section. Henslow at least effected a vast improvement in the mode of displaying botanical objects; and a collection prepared by his own hands, which was exhibited at one of the Paris exhibitions, excited the warm admiration of the French botanists, who always appreciate the clear illustration of morphological facts.

### OLD SCHOOL OF NATURAL HISTORY.

If the old school of natural history of which Henslow in his day was a living spirit is at present, as seems to be the case, continually losing its hold upon us, this has certainly not been due to its want of value as an educational discipline, or to its sterility in contributing new ideas to human knowledge. Darwin's 'Origin of Species' may certainly be regarded as its offspring, and of this Huxley [1] says with justice: 'It is doubtful if any single book, except the "Principia," ever worked so great and rapid a revolution in science, or made so deep an impression on the general mind.' Yet Darwin's biographer, in that admirable Life which ranks with the few really great biographies in our language, remarks (i. 155): 'In reading his books one is reminded of the older naturalists rather than of the modern school of writers. He was a naturalist in the old sense of the word, that is, a man who works at many branches of science, not merely a specialist in one.' This is no doubt true, but does not exactly hit off the distinction between the kind of study which has gone out of fashion and that which has come in. The older workers in biology were occupied mainly with the external or, at any rate, grosser features of organisms and their relation to surrounding conditions; the modern, on the other hand, are engaged on the study of internal and intimate structure. Work in the laboratory, with its necessary limitations, takes the place of research in the field. One may almost, in fact, say that the use of the compound microscope divides the two classes. Asa Gray has compared Robert Brown with Darwin as the 'two British naturalists' who have, 'more than any others, impressed their influence upon science in the nineteenth century.' [2] Now it is noteworthy that Robert Brown did all his work with a simple microscope. And Francis Darwin writes of his father: 'It strikes us nowadays as extraordinary that he should have had no compound microscope when he went his "Beagle" voyage; but in this he followed the advice of Robert Brown, who was an authority on such matters' (i. 145). One often meets with persons, and sometimes of no small eminence, who speak as if there were some necessary antagonism between the old and the new studies. Thus I have heard a distinguished systematist describe the microscope as a curse, and a no less distinguished morphologist speak of a herbarium having its proper place on a bonfire. To me I confess this anathematisation of the instruments of research proper to any branch of our subject is not easily intelligible. Yet in the case of Darwin himself it is certain that if his earlier work may be said to rest solely on the older methods, his later researches take their place with the work of the new school. At our last meeting Pfeffer vindicated one of his latest and most important observations.

The case of Robert Brown is even more striking. He is equally great whether we class him with the older or the modern school. In fact, so far as botany in this country is concerned, he may be regarded as the founder of the latter. It is to him that we owe the establishment of the structure of the ovule and its development into the seed. Even more important were the discoveries to which I have already referred, which ultimately led to the establishment of the group of Gymnosperms. 'No more important discovery,' says Sachs, [3] 'was ever made in the domain of comparative morphology and systematic botany. The first steps towards this result, which was clearly brought out by Hofmeister twenty-five years later,

were secured by Robert Brown's researches, and he was incidentally led to these researches by some difficulties in the construction of the seed of an Australian genus.' Yet it may be remembered that he began his career as naturalist to Flinders's expedition for the exploration of Australia. He returned to England with 4,000 ' for the most part new species of plants.' And these have formed the foundation of our knowledge of the flora of that continent. Brown's chief work was done between 1820 and 1840, and, as Sachs[1] tells us, ' was better appreciated during that time in Germany than in any other country.'

### Modern School.

The real founder of the modern teaching in this country in both branches of biology I cannot doubt was Carpenter. The first edition of his admirable ' Principles of Comparative Physiology' was published in 1838, the last in 1854. All who owe, as I do, a deep debt of gratitude to that book will agree with Huxley[2] in regarding it as ' by far the best general survey of the whole field of life and of the broad principles of biology which had been produced up to the time of its publication. Indeed,' he adds, ' although the fourth edition is now in many respects out of date, I do not know its equal for breadth of view, sobriety of speculation, and accuracy of detail.'

The charm of a wide and philosophic survey of the different forms under which life presents itself could not but attract the attention of teachers. Rolleston elaborated a course of instruction in zoology at Oxford in which the structures described in the lecture-room were subsequently worked out in the laboratory. In 1872 Huxley organised the memorable course in elementary biology at South Kensington which has since, in its essential features, been adopted throughout the country. In the following year, during Huxley's absence abroad through ill-health, I arranged, at his request, a course of instruction on the same lines for the Vegetable Kingdom

That the development of the new teaching was inevitable can hardly be doubted, and I for my part am not disposed to regret the share I took in it. But it was not obvious, and certainly it was not expected. that it would to so large an extent cut the ground from under the feet of the old Natural History studies. The consequences are rather serious, and I think it is worth while pointing them out.

In a vast empire like our own there is a good deal of work to be done and a good many posts to be filled, for which the old Natural History training was not merely a useful but even a necessary preparation. But at the present time the universities almost entirely fail to supply men suited to the work. They neither care to collect, nor have they the skilled aptitude for observation. Then, though this country is possessed at home of incomparable stores of accumulated material, the class of competent amateurs who were mostly trained at our universities and who did such good service in working that material out is fast disappearing. It may not be easy indeed in the future to fill important posts even in this country with men possessing the necessary qualifications. But there was still another source of naturalists, even more useful, which has practically dried up. It is an interesting fact that the large majority of men of the last generation who have won distinction in this field have begun their career with the study of medicine. That the kind of training that Natural History studies give is of advantage to students of medicine which, rightly regarded, is itself a Natural History study, can hardly be denied. But the exigencies of the medical curriculum have crowded them out, and this, I am afraid, must be accepted as irremediable. I cannot refrain from reading you, on this point, an extract from a letter which I have received from a distinguished official lately entrusted with an important foreign mission. I should add that he had himself been trained in the old way.

' I have had my time, and must leave to younger men the delight of working these interesting fields. Such chances never will occur again, for roads are now being made and ways cut in the jungle and forest, and you have at hand all sorts of trees level on the ground ready for study. These bring down with them orchids, ferns, and climbers of many kinds, including rattan palms, &c. But, excellent as are the officers who devote their energy to thus opening up this country, there is not one

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[1] *Loc cit.*, 139, 140.    [2] *Memorial Sketch*, 67.

man who knows a palm from a dragon-tree, so the chance is lost. Strange to say, the medical men of the Government service know less and care less for Natural History than the military men, who at least regret they have no training or study to enable them to take an intelligent interest in what they see around them. A doctor nowadays cares for no living thing larger or more complicated than a *bacterium* or a *bacillus*.'

But there are other and even more serious grounds why the present dominance of one aspect of our subject is a matter for regret. In the concluding chapter of the 'Origin,' Darwin wrote : 'I look with confidence to the future—to young and rising naturalists.' But I observe that most of the new writers on the Darwinian theory, and, oddly enough, especially when they have been trained at Cambridge, generally begin by more or less rejecting it as a theory of the origin of species, and then proceed unhesitatingly to reconstruct it. The attempt rarely seems to me successful, perhaps because the limits of the laboratory are unfavourable to the accumulation of the class of observations which are suitable for the purpose. The laboratory, in fact, has not contributed much to the Darwinian theory, except the 'Law of Recapitulation,' and that, I am told, is going out of fashion.

The Darwinian theory, being, as I have attempted to show, the outcome of the Natural History method, rested at every point on a copious basis of fact and observation. This more modern speculation lacks. The result is a revival of transcendentalism. Of this we have had a copious crop in this country, but it is quite put in the shade by that with which we have been supplied from America. Perhaps the most remarkable feature is the persistent vitality of Lamarckism. As Darwin remarks: 'Lamarck's one suggestion as to the cause of the gradual modification of species—effort excited by change of conditions—was, on the face of it, inapplicable to the whole vegetable world' (ii. 189). And if we fall back on the inherited direct effect of change of conditions, though Darwin admits that 'physical conditions have a more direct effect on plants than on animals' (ii. 319), I have never been able to convince myself that that effect is inherited. I will give one illustration. The difference in habit of even the same species of plant when grown under mountain and lowland conditions is a matter of general observation. It would be difficult to imagine a case of 'acquired characters' more likely to be 'inherited.' But this does not seem to be the case. The recent careful research of Gaston Bonnier only confirms the experience of cultivators. 'The modifications acquired by the plant when transported for a definite time from the plains to the Alps, or *vice versâ*, disappear at the end of the same period when the plant is restored to its original conditions.'[1]

Darwin, in an eloquent passage, which is too long for me to quote,[2] has shown how enormously the interest of Natural History is enhanced 'when we regard every production of Nature as one which has had a long history,' and 'when we contemplate every complex structure . . . as the summing up of many contrivances.' But this can only be done, or at any rate begun, in the field and not in the laboratory.

A more serious peril is the dying out amongst us of two branches of botanical study in which we have hitherto occupied a position of no small distinction. Apart from the staffs of our official institutions, there seems to be no one who either takes any interest in, or appreciates in the smallest degree, the importance of systematic and descriptive botany. And geographical distribution is almost in a worse plight, yet Darwin calls it, 'that grand subject, that almost keystone of the laws of creation' (i. 356).

I am aware that it is far easier to point out an evil than to remedy it. The teaching of botany at the present day has reached a pitch of excellence and earnestness which it has never reached before. That it is somewhat one-sided cannot probably be remedied without a subdivision of the subject and an increase in the number of teachers. If it has a positive fault, it is that it is sometimes inclined to be too dogmatic and deductive. Like Darwin, at any rate in a biological matter, 'I never feel convinced by deduction, even in the case of

[1] *Ann. d. Sc. nat.*, 7ᵉ sér. xx. 355      [2] *Origin*, 426.

H. Spencer's writings' (iii. 168). The intellectual indolence of the student inclines him only too gladly to explain phenomena by referring them to 'isms,' instead of making them tell their own story.

## ORGANISATION OF SECTION.

I am afraid I have detained you too long over these matters, on which I must admit I have spoken with some frankness. But I take it that one of the objects of our Section is to deliver our minds of any perilous stuff that is fermenting in it. But now, having taken leave of the past, let us turn to the future.

We start at least with a clean slate. We cannot bind our successors, it is true, at other meetings. But I cannot doubt that it will be in our power to materially shape our future, notwithstanding. When we were only a department I think we all felt the advantage of these annual meetings, of the profitable discussion formal and informal, and of the privilege of meeting so many of our foreign brethren who have so generously supported us by their presence and sympathy.

I am anxious, then, to suggest that we should conduct our proceedings on as broad lines as possible. I do not think we should be too ready to encourage papers which may well be communicated to societies, either local or central.

The field is large; the labourers as they advance in life can hardly expect to keep pace with all that is going on in it. We must look to individual members of our number to help us by informing and stimulating addresses on subjects they have made peculiarly their own, or on important researches on which they have been especially engaged.

## NOMENCLATURE.

There is one subject upon which, from my official position elsewhere, I desire to take the opportunity of saying a few words. It is that of Nomenclature. It is not on its technical side, I am afraid, of sufficient general interest to justify my devoting to it the space which its importance would otherwise deserve. But I hope to be able to enlist your support for the broad common-sense principles on which our practice should rest.

As I suppose, everyone knows we owe our present method of nomenclature in natural history to Linnæus. He devised the binominal, or, as it is often absurdly called, the binomial system. That we must have a technical system of nomenclature I suppose no one here will dispute. It is not, however, always admitted by popular writers who have not appreciated the difficulty of the matter, and who think all names should be in the vernacular. There is the obvious difficulty that the vast majority of plants do not possess any names at all, and the attempts to manufacture them in a popular shape have met with but little success. Then, from lack of discriminating power on the part of those who use them, vernacular names are often ambiguous; thus Bullrush is applied equally to *Typha* and to *Scirpus*, plants extremely different. Vernacular names, again, are only of local utility, while the Linnean system is intelligible throughout the world.

A technical name, then, for a plant or animal is a necessity, as without it we cannot fix the object of our investigations into its affinity, structure, or properties.[1] 'Nomina si nescis perit et cognitio rerum.'

In order to get clear ideas on the matter let us look at the logical principles on which such names are based. It is fortunate for us that these are stated by Mill, who, besides being an authority on logic, was also an accomplished botanist. He tells us:[2] 'A naturalist, for purposes connected with his particular science, sees reason to distribute the animal or vegetable creation into certain groups rather than into any others, and he requires a name to bind, as it were, each of his groups together.' He further explains that such names, whether of species, genera, or orders, are what logicians call connotative: they *denote* the members of each group, and *connote* the distinctive characters by which it is defined. A species, then, connotes the common characters of the individuals belonging to it; a genus, those of the species; an order, those of the genera.

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[1] *Linn. Phil*, 210.   [2] *System of Logic*, i 132

But these are the logical principles, which are applicable to names generally. A name such as *Ranunculus repens* does not differ in any particular from a name such as John Smith, except that one denotes a species, the other an individual.

This being the case, and technical names being a necessity, they continually pass into general use in connection with horticulture, commerce, medicine, and the arts. It seems obvious that, if science is to keep in touch with human affairs, stability in nomenclature is a thing not merely to aim at but to respect. Changes become necessary, but should never be insisted on without grave and solid reason. In some cases they are inevitable unless the taxonomic side of botany is to remain at a standstill. From time to time the revision of a large group has to be undertaken from a uniform and comparative point of view. It then often occurs that new genera are seen to have been too hastily founded on insufficient grounds, and must therefore be merged in others. This may involve the creation of a large number of new names, the old ones becoming henceforth a burden to literature as synonyms. It is usual in such cases to retain the specific portion of the original name, if possible. If it is, however, already preoccupied in the genus to which the transference is made, a new one must be devised. Many modern systematists have, however, set up the doctrine that a specific epithet once given is indelible, and whatever the taxonomic wanderings of the organism to which it was once assigned, it must always accompany it This, however, would not have met with much sympathy from Linnæus, who attached no importance to the specific epithet at all : ' Nomen specificum sine generico est quasi pistillum sine campana.' [1] Linnæus always had a solid reason for everything he did or said, and it is worth while considering in this case what it was.

Before his time the practice of associating plants in genera had made some progress in the hands of Tournefort and others, but specific names were still cumbrous and practically unusable. Genera were often distinguished by a single word ; and it was the great reform accomplished by Linnæus to adopt the binominal principle for species But there is this difference. Generic names are unique, and must not be applied to more than one distinct group. Specific names might have been constituted on the same basis ; the specific name in that case would then have never been used to designate more than one plant, and would have been sufficient to indicate it. We should have lost, it is true, the useful information which we get from our present practice in learning the genus to which the species belongs ; but theoretically a nomenclature could have been established on the one-name principle. The thing, however, is impossible now, even if it were desirable. A specific epithet like *vulgaris* may belong to hundreds of different species belonging to as many different genera, and taken alone is meaningless. A Linnean name, then, though it consists of two parts, must be treated as a whole. ' Nomen omne plantarum constabit nomine generico et specifico.' [2] A fragment can have no vitality of its own. Consequently, if superseded, it may be replaced by another which may be perfectly independent.[3]

It constantly happens that the same species is named and described by more than one writer, or different views are taken of specific differences by various writers ; the species of one are therefore ' lumped ' by another. In such cases, where there is a choice of names, it is customary to select the earliest published. I agree, however, with the late Sereno Watson [4] that ' there is nothing whatever of an ethical character inherent in a name, through any priority of publication or position, which should render it morally obligatory upon anyone to accept one name rather than another.' And in point of fact Linnæus and the early systematists attached little importance to priority. The rigid application of the principle involves the assumption that all persons who describe or attempt to describe plants

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[1] *Phil*, 219.                 [2] *Phil*, 212.

[3] As Alphonse de Candolle points out in a letter published in the *Bull. de la Soc. bot. de France* (xxxix), ' the real merit of Linnæus has been to combine, for all plants, the generic name with the specific epithet.' It is important to remember that in a logical sense the ' name ' of a species consists, as Linnæus himself insisted, in the combination, not in the specific epithet, which is a mere fragment of the name, and meaningless when taken by itself.        [4] *Nature*, xlvii 54.

are equally competent to the task. But this is so far from being the case that it is sometimes all but impossible even to guess what could possibly have been meant.[1]

In 1872 Sir Joseph Hooker [2] wrote : ' The number of species described by authors who cannot determine their affinities increases annually, and I regard the naturalist who puts a described plant into its proper position in regard to its allies as rendering a greater service to science than its describer when he either puts it into a wrong place or throws it into any of those chaotic heaps, miscalled genera, with which systematic works still abound.' This has always seemed to me not merely sound sense, but a scientific way of treating the matter. What we want in nomenclature is the maximum amount of stability and the minimum amount of change compatible with progress in perfecting our taxonomic system. Nomenclature is a means, not an end. There are perhaps 150,000 species of flowering plants in existence What we want to do is to push on the task of getting them named and described in an intelligible manner, and their affinities determined as correctly as possible. We shall then have material for dealing with the larger problems which the vegetation of our globe will present when treated as a whole. To me the botanists who waste their time over priority are like boys who, when sent on an errand, spend their time in playing by the roadside. By such men even Linnæus is not to be allowed to decide his own names To one of the most splendid ornaments of our gardens he gave the name of *Magnolia grandiflora :* this is now to be known as *Magnolia fœtida.* The reformer himself is constrained to admit, ' The change is a most unfortunate one in every way.' [3] It is difficult to see what is gained by making it, except to render systematic botany ridiculous. The genus *Aspidium,* known to every fern-cultivator, was founded by Swartz. It now contains some 400 species, of which the vast majority were of course unknown to him at the time ; yet the names of all these are to be changed because Adanson founded a genus, *Dryopteris,* which seems to be the same thing as *Aspidium.* What, it may be asked, is gained by the change ? To science it is certainly nothing. On the other hand, we lumber our books with a mass of synonyms, and perplex everyone who takes an interest in ferns. It appears that the name of the well-known Australian genus *Banksia* really belongs to *Pimelea :* the species are therefore to be renamed, and *Banksia* is to be rechristened *Sirmuellera,* after Sir Ferdinand von Mueller ; a proposal which, I need hardly say, did not emanate from an Englishman.

I will not multiply instances. But the worst of it is that those who have carefully studied the subject know that, from various causes which I cannot afford the time to discuss, when once it is attempted to disturb accepted nomenclature it is almost impossible to reach finality. Many genera only exist by virtue of their redefinition in modern times : in the form in which they were originally promulgated they have hardly any intelligible meaning at all

It can hardly be doubted that one cause of the want of attention which systematic botany now receives is the repulsive labour of the bibliographical work with which it has been overlaid. What an enormous bulk nomenclature has already attained may be judged from the *Index Kewensis,* which was prepared at Kew, and which we owe to the munificence of Mr. Darwin In his own studies he constantly came on the track of names which he was unable to run down to their source. This the *Index* enables to be done. It is based, in fact, on a manuscript index which we compiled for our own use at Kew. But it is a mistake to suppose that it is anything more than the name signifies, or that it expresses any opinion, as to the validity of the names themselves. That those who use the book must judge of for themselves. We have indexed existing names, but we have not added to the burden by making any new ones for species already described.

What synonymy has now come to may be judged by an example supplied me by my friend Mr. C. B Clarke. For a single species of *Fimbristylis* he finds 135

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[1] Darwin, who always seems to me, almost instinctively, to take the right view in matters relating to natural history, is (*Life,* vol. 1 p 364) dead against the new ' practice of naturalists appending for perpetuity the name of the *first* describer to species ' He is equally against the priority craze :—' I cannot yet bring myself to reject very *well-known* names ' (*ibid*, p. 369).

[2] *Flora of British India,* i. vii.         [3] *Garden and Forest,* ii. 615.

published names under six genera. If we go on in this way we shall have to invent a new Linnæus, wipe out the past, and begin all over again.

Although I have brought the matter before the Section it is not one in which this, or indeed any collective assembly of botanists, can do very much. While I hope I shall carry your assent with the general principles I have laid down, it must be admitted that the technical details can only be appreciated by experienced specialists. All that can be hoped is a general agreement amongst the staffs of the principal institutions in different countries where systematic botany is worked at ; the free-lances must be left to do as they like.

### PUBLICATIONS.

I have dwelt at such length on certain aspects of my subject that perhaps, without great injustice, you may retort on me the complaint of one-sidedness. But when I survey the larger field of botany in this country, the prospect seems to me so vast that I should despair even if I had my whole address at my disposal of doing it justice. I think that its extent is measured by the way in which the publications belonging to our subject are maintained. First of all, we have access to the Royal Society, a privilege of which I hope we shall always continue to take advantage for communications which either treat of fundamental subjects, or at least are of general interest to biologists. Next to this we have our ancient Linnean Society, with a branch of its publications handsomely and efficiently devoted to systematic work. Then we have the 'Annals of Botany,' which has now, I think, established its position, and which brings together the chief morphological and physiological work accomplished in the country. Lastly, we have the 'Journal of Botany,' a less ambitious but useful periodical, which is mainly devoted to the labours of British botanists. I remember there was a time when I thought that this, at any rate, was an exhausted field. But it is not so ; knowledge in its most limited aspects is inexhaustible if the labourer have the necessary insight. The discoveries of Mr. Arthur Bennett amongst the potamogetons of the Eastern Counties is a striking and brilliant instance.

Besides the publication of the 'Annals' we owe to the Oxford Press a splendid series of the best foreign text-books issued in our own language. If the thought has sometimes occurred to one's mind that we were borrowers too freely from our indefatigable neighbours, I, at least, remember that the late Professor Eichler paid us the compliment of saying that he preferred to read one of these monumental books in the English translation rather than in the original. I believe it is no secret that botany owes the aid that Oxford has rendered it in these and other matters in great measure to my old friend the Master of Pembroke College, than whom I believe science has no more devoted supporter.

### PALÆOBOTANY.

I have said much of recent botany ; I must not pass over that of past ages. Two notable workers in this field have passed away since our last meeting. Saporta was with us at Manchester, and we shall not readily forget his personal charm. If some of his work has about it a too imaginative character, the patience and entire sincerity with which he traced the origin of the existing forms of vegetation in Southern Europe to their ancestors in the not distant geological past will always deserve attentive study. But in the venerable, yet always youthful, Williamson we lose a figure whose memory we shall long preserve. With rare instinct he accumulated a wealth of material illustrative of the vegetation of the Carboniferous epoch, which, I suppose, is unique in the world. And this was prepared for examination with incomparable patience either by his own hands or under his own eyes. He illustrated it with absolute fidelity. And if he did not in describing it always use language with which we could agree, nothing could ruffle either his imperturbable good nature or the noble simplicity of his character. Truth to tell, we were often in friendly warfare with him. But I rejoice to think that before his peaceful end came he had patiently reconsidered and abandoned all that we regarded as his heresies, but which were, in truth, only the old manner of

looking at things. And I think that if anything could have contributed to make his departure happy, it was the conviction that the completion of his work and his scientific reputation would remain perfectly secure in the hands of Dr. Scott.

## VEGETABLE PHYSIOLOGY.

Turning again to the present, the difficulty is to limit the choice of topics on which I would willingly dwell. In an address which I delivered at the Bath meeting in 1888 I ventured to point out the important part which the action of enzymes would be found to play in plant metabolism. My expectations have been more than realised by the admirable work of Professor Green on the one hand, and of Mr. Horace Brown on the other. The wildest imagination could not have foreseen the developments which in the hands of animal physiologists would spring from the study of the fermentative changes produced by yeast and bacteria. These, it seems to me, bid fair to revolutionise our whole conceptions of disease. The reciprocal action of ferments, developed in so admirable a manner by Marshall Ward in the case of the ginger-beer plant, is destined, I am convinced, to an expansion scarcely less important.

But, perhaps, the most noteworthy feature in recent work is the disposition to reopen in every direction fundamental questions. And here, I think, we may take a useful lesson from the practice of the older Sections, and adopt the plan of entrusting the investigation of special problems to small committees, or to individuals who are willing to undertake the labour of reporting upon special questions which they have made peculiarly their own. These reports would be printed *in extenso*, and are capable of rendering invaluable service by making accessible acquired knowledge which could not be got at in any other way.

We owe to Mr. Blackman a masterly demonstration of the fact, long believed, but never, perhaps, properly proved, that the surface of plants is ordinarily impermeable to gases. Mr. Dixon has brought forward some new views about water-movement in plants, which I confess I found less instructive than many of my brother botanists. They are expressed in language of extreme technicality; but, as far as I understand them, they amount to this. The water moving in the plant is contained in capillary channels; as it evaporates at the surface of the leaves a tensile strain is set up, as long as the columns are not broken, to restore the original level. I can understand that in this way the 'transpiration current' may be maintained. But what I want to know is how this explains the phenomena in the sugar maple, a single tree of which will yield, I believe, 20–30 gallons of fluid before a single leaf is expanded.

We owe to Messrs. Darwin and Acton the supply of a 'Manual of Practical Vegetable Physiology,' the want of which has long been keenly felt. Like the father of one of the authors, 'I love to exalt plants' (i 98). I have long been satisfied that the facts of vegetable physiology are capable of being widely taught, and are not less significant and infinitely more convenient than most of those which can be easily demonstrated on the animal side. How little any accurate knowledge of the subject has extended was conspicuously demonstrated in a recent discussion at the Royal Society, when two of our foremost chemists roundly denied the existence of a function of respiration in plants, because it was unknown to Liebig!

## ASSIMILATION.

The greatest and most fundamental problem of all is that of assimilation. The very existence of life upon the earth ultimately depends upon it. The veil is slowly, but I think surely, being lifted from its secrets. We now know that starch, if its first visible product, is not its first result. We are pretty well agreed that this is what I have called a 'proto-carbohydrate.' How is the synthesis of this effected? Mr. Acton, whose untimely end we cannot but deeply deplore, made some remarkable researches, which were communicated to the Royal Society in 1889, on the extent to which plants could take advantage of organic compounds made, so to speak, ready to their hand. Loew, in a remarkable paper, which will perhaps attract less attention than it deserves from being published in Japan,[1] has,

[1] *Bull College of Agric Imp. Univ Tokio*, vol. ii.

from the study of the nutrition of bacteria, arrived at some general conclusions in the same direction. Bokorny appears recently to have similarly experimented on algæ. Neither writer, however, seems to have been acquainted with Acton's work. The general conclusion which I draw from Loew is to strengthen the belief that form-aldehyde is actually one of the first steps of organic synthesis, as long ago suggested by Adolph Baeyer. Plants, then, will avail themselves of ready-made organic compounds which will yield them this body. That a sugar can be constructed from it has long been known, and Bokorny has shown that this can be utilised by plants in the production of starch.

The precise mode of the formation of form-aldehyde in the process of assimilation is a matter of dispute. But it is quite clear that either the carbon dioxide or the water, which are the materials from which it is formed, must suffer dissociation. And this requires a supply of energy to accomplish it. Warington has drawn attention to the striking fact that in the case of the nitrifying bacterium, assimilation may go on without the intervention of chlorophyll, the energy being supplied by the oxidation of ammonia. This brings us down to the fact, which has long been suspected, that protoplasm is at the bottom of the whole business, and that chlorophyll only plays some subsidiary and indirect part, perhaps, as Adolph Baeyer long ago suggested, of temporarily fixing carbon oxide like hæmoglobin, and so facilitating the dissociation.

Chlorophyll itself is still the subject of the careful study by Dr. Schunck, originally commenced by him some years ago at Kew. This will, I hope, give us eventually an accurate insight into the chemical constitution of this important substance.

The steps in plant metabolism which follow the synthesis of the proto-carbohydrate are still obscure. Brown and Morris have arrived at the unexpected conclusion that 'cane-sugar is the first sugar to be synthesised by the assimilatory processes.' I made some remarks upon this at the time,[1] which I may be permitted to reproduce here

'The point of view arrived at by botanists was briefly stated by Sachs in the case of the sugar-beet, starch in the leaf, glucose in the petiole, cane-sugar in the root. The facts in the sugar-cane seem to be strictly comparable.[2] Cane-sugar the botanist looks on, therefore, as a "reserve material." We may call "glucose" the sugar "currency" of the plant, cane-sugar its "banking reserve."

'The immediate result of the diastatic transformation of starch is not glucose, but maltose. But Mr. Horace Brown has shown in his remarkable experiments on feeding barley embryos that, while they can readily convert maltose into cane-sugar, they altogether fail to do this with glucose. We may conclude, therefore, that glucose is, from the point of view of vegetable nutrition, a somewhat inert body. On the other hand, evidence is apparently wanting that maltose plays the part in vegetable metabolism that might be expected of it. Its conversion into glucose may be perhaps accounted for by the constant presence in plant tissues of vegetable acids. But, so far, the change would seem to be positively disadvantageous. Perhaps glucose, in the botanical sense, will prove to have a not very exact chemical connotation.

'That the connection between cane-sugar and starch is intimate is a conclusion to which both the chemical and the botanical evidence seems to point. And on botanical grounds this would seem to be equally true of its connection with cellulose.

'It must be confessed that the conclusion that "cane-sugar" is the first sugar to be synthesised by the assimilatory processes seems hard to reconcile with its probable high chemical complexity, and with the fact that, botanically, it seems to stand at the end and not at the beginning of the series of metabolic change.'

### PROTOPLASMIC CHEMISTRY.

The synthesis of proteids is the problem which is second only in importance to that of carbohydrates. Loew's views of this deserve attentive study. Asparagin, as has long been suspected, plays an important part. It has, he says, two sources

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[1] *Journ. Chem. Soc.*, 1893, 673.    [2] *Kew Bulletin*, 1891, 35–41.

in the plant. ' It may either be formed directly from glucose, ammonia (or nitrates) and sulphates, or it may be a transitory product between protein-decomposition and reconstruction from the fragments.' [1]

In the remarks I made to the Chemical Society I ventured to express my conviction that the chemical processes which took place under the influence of protoplasm were probably of a different kind from those with which the chemist is ordinarily occupied. The plant produces a profusion of substances, apparently with great facility, which the chemist can only build up in the most circuitous way. As Victor Meyer [2] has remarked : ' In order to isolate an organic substance we are generally confined to the purely accidental properties of crystallisation and volatilisation.' In other words, the chemist only deals with bodies of great molecular stability ; while it cannot be doubted that those which play a part in the processes of life are the very opposite in every respect. I am convinced that if the chemist is to help in the field of protoplasmic activity he will have to transcend his present limitations, and be prepared to admit that as there may be more than one algebra, there may be more than one chemistry. I am glad to see that a somewhat similar idea has been suggested by other fields of inquiry. Professor Meldola [3] thinks that the investigation of photochemical processes ' may lead to the recognition of a new order of chemical attraction, or of the old chemical attraction in a different degree.' I am delighted to see that the ideas which were floating, I confess, in a very nebulous form in my brain are being clothed with greater precision by Loew.

In the paper which I have already quoted, he says of proteids : [4] ' They are *exceedingly labil compounds* that can be easily converted into relatively stable ones. A great lability is the indispensable and necessary foundation for the production of the various actions of the living protoplasm, for the mode of motions that move the life-machinery. There is a *source of motion* in the labil position of atoms in molecules, a source that has hitherto not been taken into consideration either by chemists or by physicists.'

But I must say no more. The problems to which I might invite attention on an occasion like this are endless I have not even attempted to do justice to the work that has been accomplished amongst ourselves, full of interest and novelty as it is But I will venture to say this, that if capacity and earnestness afford an augury of success, the prospects of the future of our Section possess every element of promise.

[1] *Loc. cit.*, 64        [2] *Pharm. Journ.*, 1890, 773.
[3] *Nature*, xlii. 250.       *Loc. cit*, 13.

# British Association for the Advancement of Science.

## LIVERPOOL, 1896

# ADDRESS

BY

## SIR JOSEPH LISTER, Bart., D.C.L., LL.D., P.R.S.,

### PRESIDENT.

My Lord Mayor, my Lords, Ladies, and Gentlemen, I have first to express my deep sense of gratitude for the great honour conferred upon me by my election to the high office which I occupy to-day. It came upon me as a great surprise. The engrossing claims of surgery have prevented me for many years from attending the meetings of the Association, which excludes from her sections medicine in all its branches. This severance of the art of healing from the work of the Association was right and indeed inevitable. Not that medicine has little in common with science. The surgeon never performs an operation without the aid of anatomy and physiology; and in what is often the most difficult part of his duty, the selection of the right course to follow, he, like the physician, is guided by pathology, the science of the nature of disease, which, though very difficult from the complexity of its subject matter, has made during the last half-century astonishing progress; so that the practice of medicine in every department is becoming more and more based on science as distinguished from empiricism. I propose on the present occasion to bring before you some illustrations of the interdependence of science and the healing art; and the first that I will take is perhaps the most astonishing of all results of purely physical inquiry—the discovery of the Röntgen rays, so called after the man who first clearly revealed them to the world. Mysterious as they still are, there is one of their properties which we can all appreciate—their power of passing through substances opaque to ordinary light. There seems to be no relation whatever between transparency in the common sense of

the term and penetrability to these emanations. The glasses of a pair of spectacles may arrest them while their wooden and leathern case allows them to pass almost unchecked. Yet they produce, whether directly or indirectly, the same effects as light upon a photographic plate. As a general rule the denser any object is the greater obstacle does it oppose to the rays. Hence, as bone is denser than flesh, if the hand or other part of the body is placed above the sensitive film enclosed in a case of wood or other light material at a suitable distance from the source of the rays, while they pass with the utmost facility through the uncovered parts of the lid of the box and powerfully affect the plate beneath, they are arrested to a large extent by the bones, so that the plate is little acted upon in the parts opposite to them, while the portions corresponding to the muscles and other soft parts are influenced in an intermediate degree Thus a picture is obtained in which the bones stand out in sharp relief among the flesh, and anything abnormal in their shape or position is clearly displayed.

I need hardly point out what important aid this must give to the surgeon. As an instance, I may mention a case which occurred in the practice of Mr. Howard Marsh. He was called to see a severe injury of the elbow, in which the swelling was so great as to make it impossible for him by ordinary means of examination to decide whether he had to deal with a fracture or a dislocation. If it were the latter, a cure would be effected by the exercise of violence which would be not only useless but most injurious if a bone was broken By the aid of the Röntgen rays a photograph was taken in which the bone of the upper arm was clearly seen displaced forwards on those of the forearm. The diagnosis being thus established, Mr. Marsh proceeded to reduce the dislocation ; and his success was proved by another photograph which showed the bones in their natural relative position.

The common metals, such as lead, iron, and copper, being still denser than the osseous structures, these rays can show a bullet embedded in a bone or a needle lodged about a joint. At the last conversazione of the Royal Society a picture produced by the new photography displayed with perfect distinctness through the bony framework of the chest a half-penny low down in a boy's gullet. It had been there for six months, causing uneasiness at the pit of the stomach during swallowing ; but whether the coin really remained impacted, or if so, what was its position, was entirely uncertain till the Röntgen rays revealed it. Dr. Macintyre of Glasgow, who was the photographer, informs me that when the presence of the halfpenny had been thus demonstrated, the surgeon in charge of the case made an attempt to extract it, and although this was not successful in its immediate object, it had the effect of dislodging the coin ; for a subsequent photograph by Dr. Macintyre not only showed that it had disappeared from the gullet, but also, thanks to the wonderful penetrating power which the rays had acquired in his hands, proved that it had not

lodged further down in the alimentary passage. The boy has since completely recovered.

The Röntgen rays cause certain chemical compounds to fluoresce, and emit a faint light plainly visible in the dark ; and if they are made to fall upon a translucent screen impregnated with such a salt, it becomes beautifully illuminated. If a part of the human body is interposed between the screen and the source of the rays, the bones and other structures are thrown in shadow upon it, and thus a diagnosis can be made without the delay involved in taking a photograph. It was in fact in this way that Dr. Macintyre first detected the coin in the boy's gullet. Mr. Herbert Jackson, of King's College, London, early distinguished himself in this branch of the subject. There is no reason to suppose that the limits of the capabilities of the rays in this way have yet been reached. By virtue of the greater density of the heart than the adjacent lungs with their contained air, the form and dimensions of that organ in the living body may be displayed on the fluorescent screen, and even its movements have been lately seen by several different observers.

Such important applications of the new rays to medical practice have strongly attracted the interest of the public to them, and I venture to think that they have even served to stimulate the investigations of physicists. The eminent Professor of Physics in the University College of this city (Professor Lodge) was one of the first to make such practical applications, and I was able to show to the Royal Society at a very early period a photograph, which he had the kindness to send me, of a bullet embedded in the hand. His interest in the medical aspect of the subject remains unabated, and at the same time he has been one of the most distinguished investigators of its purely physical side.

There is another way in which the Röntgen rays connect themselves with physiology, and may possibly influence medicine. It is found that if the skin is long exposed to their action it becomes very much irritated, affected with a sort of aggravated sun-burning. This suggests the idea that the transmission of the rays through the human body may be not altogether a matter of indifference to internal organs, but may, by long-continued action, produce, according to the condition of the part concerned, injurious irritation or salutary stimulation.

This is the jubilee of Anæsthesia in surgery. That priceless blessing to mankind came from America. It had, indeed, been foreshadowed in the first year of this century by Sir Humphry Davy, who, having found a toothache from which he was suffering relieved as he inhaled laughing gas (nitrous oxide), threw out the suggestion that it might perhaps be used for preventing pain in surgical operations But it was not till, on September 30, 1846, Dr. W. T. G. Morton, of Boston, after a series of experiments upon himself and the lower animals, extracted a tooth painlessly from a patient whom he had caused to inhale the vapour of sulphuric ether, that the idea was fully realised. He soon afterwards publicly

exhibited his method at the Massachusetts General Hospital, and after that event the great discovery spread rapidly over the civilised world.    I witnessed the first operation in England under ether.    It was performed by Robert Liston in University College Hospital, and it was a complete success. Soon afterwards I saw the same great surgeon amputate the thigh as painlessly, with less complicated anæsthetic apparatus, by aid of another agent, chloroform, which was being powerfully advocated as a substitute for ether by Dr. (afterwards Sir James Y.) Simpson, who also had the great merit of showing that confinements could be conducted painlessly, yet safely, under its influence.    These two agents still hold the field as general anæsthetics for protracted operations, although the gas originally suggested by Davy, in consequence of its rapid action and other advantages, has taken their place in short operations, such as tooth extraction. In the birthplace of anæsthesia ether has always maintained its ground , but in Europe it was to a large extent displaced by chloroform till recently, when many have returned to ether, under the idea that, though less convenient, it is safer.    For my own part, I believe that chloroform, if carefully administered on right principles, is, on the average, the safer agent of the two.

The discovery of anæsthesia inaugurated a new era in surgery.    Not only was the pain of operations abolished, but the serious and sometimes mortal shock which they occasioned to the system was averted, while the patient was saved the terrible ordeal of preparing to endure them.    At the same time the field of surgery became widely extended, since many procedures in themselves desirable, but before impossible from the protracted agony they would occasion, became matters of routine practice. Nor have I by any means exhausted the list of the benefits conferred by this discovery.

Anæsthesia in surgery has been from first to last a gift of science. Nitrous oxide, sulphuric ether, and chloroform are all artificial products of chemistry, their employment as anæsthetics was the result of scientific investigation, and their administration, far from being, like the giving of a dose of medicine, a matter of rule of thumb, imperatively demands the vigilant exercise of physiological and pathological knowledge.

While rendering such signal service to surgery, anæsthetics have thrown light upon biology generally.    It has been found that they exert their soporific influence not only upon vertebrata, but upon animals so remote in structure from man as bees and other insects.    Even the functions of vegetables are suspended by their agency.    They thus afford strong confirmation of the great generalisation that living matter is of the same essential nature wherever it is met with on this planet, whether in the animal or vegetable kingdom.    Anæsthetics have also, in ways to which I need not here refer, powerfully promoted the progress of physiology and pathology.

My next illustration may be taken from the work of Pasteur on fer-

mentation. The prevailing opinion regarding this class of phenomena when they first engaged his attention was that they were occasioned primarily by the oxygen of the air acting upon unstable animal or vegetable products, which, breaking up under its influence, communicated disturbance to other organic materials in their vicinity, and thus led to their decomposition. Cagniard-Latour had indeed shown several years before that yeast consists essentially of the cells of a microscopic fungus which grows as the sweetwort ferments ; and he had attributed the breaking up of the sugar into alcohol and carbonic acid to the growth of the micro-organism. In Germany Schwann, who independently discovered the yeast plant, had published very striking experiments in support of analogous ideas regarding the putrefaction of meat. Such views had also found other advocates, but they had become utterly discredited, largely through the great authority of Liebig, who bitterly opposed them.

Pasteur, having been appointed as a young man Dean of the Faculty of Sciences in the University of Lille, a town where the products of alcoholic fermentation were staple articles of manufacture, determined to study that process thoroughly , and as a result he became firmly convinced of the correctness of Cagniard-Latour's views regarding it. In the case of other fermentations, however, nothing fairly comparable to the formation of yeast had till then been observed. This was now done by Pasteur for that fermentation in which sugar is resolved into lactic acid This lactic fermentation was at that time brought about by adding some animal substance, such as fibrin, to a solution of sugar, together with chalk that should combine with the acid as it was formed. Pasteur saw, what had never before been noticed, that a fine grey deposit was formed, differing little in appearance from the decomposing fibrin, but steadily increasing as the fermentation proceeded Struck by the analogy presented by the increasing deposit to the growth of yeast in sweetwort, he examined it with the microscope, and found it to consist of minute particles of uniform size. Pasteur was not a biologist, but although these particles were of extreme minuteness in comparison with the constituents of the yeast plant, he felt convinced that they were of an analogous nature, the cells of a tiny microscopic fungus. This he regarded as the essential ferment, the fibrin or other so-called ferment serving, as he believed, merely the purpose of supplying to the growing plant certain chemical ingredients not contained in the sugar but essential to its nutrition. And the correctness of this view he confirmed in a very striking manner, by doing away with the fibrin or other animal material altogether, and substituting for it mineral salts containing the requisite chemical elements. A trace of the grey deposit being applied to a solution of sugar containing these salts in addition to the chalk, a brisker lactic fermentation ensued than could be procured in the ordinary way

I have referred to this research in some detail because it illustrates

Pasteur's acuteness as an observer and his ingenuity in experiment, as well as his almost intuitive perception of truth.

A series of other beautiful investigations followed, clearly proving that all true fermentations, including putrefaction, are caused by the growth of micro-organisms.

It was natural that Pasteur should desire to know how the microbes which he showed to be the essential causes of the various fermentations took their origin. It was at that period a prevalent notion, even among many eminent naturalists, that such humble and minute beings originated *de novo* in decomposing organic substances; the doctrine of spontaneous generation, which had been chased successively from various positions which it once occupied among creatures visible to the naked eye, having taken its last refuge where the objects of study were of such minuteness that their habits and history were correspondingly difficult to trace. Here again Pasteur at once saw, as if by instinct, on which side the truth lay; and, perceiving its immense importance, he threw himself with ardour into its demonstration. I may describe briefly one class of experiments which he performed with this object. He charged a series of narrow-necked glass flasks with a decoction of yeast, a liquid peculiarly liable to alteration on exposure to the air. Having boiled the liquid in each flask, to kill any living germs it might contain, he sealed its neck with a blow-pipe during ebullition; after which, the flask being allowed to cool, the steam within it condensed, leaving a vacuum above the liquid. If, then, the neck of the flask were broken in any locality, the air at that particular place would rush in to fill the vacuum, carrying with it any living microbes that might be floating in it. The neck of the flask having been again sealed, any germs so introduced would in due time manifest their presence by developing in the clear liquid. When any of such a series of flasks were opened and re-sealed in an inhabited room, or under the trees of a forest, multitudes of minute living forms made their appearance in them; but if this was done in a cellar long unused, where the suspended organisms, like other dust, might be expected to have all fallen to the ground, the decoction remained perfectly clear and unaltered. The oxygen and other gaseous constituents of the atmosphere were thus shown to be of themselves incapable of inducing any organic development in yeast-water.

Such is a sample of the many well-devised experiments by which he carried to most minds the conviction that, as he expressed it, '*la génération spontanée est une chimère*,' and that the humblest and minutest living organisms can only originate by parentage from beings like themselves.

Pasteur pointed out the enormous importance of these humble organisms in the economy of nature. It is by their agency that the dead bodies of plants and animals are resolved into simpler compounds fitted for assimilation by new living forms. Without their aid the world would be, as Pasteur expresses it, *encombré de cadavres*. They are essential not only to our well-being, but to our very existence. Similar microbes must

have discharged the same necessary function of removing refuse and providing food for successive generations of plants and animals during the past periods of the world's history , and it is interesting to think that organisms as simple as can well be conceived to have existed when life first appeared upon our globe have, in all probability, propagated the same lowly but most useful offspring during the ages of geological time

Pasteur's labours on fermentation have had a very important influence upon surgery. I have been often asked to speak on my share in this matter before a public audience ; but I have hitherto refused to do so, partly because the details are so entirely technical, but chiefly because I have felt an invincible repugnance to what might seem to savour of self-advertisement. The latter objection now no longer exists, since advancing years have indicated that it is right for me to leave to younger men the practice of my dearly loved profession. And it will perhaps be expected that, if I can make myself intelligible, I should say something upon the subject on the present occasion.

Nothing was formerly more striking in surgical experience than the difference in the behaviour of injuries according to whether the skin was implicated or not. Thus, if the bones of the leg were broken and the skin remained intact, the surgeon applied the necessary apparatus without any other anxiety than that of maintaining a good position of the fragments, although the internal injury to bones and soft parts might be very severe. If, on the other hand, a wound of the skin was present communicating with the broken bones, although the damage might be in other respects comparatively slight, the compound fracture, as it was termed, was one of the most dangerous accidents that could happen. Mr. Syme, who was, I believe, the safest surgeon of his time, once told me that he was inclined to think that it would be, on the whole, better if all compound fractures of the leg were subjected to amputation, without any attempt to save the limb. What was the cause of this astonishing difference ? It was clearly in some way due to the exposure of the injured parts to the external world. One obvious effect of such exposure was indicated by the odour of the discharge, which showed that the blood in the wound had undergone putrefactive change by which the bland nutrient liquid had been converted into highly irritating and poisonous substances. I have seen a man with compound fracture of the leg die within two days of the accident, as plainly poisoned by the products of putrefaction as if he had taken a fatal dose of some potent toxic drug.

An external wound of the soft parts might be healed in one of two ways. If its surfaces were clean cut and could be brought into accurate apposition, it might unite rapidly and painlessly ' by the first intention.' This, however, was exceptional. Too often the surgeon's efforts to obtain primary union were frustrated : the wound inflamed and the retentive stitches had to be removed, allowing it to gape , and then, as if it had been left open from the first, healing had to be effected in the other way

which it is necessary for me briefly to describe.   An exposed raw surface became covered in the first instance with a layer of clotted blood or certain of its constituents, which invariably putrefied ; and the irritation of the sensitive tissues by the putrid products appeared to me to account sufficiently for the inflammation which always occurred in and around an open wound during the three or four days which elapsed before what were termed ' granulations ' had been produced.   These constituted a coarsely granular coating of very imperfect or embryonic structure, destitute of sensory nerves and prone to throw off matter or pus, rather than absorb, as freshly divided tissues do, the products of putrefaction.   The granulations thus formed a beautiful living plaster, which protected the sensitive parts beneath from irritation, and the system generally from poisoning and consequent febrile disturbance.   The granulations had other useful properties of which I may mention their tendency to shrink as they grew, thus gradually reducing the dimensions of the sore.   Meanwhile, another cause of its diminution was in operation.   The cells of the epidermis or scarf-skin of the cutaneous margins were perpetually producing a crop of young cells of similar nature, which gradually spread over the granulations till they covered them entirely, and a complete cicatrix or scar was the result.   Such was the other mode of healing, that by granulation and cicatrisation ; a process which, when it proceeded unchecked to its completion, commanded our profound admiration.   It was, however, essentially tedious compared with primary union, while, as we have seen, it was always preceded by more or less inflammation and fever, sometimes very serious in their effects.   It was also liable to unforeseen interruptions. The sore might become larger instead of smaller, cicatrisation giving place to ulceration in one of its various forms, or even to the frightful destruction of tissue which, from the circumstance that it was most frequently met with in hospitals, was termed hospital gangrene.   Other serious and often fatal complications might arise, which the surgeon could only regard as untoward accidents and over which he had no efficient control.

It will be readily understood from the above description that the inflammation which so often frustrated the surgeon's endeavours after primary union was in my opinion essentially due to decomposition of blood within the wound.

These and many other considerations had long impressed me with the greatness of the evil of putrefaction in surgery.   I had done my best to mitigate it by scrupulous ordinary cleanliness and the use of various deodorant lotions.   But to prevent it altogether appeared hopeless while we believed with Liebig that its primary cause was the atmospheric oxygen which, in accordance with the researches of Graham, could not fail to be perpetually diffused through the porous dressings which were used to absorb the blood discharged from the wound.   But when Pasteur had shown that putrefaction was a fermentation caused by the growth of microbes, and that these could not arise *de novo* in the

decomposable substance, the problem assumed a more hopeful aspect. If the wound could be treated with some substance which, without doing too serious mischief to the human tissues, would kill the microbes already contained in it and prevent the future access of others in the living state, putrefaction might be prevented, however freely the air with its oxygen might enter. I had heard of carbolic acid as having a remarkable deodorising effect upon sewage, and having obtained from my colleague Dr Anderson, Professor of Chemistry in the University of Glasgow, a sample which he had of this product, then little more than a chemical curiosity in Scotland, I determined to try it in compound fractures. Applying it undiluted to the wound, with an arrangement for its occasional renewal, I had the joy of seeing these formidable injuries follow the same safe and tranquil course as simple fractures, in which the skin remains unbroken.

At the same time we had the intense interest of observing in open wounds what had previously been hidden from human view, the manner in which subcutaneous injuries are repaired. Of special interest was the process by which portions of tissue killed by the violence of the accident were disposed of, as contrasted with what had till then been invariably witnessed. Dead parts had been always seen to be gradually separated from the living by an inflammatory process and thrown off as sloughs. But when protected by the antiseptic dressing from becoming putrid and therefore irritating, a structure deprived of its life caused no disturbance in its vicinity ; and, on the contrary, being of a nutritious nature, it served as pabulum for the growing elements of the neighbouring living structures, and these became in due time entirely substituted for it. Even dead bone was seen to be thus replaced by living osseous tissue.

This suggested the idea of using threads of dead animal tissue for tying blood-vessels ; and this was realised by means of catgut, which is made from the intestine of the sheep. If deprived of living microbes, and otherwise properly prepared, catgut answers its purpose completely ; the knot holding securely, while the ligature around the vessel becomes gradually absorbed and replaced by a ring of living tissue. The threads, instead of being left long as before, could now be cut short, and the tedious process of separation of the ligature, with its attendant serious danger of bleeding, was avoided.

Undiluted carbolic acid is a powerful caustic ; and although it might be employed in compound fracture, where some loss of tissue was of little moment in comparison with the tremendous danger to be averted, it was altogether unsuitable for wounds made by the surgeon. It soon appeared, however, that the acid would answer the purpose aimed at, though used in diluted forms devoid of caustic action, and therefore applicable to operative surgery. According to our then existing knowledge, two essential points had to be aimed at : to conduct the operation so that on its completion the wound should contain no living microbes, and to apply a

dressing capable of preventing the access of other living organisms till the time should have arrived for changing it.

Carbolic acid lent itself well to both these objects. Our experience with this agent brought out what was, I believe, a new principle in pharmacology—namely, that the energy of action of any substance upon the human tissues depends not only upon the proportion in which it is contained in the material used as a vehicle for its administration, but also upon the degree of tenacity with which it is held by its solvent. Water dissolves carbolic acid sparingly and holds it extremely lightly, leaving it free to act energetically on other things for which it has greater affinity, while various organic substances absorb it greedily and hold it tenaciously. Hence its watery solution seemed admirably suited for a detergent lotion to be used during the operation for destroying any microbes that might fall upon the wound, and for purifying the surrounding skin and also the surgeon's hands and instruments. For the last-named purpose it had the further advantage that it did not act on steel.

For an external dressing the watery solution was not adapted, as it soon lost the acid it contained, and was irritating while it lasted. For this purpose some organic substances were found to answer well. Large proportions of the acid could be blended with them in so bland a form as to be unirritating ; and such mixtures, while perpetually giving off enough of the volatile salt to prevent organic development in the discharges that flowed past them, served as a reliable store of the antiseptic for days together.

The appliances which I first used for carrying out the antiseptic principle were both rude and needlessly complicated. The years that have since passed have witnessed great improvements in both respects. Of the various materials which have been employed by myself and others, and their modes of application, I need say nothing except to express my belief, as a matter of long experience, that carbolic acid, by virtue of its powerful affinity for the epidermis and oily matters associated with it, and also its great penetrating power, is still the best agent at our disposal for purifying the skin around the wound. But I must say a few words regarding a most important simplification of our procedure. Pasteur, as we have seen, had shown that the air of every inhabited room teems with microbes ; and for a long time I employed various more or less elaborate precautions against the living atmospheric dust, not doubting that, as all wounds except the few which healed completely by the first intention underwent putrefactive fermentation, the blood must be a peculiarly favourable soil for the growth of putrefactive microbes. But I afterwards learnt that such was by no means the case. I had performed many experiments in confirmation of Pasteur's germ theory, not indeed in order to satisfy myself of its truth, but in the hope of convincing others. I had observed that uncontaminated milk, which would remain unaltered for an indefinite time if protected from dust,

was made to teem with microbes of different kinds by a very brief exposure to the atmosphere, and that the same effect was produced by the addition of a drop of ordinary water. But when I came to experiment with blood drawn with antiseptic precautions into sterilised vessels, I saw to my surprise that it might remain free from microbes in spite of similar access of air or treatment with water. I even found that if very putrid blood was largely diluted with sterilised water, so as to diffuse its microbes widely and wash them of their acrid products, a drop of such dilution added to pure blood might leave it unchanged for days at the temperature of the body, although a trace of the septic liquid undiluted caused intense putrefaction within twenty-four hours. Hence I was led to conclude that it was the grosser forms of septic mischief, rather than microbes in the attenuated condition in which they existed in the atmosphere, that we had to dread in surgical practice. And at the London Medical Congress in 1881, I hinted, when describing the experiments I have alluded to, that it might turn out possible to disregard altogether the atmospheric dust. But greatly as I should have rejoiced at such a simplification of our procedure, if justifiable, I did not then venture to test it in practice. I knew that with the safeguards which we then employed I could ensure the safety of my patients, and I did not dare to imperil it by relaxing them. There is one golden rule for all experiments upon our fellow-men. Let the thing tried be that which, according to our best judgment, is the most likely to promote the welfare of the patient. In other words, Do as you would be done by

Nine years later, however, at the Berlin Congress in 1890, I was able to bring forward what was, I believe, absolute demonstration of the harmlessness of the atmospheric dust in surgical operations. This conclusion has been justified by subsequent experience : the irritation of the wound by antiseptic irrigation and washing may therefore now be avoided, and nature left quite undisturbed to carry out her best methods of repair, while the surgeon may conduct his operations as simply as in former days, provided always that, deeply impressed with the tremendous importance of his object, and inspiring the same conviction in all his assistants, he vigilantly maintains from first to last, with a care that, once learnt, becomes instinctive, but for the want of which nothing else can compensate, the use of the simple means which will suffice to exclude from the wound the coarser forms of septic impurity.

Even our earlier and ruder methods of carrying out the antiseptic principle soon produced a wonderful change in my surgical wards in the Glasgow Royal Infirmary, which, from being some of the most unhealthy in the kingdom, became, as I believe I may say without exaggeration, the healthiest in the world ; while other wards, separated from mine only by a passage a few feet broad, where former modes of treatment were for a while continued, retained their former insalubrity. This result, I need hardly remark, was not in any degree due to special skill on my part, but simply

to the strenuous endeavour to carry out strictly what seemed to me a prin-ciple of supreme importance.

Equally striking changes were afterwards witnessed in other institu-tions. Of these I may give one example. In the great Allgemeines Krankenhaus of Munich, hospital gangrene had become more and more rife from year to year, till at length the frightful condition was reached that 80 per cent. of all wounds became affected by it. It is only just to the memory of Professor von Nussbaum, then the head of that establish-ment, to say that he had done his utmost to check this frightful scourge ; and that the evil was not caused by anything peculiar in his management was shown by the fact that in a private hospital under his care there was no unusual unhealthiness. The larger institution seemed to have become hopelessly infected, and the city authorities were contemplating its demo-lition and reconstruction. Under these circumstances, Professor von Nussbaum despatched his chief assistant, Dr. Lindpaintner, to Edinburgh, where I at that time occupied the chair of clinical surgery, to learn the details of the antiseptic system as we then practised it. He remained until he had entirely mastered them, and after his return all the cases were on a certain day dressed on our plan. From that day forward not a single case of hospital gangrene occurred in the Krankenhaus. The fearful disease pyæmia likewise disappeared, and erysipelas soon followed its example.

But it was by no means only in removing the unhealthiness of hos-pitals that the antiseptic system showed its benefits. Inflammation being suppressed, with attendant pain, fever, and wasting discharge, the suffer-ings of the patient were, of course, immensely lessened ; rapid primary union being now the rule, convalescence was correspondingly curtailed ; while as regards safety and the essential nature of the mode of repair, it became a matter of indifference whether the wound had clean-cut surfaces which could be closely approximated, or whether the injury inflicted had been such as to cause destruction of tissue. And operations which had been regarded from time immemorial as unjustifiable were adopted with complete safety.

It pleases me to think that there is an ever-increasing number of prac-titioners throughout the world to whom this will not appear the language of exaggeration. There are cases in which, from the situation of the part concerned or other unusual circumstances, it is impossible to carry out the antiseptic system completely. These, however, are quite exceptional; and even in them much has been done to mitigate the evil which cannot be altogether avoided.

I ask your indulgence if I have seemed to dwell too long upon matters in which I have been personally concerned. I now gladly return to the labours of others.

The striking results of the application of the germ theory to Surgery acted as a powerful stimulus to the investigation of the nature of the

micro-organisms concerned; and it soon appeared that putrefaction was by no means the only evil of microbic origin to which wounds were liable. I had myself very early noticed that hospital gangrene was not necessarily attended by any unpleasant odour; and I afterwards made a similar observation regarding the matter formed in a remarkable epidemic of erysipelas in Edinburgh obviously of infective character. I had also seen a careless dressing followed by the occurrence of suppuration without putrefaction. And as these non-putrefactive disorders had the same self-propagating property as ferments, and were suppressed by the same antiseptic agencies which were used for combating the putrefactive microbes, I did not doubt that they were of an analogous origin; and I ventured to express the view that, just as the various fermentations had each its special microbe, so it might be with the various complications of wounds. This surmise was afterwards amply verified. Professor Ogston, of Aberdeen, was an early worker in this field, and showed that in acute abscesses, that is to say those which run a rapid course, the matter, although often quite free from unpleasant odour, invariably contains micro-organisms belonging to the group which, from the spherical form of their elements, are termed micrococci; and these he classed as streptococci or staphylococci, according as they were arranged in chains or disposed in irregular clusters like bunches of grapes. The German pathologist, Fehleisen, followed with a beautiful research, by which he clearly proved that erysipelas is caused by a streptococcus. A host of earnest workers in different countries have cultivated the new science of Bacteriology, and, while opening up a wide fresh domain of Biology, have demonstrated in so many cases the causal relation between special micro-organisms and special diseases, not only in wounds but in the system generally, as to afford ample confirmation of the induction which had been made by Pasteur that all infective disorders are of microbic origin.

Not that we can look forward with anything like confidence to being able ever to see the *materies morbi* of every disease of this nature. One of the latest of such discoveries has been that by Pfeiffer of Berlin of the bacillus of influenza, perhaps the most minute of all micro-organisms ever yet detected. The bacillus of anthrax, the cause of a plague common among cattle in some parts of Europe, and often communicated to sorters of foreign wool in this country, is a giant as compared with this tiny being; and supposing the microbe of any infectious fever to be as much smaller than the influenza bacillus as this is less than that of anthrax, a by no means unlikely hypothesis, it is probable that it would never be visible to man. The improvements of the microscope, based on the principle established by my father in the earlier part of the century, have apparently nearly reached the limits of what is possible. But that such parasites are really the causes of all this great class of diseases can no longer be doubted.

The first rational step towards the prevention or cure of disease is to

know its cause ; and it is impossible to over-estimate the practical value of researches such as those to which I am now referring. Among their many achievements is what may be fairly regarded as the most important discovery ever made in pathology, because it revealed the true nature of the disease which causes more sickness and death in the human race than any other. It was made by Robert Koch, who greatly distinguished himself, when a practitioner in an obscure town in Germany, by the remarkable combination of experimental acuteness and skill, chemical and optical knowledge and successful micro-photography which he brought to bear upon the illustration of infective diseases of wounds in the lower animals ; in recognition of which service the enlightened Prussian Government at once appointed him to an official position of great importance in Berlin. There he conducted various important researches ; and at the London Congress in 1881 he showed to us for the first time the bacillus of tubercle. Wonderful light was thrown by this discovery upon a great group of diseases which had before been rather guessed than known to be of an allied nature ; a precision and efficacy never before possible was introduced into their surgical treatment, while the physician became guided by new and sure light as regards their diagnosis and prevention.

At that same London Congress Koch demonstrated to us his ' plate culture' of bacteria, which was so important that I must devote a few words to its description. With a view to the successful study of the habits and effects of any particular microbe outside the living body, it is essential that it should be present unmixed in the medium in which it is cultivated. It can be readily understood how difficult it must have been to isolate any particular micro-organism when it existed mixed, as was often the case, with a multitude of other forms. In fact, the various ingenious attempts made to effect this object had often proved entire failures. Koch, however, by an ingenious procedure converted what had been before impossible into a matter of the utmost facility. In the broth or other nutrient liquid which was to serve as food for the growing microbe he dissolved, by aid of heat, just enough gelatine to ensure that, while it should become a solid mass when cold, it should remain fluid though reduced in temperature so much as to be incapable of killing living germs To the medium thus partially cooled was added some liquid containing, among others, the microbe to be investigated ; and the mixture was thoroughly shaken so as to diffuse the bacteria and separate them from each other. Some of the liquid was then poured out in a thin layer upon a glass plate and allowed to cool so as to assume the solid form. The various microbes, fixed in the gelatine and so prevented from intermingling, proceeded to develop each its special progeny, which in course of time showed itself as an opaque speck in the transparent film. Any one of such specks could now be removed and transferred to another vessel in which the microbe composing it grew in perfect isolation.

Pasteur was present at this demonstration, and expressed his sense of

the great progress effected by the new method. It was soon introduced into his own institute and other laboratories throughout the world ; and it has immensely facilitated bacteriological study.

One fruit of it in Koch's own hands was the discovery of the microbe of cholera in India, whither he went to study the disease. This organism was termed by Koch from its curved form the ' comma bacillus,' and by the French the cholera vibrio. Great doubts were for a long time felt regarding this discovery. Several other kinds of bacteria were found of the same shape, some of them producing very similar appearances in culture media. But bacteriologists are now universally agreed that, although various other conditions are necessary to the production of an attack of cholera besides the mere presence of the vibrio, yet it is the essential *materies morbi* ; and it is by the aid of the diagnosis which its presence in any case of true cholera enables the bacteriologist to make, that threatened invasions of this awful disease have of late years been so successfully repelled from our shores. If bacteriology had done nothing more for us than this, it might well have earned our gratitude.

I have next to invite your attention to some earlier work of Pasteur. There is a disease known in France under the name of *choléra des poules*, which often produced great havoc among the poultry yards of Paris. It had been observed that the blood of birds that had died of this disease was peopled by a multitude of minute bacteria, not very dissimilar in form and size to the microbe of the lactic ferment to which I have before referred. And Pasteur found that, if this bacterium was cultivated outside the body for a protracted period under certain conditions, it underwent a remarkable diminution of its virulence ; so that, if inoculated into a healthy fowl, it no longer caused the death of the bird, as it would have done in its original condition, but produced a milder form of the disease which was not fatal. And this altered character of the microbe, caused by certain conditions, was found to persist in successive generations cultivated in the ordinary way. Thus was discovered the great fact of what Pasteur termed the *atténuation des virus*, which at once gave the clue to understanding what had before been quite mysterious, the difference in virulence of the same disease in different epidemics.

But he made the further very important observation that a bird which had gone through the mild form of the complaint had acquired immunity against it in its most virulent condition. Pasteur afterwards succeeded in obtaining mitigated varieties of microbes for some other diseases ; and he applied with great success the principle which he had discovered in fowl-cholera for protecting the larger domestic animals against the plague of anthrax. The preparations used for such preventive inoculations he termed ' vaccins ' in honour of our great countryman, Edward Jenner. For Pasteur at once saw the analogy between the immunity to fowl-cholera produced by its attenuated virus and the protection afforded against small-pox by vaccination. And while pathologists still hesitated,

he had no doubt of the correctness of Jenner's expression *variolæ vaccinæ*, or small-pox in the cow.

It is just a hundred years since Jenner made the crucial experiment of inoculating with small-pox a boy whom he had previously vaccinated, the result being, as he anticipated, that the boy was quite unaffected. It may be remarked that this was a perfectly legitimate experiment, involving no danger to the subject of it. Inoculation was at that time the established practice ; and if vaccination should prove nugatory, the inoculation would be only what would have been otherwise called for ; while it would be perfectly harmless if the hoped-for effect of vaccination had been produced.

We are a practical people, not much addicted to personal commemorations : although our nation did indeed celebrate with fitting splendour the jubilee of the reign of our beloved Queen ; and at the invitation of Glasgow the scientific world has lately marked in a manner, though different, as imposing, the jubilee of the life-work of a sovereign in science (Lord Kelvin). But while we cannot be astonished that the centenary of Jenner's immortal discovery should have failed to receive general recognition in this country, it is melancholy to think that this year should, in his native county, have been distinguished by a terrible illustration of the results which would sooner or later inevitably follow the general neglect of his prescriptions.

I have no desire to speak severely of the Gloucester Guardians. They are not sanitary authorities, and had not the technical knowledge necessary to enable them to judge between the teachings of true science and the declamations of misguided, though well-meaning, enthusiasts. They did what they believed to be right ; and when roused to a sense of the greatness of their mistake, they did their very best to repair it, so that their city is said to be now the best vaccinated in Her Majesty's dominions. But though by their praiseworthy exertions they succeeded in promptly checking the raging epidemic, they cannot recall the dead to life, or restore beauty to marred features, or sight to blinded eyes. Would that the entire country and our Legislature might take duly to heart this object-lesson !

How completely the medical profession were convinced of the efficacy of vaccination in the early part of this century was strikingly illustrated by an account given by Professor Crookshank, in his interesting history of this subject, of several eminent medical men in Edinburgh meeting to see the to them unprecedented fact of a vaccinated person having taken small-pox. It has, of course, since become well known that the milder form of the disease, as modified by passing through the cow, confers a less permanent protection than the original human disorder. This it was, of course, impossible for Jenner to foresee. It is, indeed, a question of degree, since a second attack of ordinary small-pox is occasionally known to occur, and vaccination, long after it has ceased to give perfect immunity, greatly modifies the character of the disorder and diminishes its

danger. And, happily, in re-vaccination after a certain number of years we have the means of making Jenner's work complete I understand that the majority of the Commissioners, who have recently issued their report upon this subject, while recognising the value and importance of re-vaccination, are so impressed with the difficulties that would attend making it compulsory by legislation that they do not recommend that course ; although it is advocated by two of their number who are of peculiarly high authority on such a question. I was lately told by a Berlin professor that no serious difficulty is experienced in carrying out the compulsory law that prevails in Germany. The masters of the schools are directed to ascertain in the case of every child attaining the age of twelve whether re-vaccination has been practised. If not, and the parents refuse to have it done, they are fined one mark. If this does not prove effectual, the fine is doubled : and if even the double penalty should not prove efficacious, a second doubling of it would follow, but, as my informant remarked, it is very seldom that it is called for The result is that small-pox is a matter of extreme rarity in that country , while it is absolutely unknown in the huge German army, in consequence of the rule that every soldier is re-vaccinated on entering the service. Whatever view our Legislature may take on this question, one thing seems to me clear : that it will be the duty of Government to encourage by every available means the use of calf lymph, so as to exclude the possibility of the communication of any human disease to the child, and to institute such efficient inspection of vaccination institutes as shall ensure careful antiseptic arrangements, and so prevent contamination by extraneous microbes. If this were done, 'conscientious objections' would cease to have any rational basis. At the same time, the administration of the regulations on vaccination should be transferred (as advised by the Commissioners) to competent sanitary authorities.

But to return to Pasteur. In 1880 he entered upon the study of that terrible but then most obscure disease, Hydrophobia or Rabies, which from its infective character he was sure must be of microbic origin, although no micro-organism could be detected in it. He early demonstrated the new pathological fact that the virus had its essential seat in the nervous system. This proved the key to his success in this subject. One result that flowed from it has been the cause of unspeakable consolation to many. The foolish practice is still too prevalent of killing the dog that has bitten any one, on the absurd notion that, if it were mad, its destruction would prevent the occurrence of Hydrophobia in the person bitten. The idea of the bare possibility of the animal having been so affected causes an agony of suspense during the long weeks or months of possible incubation of the disease. Very serious nervous symptoms aping true Hydrophobia have been known to result from the terror thus inspired. Pasteur showed that if a little of the brain or spinal cord of a dog that had been really mad was inoculated in an appropriate manner into a rabbit, it

infallibly caused rabies in that animal in a few days. If therefore such
an experiment was made with a negative result, the conclusion might
be drawn with certainty that the dog had been healthy. It is perhaps
right that I should say that the inoculation is painlessly done under an
anæsthetic, and that in the rabbit rabies does not assume the violent form
that it does in the dog, but produces gradual loss of power with little if
any suffering.

This is the more satisfactory because rabbits in which the disease has
been thus artificially induced are employed in carrying out what was
Pasteur's greatest triumph, the preventive treatment of Hydrophobia in
the human subject. We have seen that Pasteur discovered that microbes
might under some circumstances undergo mitigation of their virulence.
He afterwards found that under different conditions they might have it
exalted, or, as he expressed it, there might be a *renforcement du virus*.
Such proved to be the case with rabies in the rabbit ; so that the spinal
cords of animals which had died of it contained the poison in a highly
intensified condition. But he also found that if such a highly virulent
cord was suspended under strict antiseptic precautions in a dry atmosphere
at a certain temperature, it gradually from day to day lost in potency, till
in course of time it became absolutely inert. If now an emulsion of such
a harmless cord was introduced under the skin of an animal, as in the
subcutaneous administration of morphia, it might be followed without harm
another day by a similar dose of a cord still rather poisonous ; and so from
day to day stronger and stronger injections might be used, the system
becoming gradually accustomed to the poison, till a degree of virulence
had been reached far exceeding that of the bite of a mad dog. When this
had been attained, the animal proved incapable of taking the disease in
the ordinary way ; and more than that, if such treatment was adopted
after an animal had already received the poison, provided that too long a
time had not elapsed, the outbreak of the disease was prevented. It was
only after great searching of heart that Pasteur, after consultation with
some trusted medical friends, ventured upon trying this practice upon
man. It has since been extensively adopted in various parts of the world
with increasing success as the details of the method were improved. It is
not of course the case that every one bitten by a really rabid animal takes
the disease ; but the percentage of those who do so, which was formerly
large, has been reduced almost to zero by this treatment, if not too long
delayed.

While the intensity of rabies in the rabbit is undoubtedly due to a
peculiarly virulent form of the microbe concerned, we cannot suppose that
the daily diminishing potency of the cord suspended in dry warm air is
an instance of attenuation of virus, using the term ' virus ' as synonymous
with the microbe concerned. In other words, we have no reason to
believe that the special micro-organism of hydrophobia continues to
develop in the dead cord and produce successively a milder and milder

progeny, since rabies cannot be cultivated in the nervous system of a dead animal  We must rather conclude that there must be some chemical poison present which gradually loses its potency as time passes.  And this leads me to refer to another most important branch of this large subject of bacteriology, that of the poisonous products of microbes.

It was shown several years ago by Roux and Yersin, working in the *Institut Pasteur*, that the crust or false membrane which forms upon the throats of patients affected with diphtheria contains bacteria which can be cultivated outside the body in a nutrient liquid, with the result that it acquires poisonous qualities of astonishing intensity, comparable to that of the secretion of the poison-glands of the most venomous serpents. And they also ascertained that the liquid retained this property after the microbes had been removed from it by filtration, which proved that the poison must be a chemical substance in solution, as distinguished from the living element which had produced it.  These poisonous products of bacteria, or toxins as they have been termed, explain the deadly effects of some microbes, which it would otherwise be impossible to understand. Thus, in diphtheria itself the special bacillus which was shown by Loffler to be its cause, does not become propagated in the blood, like the microbe of chicken cholera, but remains confined to the surface on which it first appeared : but the toxin which it secretes is absorbed from that surface into the blood, and so poisons the system.  Similar observations have been made with regard to the microbes of some other diseases, as, for example, the bacillus of tetanus or lockjaw.  This remains localised in the wound, but forms a special toxin of extreme potency, which becomes absorbed and diffused through the body.

Wonderful as it seems, each poisonous microbe appears to form its own peculiar toxin.  Koch's tuberculin was of this nature, a product of the growth of the tubercle bacillus in culture media.  Here, again, great effects were produced by extremely minute quantities of the substance, but here a new peculiarity showed itself, viz. that patients affected with tubercular disease, in any of its varied forms, exhibited inflammation in the affected part and general fever after receiving under the skin an amount of the material which had no effect whatever upon healthy persons.  I witnessed in Berlin some instances of these effects, which were simply astounding.  Patients affected with a peculiar form of obstinate ulcer of the face showed, after a single injection of the tuberculin, violent inflammatory redness and swelling of the sore and surrounding skin ; and, what was equally surprising, when this disturbance subsided the disease was found to have undergone great improvement.  By repetitions of such procedures, ulcers which had previously been steadily advancing, in spite of ordinary treatment, became greatly reduced in size, and in some instances apparently cured.  Such results led Koch to believe that he had obtained an effectual means of dealing with tubercular disease in all its forms.  Unhappily, the apparent cure proved to be only of

transient duration, and the high hopes which had been inspired by Koch's great reputation were dashed. It is but fair to say that he was strongly urged to publish before he was himself disposed to do so, and we cannot but regret that he yielded to the pressure put upon him.

But though Koch's sanguine anticipations were not realised, it would be a great mistake to suppose that his labours with tuberculin have been fruitless. Cattle are liable to tubercle, and, when affected with it, may become a very serious source of infection for human beings, more especially when the disease affects the udders of cows, and so contaminates the milk. By virtue of the close affinity that prevails between the lower animals and ourselves, in disease as well as in health, tuberculin produces fever in tubercular cows in doses which do not affect healthy beasts. Thus, by the subcutaneous use of a little of the fluid, tubercle latent in internal organs of an apparently healthy cow can be with certainty revealed, and the slaughter of the animal after this discovery protects man from infection.

It has been ascertained that glanders presents a precise analogy with tubercle as regards the effects of its toxic products. If the microbe which has been found to be the cause of this disease is cultivated in appropriate media, it produces a poison which has received the name of mallein, and the subcutaneous injection of a suitable dose of this fluid into a glandered horse causes striking febrile symptoms which do not occur in a healthy animal. Glanders, like tubercle, may exist in insidious latent forms which there was formerly no means of detecting, but which are at once disclosed by this means. If a glandered horse has been accidentally introduced into a large stable, this method of diagnosis surely tells if it has infected others. All receive a little mallein. Those which become affected with fever are slaughtered, and thus not only is the disease prevented from spreading to other horses, but the grooms are protected from a mortal disorder.

This valuable resource sprang from Koch's work on tuberculin, which has also indirectly done good in other ways. His distinguished pupil, Behring, has expressly attributed to those researches the inspiration of the work which led him and his since famous collaborateur, the Japanese Kitasato, to their surprising discovery of anti-toxic serum. They found that if an animal of a species liable to diphtheria or tetanus received a quantity of the respective toxin, so small as to be harmless, and afterwards, at suitable intervals, successively stronger and stronger doses, the creature, in course of time, acquired such a tolerance for the poison as to be able to receive with impunity a quantity very much greater than would at the outset have proved fatal. So far, we have nothing more than seems to correspond with the effects of the increasingly potent cords in Pasteur's treatment of rabies. But what was entirely new in their results was that, if blood was drawn from an animal which had acquired this high degree of artificial immunity, and some of the clear fluid or serum which exuded from it after it had clotted was introduced under the

skin of another animal, this second animal acquired a strong, though more transient, immunity against the particular toxin concerned. The serum in some way counteracted the toxin or was antitoxic. But, more than that, if some of the antitoxic serum was applied to an animal after it had already received a poisonous dose of the toxin, it preserved the life of the creature, provided that too long a time had not elapsed after the poison was introduced. In other words, the antitoxin proved to be not only preventive but curative

Similar results were afterwards obtained by Ehrlich, of Berlin, with some poisons not of bacterial origin, but derived from the vegetable kingdom ; and quite recently the independent labours of Calmette of Lille and Fraser of Edinburgh have shown that antidotes of wonderful efficacy against the venom of serpents may be procured on the same principle. Calmette has obtained antitoxin so powerful that a quantity of it only a 200,000th part of the weight of an animal will protect it perfectly against a dose of the secretion of the poison glands of the most venomous serpents known to exist, which without such protection would have proved fatal in four hours. For curative purposes larger quantities of the remedy are required, but cases have been already published by Calmette in which death appears to have been averted in the human subject by this treatment.

Behring's darling object was to discover means of curing tetanus and diphtheria in man. In tetanus the conditions are not favourable ; because the specific bacilli lurk in the depths of the wound, and only declare their presence by symptoms caused by their toxin having been already in a greater or less amount diffused through the system ; and in every case of this disease there must be a fear that the antidote may be applied too late to be useful. But in diphtheria the bacilli very early manifest their presence by the false membrane which they cause upon the throat, so that the antitoxin has a fair chance ; and here we are justified in saying that Behring's object has been attained.

The problem, however, was by no means so simple as in the case of some mere chemical poison. However effectual the antitoxin might be against the toxin, if it left the bacilli intact, not only would repeated injections be required to maintain the transient immunity to the poison perpetually secreted by the microbes, but the bacilli might by their growth and extension cause obstruction of the respiratory passages.

Roux, however, whose name must always be mentioned with honour in relation to this subject, effectually disposed of this difficulty. He showed by experiments on animals that a diphtheritic false membrane, rapidly extending and accompanied by surrounding inflammation, was brought to a stand by the use of the antitoxin, and soon dropped off, leaving a healthy surface. Whatever be the explanation, the fact was thus established that the antitoxic serum, while it renders the toxin harmless, causes the microbe to languish and disappear.

No theoretical objection could now be urged against the treatment ;

and it has during the last two years been extensively tested in practice in various parts of the world, and it has gradually made its way more and more into the confidence of the profession. One important piece of evidence in its favour in this country is derived from the report of the six large hospitals under the management of the London Asylums Board. The medical officers of these hospitals at first naturally regarded the practice with scepticism : but as it appeared to be at least harmless, they gave it a trial ; and during the year 1895 it was very generally employed upon the 2,182 cases admitted ; and they have all become convinced of its great value. In the nature of things, if the theory of the treatment is correct, the best results must be obtained when the patients are admitted at an early stage of the attack, before there has been time for much poisoning of the system : and accordingly we learn from the report that, comparing 1895 with 1894, during which latter year the ordinary treatment had been used, the percentage of mortality, in all the six hospitals combined, among the patients admitted on the first day of the disease, which in 1894 was 22·5, was only 4·6 in 1895 ; while for those admitted on the second day the numbers are 27 for 1894 and 14·8 for 1895. Thus for cases admitted on the first day the mortality was only one-fifth of what it was in the previous year, and for those entering on the second it was halved. Unfortunately in the low parts of London which furnish most of these patients the parents too often delay sending in the children till much later : so that on the average no less than 67·5 per cent. were admitted on the fourth day of the disease or later. Hence the aggregate statistics of all cases are not nearly so striking. Nevertheless, taking it altogether, the mortality in 1895 was less than had ever before been experienced in those hospitals. I should add that there was no reason to think that the disease was of a milder type than usual in 1895 ; and no change whatever was made in the treatment except as regards the antitoxic injections.

There is one piece of evidence recorded in the report which, though it is not concerned with high numbers, is well worthy of notice. It relates to a special institution to which convalescents from scarlet fever are sent from all the six hospitals. Such patients occasionally contract diphtheria, and when they do so the added disease has generally proved extremely fatal. In the five years preceding the introduction of the treatment with antitoxin the mortality from this cause had never been less than 50 per cent, and averaged on the whole 61·9 per cent. During 1895, under antitoxin, the deaths among the 119 patients of this class were only 7·5 per cent., or one-eighth of what had been previously experienced. This very striking result seems to be naturally explained by the fact that these patients being already in hospital when the diphtheria appeared, an unusually early opportunity was afforded for dealing with it.

There are certain cases of so malignant a character from the first that no treatment will probably ever be able to cope with them. But taking

all cases together it seems probable that Behring's hope that the mortality may be reduced to 5 per cent will be fully realised when the public become alive to the paramount importance of having the treatment commenced at the outset of the disease.

There are many able workers in the field of Bacteriology whose names time does not permit me to mention, and to whose important labours I cannot refer ; and even those researches of which I have spoken have been, of course, most inadequately dealt with. I feel this especially with regard to Pasteur, whose work shines out more brightly the more his writings are perused.

I have lastly to bring before you a subject which, though not bacteriological, has intimate relations with bacteria. If a drop of blood is drawn from the finger by a prick with a needle and examined microscopically between two plates of glass, there are seen in it minute solid elements of two kinds, the one pale orange bi-concave discs, which, seen in mass, give the red colour to the vital fluid, the other more or less granular spherical masses of the soft material called protoplasm, destitute of colour, and therefore called the colourless or white corpuscles. It has been long known that if the microscope was placed at such a distance from a fire as to have the temperature of the human body, the white corpuscles might be seen to put out and retract little processes or pseudopodia, and by their means crawl over the surface of the glass, just like the extremely low forms of animal life termed, from this faculty of changing their form, amœbæ. It was a somewhat weird spectacle, that of seeing what had just before been constituents of our own blood moving about like independent creatures. Yet there was nothing in this inconsistent with what we knew of the fixed components of the animal frame For example, the surface of a frog's tongue is covered with a layer of cells, each of which is provided with two or more lashing filaments or cilia, and those of all the cells acting in concert cause a constant flow of fluid in a definite direction over the organ. If we gently scrape the surface of the animal's tongue, we can detach some of these ciliated cells ; and on examining them with the microscope in a drop of water, we find that they will continue for an indefinite time their lashing movements, which are just as much living or vital in their character as the writhings of a worm. And, as I observed many years ago, these detached cells behave under the influence of a stimulus just like parts connected with the body, the movements of the cilia being excited to greater activity by gentle stimulation, and thrown into a state of temporary inactivity when the irritation was more severe. Thus each constituent element of our bodies may be regarded as in one sense an independent living being, though all work together in marvellous harmony for the good of the body politic. The independent movements of the white corpuscles outside the body were therefore not astonishing : but they long remained matters of mere curiosity. Much interest was called to them by the observation of the German pathologist Cohnheim that in some

inflammatory conditions they passed through the pores in the walls of the finest blood-vessels, and thus escaped into the interstices of the surrounding tissues. Cohnheim attributed their transit to the pressure of the blood. But why it was that, though larger than the red corpuscles, and containing a nucleus which the red ones have not, they alone passed through the pores of the vessels, or why it was that this emigration of the white corpuscles occurred abundantly in some inflammations and was absent in others, was quite unexplained

These white corpuscles, however, have been invested with extraordinary new interest by the researches of the Russian naturalist and pathologist, Metchnikoff. He observed that, after passing through the walls of the vessels, they not only crawl about like amœbæ, but, like them, receive nutritious materials into their soft bodies and digest them. It is thus that the effete materials of a tadpole's tail are got rid of ; so that they play a most important part in the function of absorption.

But still more interesting observations followed. He found that a microscopic crustacean, a kind of water-flea, was liable to be infested by a fungus which had exceedingly sharp-pointed spores These were apt to penetrate the coats of the creature's intestine, and project into its body-cavity. No sooner did this occur with any spore than it became surrounded by a group of the cells which are contained in the cavity of the body and correspond to the white corpuscles of our blood. These proceeded to attempt to devour the spore ; and if they succeeded, in every such case the animal was saved from the invasion of the parasite. But if the spores were more than could be disposed of by the devouring cells (phagocytes, as Metchnikoff termed them), the water-flea succumbed.

Starting from this fundamental observation, he ascertained that the microbes of infective diseases are subject to this same process of devouring and digestion, carried on both by the white corpuscles and by cells that line the blood-vessels. And by a long series of most beautiful researches he has, as it appears to me, firmly established the great truth that phagocytosis is the main defensive means possessed by the living body against the invasions of its microscopic foes. The power of the system to produce antitoxic substances to counteract the poisons of microbes is undoubtedly in its own place of great importance But in the large class of cases in which animals are naturally refractory to particular infective diseases the blood is not found to yield any antitoxic element by which the natural immunity can be accounted for. Here phagocytosis seems to be the sole defensive agency. And even in cases in which the serum does possess antitoxic, or, as it would seem in some cases, germicidal properties, the bodies of the dead microbes must at last be got rid of by phagocytosis, and some recent observations would seem to indicate that the useful elements of the serum may be, in part at least, derived from the digestive juices of the phagocytes. If ever there was a romantic chapter in pathology, it has surely been that of the story of phagocytosis.

I was myself peculiarly interested by these observations of Metchnikoff's, because they seemed to me to afford clear explanation of the healing of wounds by first intention under circumstances before incomprehensible. This primary union was sometimes seen to take place in wounds treated with water-dressing, that is to say, a piece of wet lint covered with a layer of oiled-silk to keep it moist. This, though cleanly when applied, was invariably putrid within twenty-four hours. The layer of blood between the cut surfaces was thus exposed at the outlet of the wound to a most potent septic focus. How was it prevented from putrefying, as it would have done under such influence if, instead of being between divided living tissues, it had been between plates of glass or other indifferent material ? Pasteur's observations pushed the question a step further. It now was, How were the bacteria of putrefaction kept from propagating in the decomposable film ? Metchnikoff's phagocytosis supplied the answer. The blood between the lips of the wound became rapidly peopled with phagocytes, which kept guard against the putrefactive microbes and seized them as they endeavoured to enter.

If phagocytosis was ever able to cope with septic microbes in so concentrated and intense a form, it could hardly fail to deal effectually with them in the very mitigated condition in which they are present in the air. We are thus strongly confirmed in our conclusion that the atmospheric dust may safely be disregarded in our operations : and Metchnikoff's researches, while they have illumined the whole pathology of infective diseases, have beautifully completed the theory of antiseptic treatment in surgery.

I might have taken equally striking illustrations of my theme from other departments in which microbes play no part. In fact any attempt to speak of all that the art of healing has borrowed from science and contributed to it during the past half-century would involve a very extensive dissertation on pathology and therapeutics. I have culled specimens from a wide field ; and I only hope that in bringing them before you I have not overstepped the bounds of what is fitting before a mixed company. For many of you my remarks can have had little if any novelty . for others they may perhaps possess some interest as showing that Medicine is no unworthy ally of the British Association—that, while her practice is ever more and more based on science, the ceaseless efforts of her votaries to improve what have been fittingly designated *Quæ prosunt omnibus artes*, are ever adding largely to the sum of abstract knowledge.

# British Association for the Advancement of Science.

## LIVERPOOL, 1896.

# ADDRESS

### TO THE

# MATHEMATICAL AND PHYSICAL SECTION

### BY

## Professor J. J. THOMSON, M.A., F.R.S.

### PRESIDENT OF THE SECTION.

THERE is a melancholy reminiscence connected with this meeting of our Section, for when the British Association last met in Liverpool the chair in Section A was occupied by Clerk-Maxwell. In the quarter of a century which has elapsed since that meeting, one of the most important advances made in our science has been the researches which, inspired by Maxwell's view of electrical action, confirmed that view, and revolutionised our conception of the processes occurring in the Electro-magnetic field. When the Association last met in Liverpool Maxwell's view was almost without supporters, to-day its opponents are fewer than its supporters then. Maxwell's theory, which is the development and extension of Faraday's, has not only affected our way of regarding the older phenomena of electricity, it has, in the hands of Hertz and others, led to the discovery of whole regions of phenomena previously undreamt of. It is sad to think that his premature death prevented him from reaping the harvest he had sown. His writings are, however, with us, and are a storehouse to which we continually turn, and never, I think, without finding something valuable and suggestive.

> ' Thus ye teach us day by day,
> Wisdom, though now far away.'

The past year has been rich in matters of interest to physicists. In it has occurred the jubilee of Lord Kelvin's tenure of the Professorship of Natural Philosophy at the University of Glasgow. Some of us were privileged to see this year at Glasgow an event unprecedented in the history of physical science in England, when congratulations to Lord Kelvin on the jubilee of his professorship were offered by people of every condition and country. Every scientific society and every scientific man is Lord Kelvin's debtor; but no society and no body of men owe him a greater debt than Section A of the British Association; he has done more for this Section than any one else, he has rarely missed its meetings, he has contributed to the Section papers which will make its proceedings imperishable, and by his enthusiasm he has year by year inspired the workers in this Section to renew with increased vigour their struggles to penetrate the secrets of nature. Long may we continue to receive from him the encouragement and assistance which have been so freely given for the past half century.

By the death of Sir W. R. Grove, the inventor of Grove's cell, we have lost a physicist whose name is a familiar one in every laboratory in the world. Besides the Grove cell, we owe to him the discovery of the gas battery, and a series of researches on the electrical behaviour of gases, whose importance is only now beginning

to be appreciated. His essay on the correlation of the physical forces had great influence in promoting that belief in the unity of the various branches of physics which is one of the characteristic features of modern natural philosophy.

In the late Professor Stoletow, of Moscow, we have lost the author of a series of most interesting researches on the electrical properties of gases illuminated by ultra-violet light, researches which, from their place of publication, are, I am afraid, not so well known in this country as they deserve to be.

As one who unfortunately of late years has had only too many opportunities of judging of the teaching of science in our public and secondary schools, I should like to bear testimony to the great improvement which has taken place in the teaching of physics in these schools during the past ten years. The standard attained in physics by the pupils of these schools is increasing year by year, and great credit is due to those by whose labours this improvement has been accomplished. I hope I may not be considered ungrateful if I express the opinion that in the zeal and energy which is now spent in the teaching of physics in schools, there may lurk a temptation to make the pupils cover too much ground. You may by careful organisation and arrangement ensure that boys shall be taken over many branches of physics in the course of a short time; it is indeed not uncommon to find boys of 17 or 18 who have compassed almost the whole range of physical subjects. But although you may increase the rate at which information is acquired, you cannot increase in anything like the same proportion the rate at which the subject is assimilated, so as to become a means of strengthening the mind and a permanent mental endowment when the facts have long been forgotten.

Physics can be taught so as to be a subject of the greatest possible educational value, but when it is so it is not so much because the student acquires a knowledge of a number of interesting and important facts, as by the mental training the study affords in, as Maxwell said, 'bringing our theoretical knowledge to bear on the objects and the objects on our theoretical knowledge.' I think this training can be got better by going very slowly through such a subject as mechanics, making the students try innumerable experiments of the simplest and, what is a matter of importance in school teaching, of the most inexpensive kind, but always endeavouring to arrive at numerical results, rather than by attempting to cover the whole range of mechanics, light, heat, sound, electricity, and magnetism. I confess I regret the presence in examinations intended for school boys of many of these subjects.

I think, too, that in the teaching of physics at our universities, there is perhaps a tendency to make the course too complex and too complete. I refer especially to the training of those students who intend to become physicists. I think that after a student has been trained to take accurate observations, to be alive to those pitfalls and errors to which all experiments are liable, mischief may in some cases be done if, with the view of learning a knowledge of methods, he is kept performing elaborate experiments, the results of which are well known. It is not given to many to wear a load of learning lightly as a flower. With many students a load of learning, especially if it takes a long time to acquire, is apt to crush enthusiasm. Now, there is, I think, hardly any quality more essential to success in physical investigations than enthusiasm. Any investigation in experimental physics requires a large expenditure of both time and patience; the apparatus seldom, if ever, begins by behaving as it ought; there are times when all the forces of nature, all the properties of matter, seem to be fighting against us; the instruments behave in the most capricious way, and we appreciate Coutts Trotter's saying, that the doctrine of the constancy of nature could never have been discovered in a laboratory. These difficulties have to be overcome, but it may take weeks or months to do so, and, unless the student is enthusiastic, he is apt to retire disheartened from the contest I think, therefore, that the preservation of youthful enthusiasm is one of the most important points for consideration in the training of physicists. In my opinion this can best be done by allowing the student, even before he is supposed to be acquainted with the whole of physics, to begin some original research of a simple kind under the guidance of a teacher who will encourage him and assist in the removal of difficulties. If the student once tastes the delights of the successful completion of an investigation, he is not likely to go back, and will be better

equipped for investigating the secrets of nature than if, like the White Knight of 'Alice in Wonderland,' he commences his career knowing how to measure or weigh every physical quantity under the sun, but with little desire or enthusiasm to have anything to do with any of them. Even for those students who intend to devote themselves to other pursuits than physical investigation, the benefits derived from original investigation as a means of general education can hardly be over-estimated, the necessity it entails of independent thought, perseverance in overcoming difficulties, and the weighing of evidence gives it an educational value which can hardly be rivalled. We have to congratulate ourselves that through the munificence of Mr. Ludwig Mond, in providing and endowing a laboratory for research, the opportunities for pursuing original investigations in this country have been greatly increased.

The discovery at the end of last year by Professor Röntgen of a new kind of radiation from a highly exhausted tube through which an electric discharge is passing, has aroused an amount of interest unprecedented in the history of physical science. The effects produced *inside* such a tube by the cathode rays, the bright phosphorescence of the glass, the shadows thrown by opaque objects, the deflection of the rays by a magnet, have, thanks to the researches of Crookes and Goldstein, long been familiar to us, but it is only recently that the remarkable effects which occur outside such a tube have been discovered. In 1893, Lenard, using a tube provided with a window made of a very thin plate of aluminium, found that a screen impregnated with a solution of a phosphorescent substance became luminous if placed outside the tube in the prolongation of the line from the cathode through the aluminium window. He also found that photographic plates placed outside the tube in this line were affected, and electrified bodies were discharged; he also obtained by these rays photographs through plates of aluminium or quartz. He found that the rays were affected by a magnet, and regarded them as the prolongations of the cathode rays. This discovery was at the end of last year followed by that of Röntgen, who found that the region round the discharge tube is traversed by rays which affect a photographic plate after passing through substances such as aluminium or cardboard, which are opaque to ordinary light: which pass from one substance to another, without any refraction, and with but little regular reflection; and which are not affected by a magnet. We may, I think, for the purposes of discussion, conveniently divide the rays occurring in or near a vacuum tube traversed by an electric current into three classes, without thereby implying that they are necessarily distinctly different in physical character. We have (1) the cathode rays inside the tube, which are deflected by a magnet; (2) the Lenard rays outside the tube, which are also deflected by a magnet; and (3) the Röntgen rays, which are not, as far as is known, deflected by a magnet. Two views are held as to the nature of the cathode rays; one view is, that they are particles of gas carrying charges of negative electricity, and moving with great velocities which they have acquired as they travelled through the intense electric field which exists in the neighbourhood of the negative electrode. The phosphorescence of the glass is on this view produced by the impact of these rapidly moving charged particles, though whether it is produced by the mechanical violence of the impact, or whether it is due to an electro-magnetic impulse produced by the sudden reversal of the velocity of the negatively charged particle—whether, in fact, it is due to mechanical or electrical causes, is an open question. This view of the constitution of the cathode rays explains in a simple way the deflection of those rays in a magnetic field, and it has lately received strong confirmation from the results of an experiment made by Perrin. Perrin placed inside the exhausted tube a cylindrical metal vessel with a small hole in it, and connected this cylinder with the leaves of a gold-leaf electroscope. The cathode rays could, by means of a magnet, be guided so as either to pass into the cylinder through the aperture, or turned quite away from it. Perrin found that when the cathode rays passed into the cylinder the gold leaf of the electroscope diverged, and had a negative charge, showing that the bundle of cathode rays enclosed by the cylinder had a charge of negative electricity. Crookes had many years ago exposed a disc connected with a gold-leaf electroscope to the bombardment of the cathode rays, and found that the disc received a slight *positive*

charge ; with this arrangement, however, the charged particles had to give up their charges to the disc if the gold leaves of the electroscope were to be affected, and we know that it is extremely difficult, if not impossible, to get electricity out of a charged gas merely by bringing the gas in contact with a metal. Lord Kelvin's electric strainers are an example of this. It is a feature of Perrin's experiment that since it acts by induction, the indications of the electroscope are independent of the communication of the charges of electricity from the gas to the cylinder, and since the cathode rays fall on the inside of the cylinder, the electroscope would not be affected, even if there were such an effect as is produced when ultra-violet light falls upon the surface of an electro-negative metal when the metal acquires a positive charge. Since any such process cannot affect the total amount of electricity inside the cylinder, it will not affect the gold leaves of the electroscope ; in fact, Perrin's experiments prove that the cathode rays carry a charge of negative electricity.

The other view held as to the *constitution* of the cathode rays is that they are waves in the ether. It would seem difficult to account for the result of Perrin's experiment on this view, and also I think very difficult to account for the magnetic deflection of the rays. Let us take the case of a uniform magnetic field : the experiments which have been made on the magnetic deflection of these rays seem to make it clear that in a magnetic field which is sensibly uniform, the path of these rays is curved ; now if these rays were due to ether waves, the curvature of the path would show that the velocity of propagation of these waves varied from point to point of the path. That is, the velocity of propagation of these waves is not only affected by the magnetic field, it is affected differently at different parts of the field. But in a uniform field what is there to differentiate one part from another, so as to account for the variability of the velocity of wave propagation in such a field ? The curvature of the path in a uniform field could not be accounted for by supposing that the velocity of this wave motion depended on the strength of the magnetic field, or that the magnetic field, by distorting the shape of the boundary of the negative dark space, changed the direction of the wave front, and so produced a deflection of the rays. The chief reason for supposing that the cathode rays are a species of wave motion is afforded by Lenard's discovery, that when the cathode rays in a vacuum tube fall on a thin aluminium window in the tube, rays having similar properties are observed on the side of the window outside the tube ; this is readily explained on the hypothesis that the rays are a species of wave motion to which the window is partially transparent, while it is not very likely that particles of the gas in the tube could force their way through a piece of metal. This discovery of Lenard's does not, however, seem to me incompatible with the view that the cathode rays are due to negatively charged particles moving with high velocities. The space outside Lenard's tube must have been traversed by Röntgen rays, these would put the surrounding gas in a state in which a current would be readily started in the gas if any electromotive force acted upon it. Now, though the metal window in Lenard's experiments was connected with the earth, and would, therefore, screen off from the outside of the tube any effect arising from slow electrostatic changes in the tube, it does not follow that it would be able to screen off the electrostatic effect of charged particles moving to and from the tube with very great rapidity. For in order to screen off electrostatic effects, there must be a definite distribution of electrification over the screen ; changes in this distribution, however, take a finite time, which depends upon the dimensions of the screen and the electrical conductivity of the material of which it is made. If the electrical changes in the tube take place at above a certain rate, the distribution of electricity on the screen will not have time to adjust itself, and the screen will cease to shield off all electrostatic effects. Thus the very rapid electrical changes which would take place if rapidly moving charged bodies were striking against the window, might give rise to electromotive forces in the region outside the window, and produce convection currents in the gas which has been made a conductor by the Röntgen rays. The Lenard rays would thus be analogous in character to the cathode rays, both being convective currents of electricity. Though there are some points in the behaviour of these Lenard rays which do not admit of a very ready explanation from this point of view, yet the

difficulties in its way seem to me considerably less than that of supposing that a wave in the ether can change its velocity when moving from point to point in a uniform magnetic field.

I now pass on to the consideration of the Röntgen rays. We are not yet acquainted with any crucial experiment which shows unmistakably that these rays are waves of transverse vibration in the ether, or that they are waves of normal vibration, or indeed that they are vibrations at all. As a working hypothesis, however, it may be worth while considering the question whether there is any property known to be possessed by these rays which is not possessed by some form or other of light. The many forms of light have in the last few months received a noteworthy addition by the discovery of M. Becquerel of an invisible radiation, possessing many of the properties of the Röntgen rays, which is emitted by many fluorescent substances, and to an especially marked extent by the uranium salts. By means of this radiation, which, since it can be polarised, is unquestionably light, photographs through opaque substances similar to, though not so beautiful as, those obtained by means of Röntgen rays, can be taken, and, like the Röntgen rays, they cause an electrified body on which they shine to lose its charge, whether this be positive or negative.

The two respects in which the Röntgen rays differ from light is in the absence of refraction and perhaps of polarisation. Let us consider the absence of refraction first. We know cases in which special rays of the spectrum pass from one substance to another without refraction; for example, Kundt showed that gold, silver, copper allow some rays to pass through them without bending, while other rays are bent in the wrong direction. Pfluger has lately found that the same is true for some of the aniline dyes when in a solid form. In addition to this, the theory of dispersion of light shows that there will be no bending when the frequency of the vibration is very great. I have here a curve taken from a paper by Helmholtz, which shows the relation between the refractive index and the frequency of vibration for a substance whose molecules have a natural period of vibration, and one only ; the frequency of this vibration is represented by OK in the diagram. The refractive index increases with the frequency of the light until the latter is equal to the frequency of the natural vibration of the substance ; the refractive index then diminishes, becomes less than unity, and finally approaches unity, and is practically equal to it when the frequency of the light greatly exceeds that of the natural vibration of the molecule. Helmholtz's results are obtained on the supposition that a molecule of the refracting substance consists of a pair of oppositely electrified atoms, and that the specific inductive capacity of the medium consists of two parts, one due to the ether, the other to the setting of the molecules along the lines of electric force.

Starting from this supposition we can easily see without mathematical analysis that the relation between the refractive index and the frequency must be of the kind indicated by the curve. Let us suppose that an electromotive force of given amplitude acts on this mixture of molecules and ether, and let us start with the frequency of the external electromotive force less than that of the free vibrations of the molecules : as the period of the force approaches that of the molecules, the effect of the force in pulling the molecules into line will increase ; thus the specific inductive capacity, and therefore the refractive index, increases with the frequency of the external force ; the effect of the force on the orientation of the molecules will be greatest when the period of the force coincides with that of the molecules. As long as the frequency of the force is less than that of the molecules, the external field tends to make the molecules set so as to increase the specific inductive capacity of the mixture ; as soon, however, as the frequency of the force exceeds that of the molecules, the molecules, if there are no viscous forces, will all topple over and point so as to make the part of the specific inductive capacity due to the molecules of opposite sign to that due to the ether. Thus, for frequencies greater than that of the molecules, the specific inductive capacity will be less than unity. When the frequency of the force only slightly exceeds that of the molecules, the effect of the external field on the molecules is very great, so that if there are a considerable number of molecules, the negative part of the specific inductive capacity due to the

molecules may be greater than the positive part due to the ether, so that the specific inductive capacity of the mixture of molecules and ether would be negative; no waves of this period could then travel through the medium, they would be totally reflected from the surface.

As the frequency of the force gets greater and greater, its effect in making the molecules set will get less and less, but the waves will continue to be totally reflected until the negative part of the specific inductive capacity due to the molecules is just equal to the positive part due to the ether. Here the refractive index of the mixture is zero  As the frequency of the force increases, its effect on the molecules gets less and less, so that the specific inductive capacity continually approaches that due to the ether alone, and practically coincides with it as soon as the frequency of the force is a considerable multiple of that of the molecules. In this case both the specific inductive capacity and the refractive index of the medium are the same as that of the ether, and there is consequently no refraction. Thus the absence of refraction, instead of being in contradiction to the Röntgen rays, being a kind of light, is exactly what we should expect if the wave length of the light were exceedingly small.

The other objection to these rays being a kind of light is, that there is no very conclusive evidence of the existence of polarisation. Numerous experiments have been made on the difference between the absorption of these rays by a pair of tourmaline plates when their axes are crossed or parallel. Many observers have failed to observe any difference at all between the absorption in the two cases. Prince Galitzine and M. de Karnojitsky, by a kind of cumulative method, have obtained photographs which seem to show that there is a slightly greater absorption when the axes are crossed than there is when the axes are parallel. There can, however, be no question that the effect, if it exists at all, is exceedingly small compared with the corresponding effect for visible light. Analogy, however, leads us to expect that to get polarisation effects we must use, in the case of short waves, polarisers of a much finer structure than would be necessary for long ones. Thus a wire bird-cage will polarise long electrical waves, but will have no effect on visible light. Rubens and Du Bois made an instrument which would polarise infra red rays by winding very fine wires very close together on a framework; this arrangement, however, was too coarse to polarise visible light. Thus, though the structure of the tourmaline is fine enough to polarise the visible rays, it may be much too coarse to polarise the Röntgen rays if these have exceedingly small wave-lengths. As far as our knowledge of these rays extends, I think we may say that though there is no direct evidence that they are a kind of light, there are no properties of the rays which are not possessed by some variety of light.

It is clear that if the Röntgen rays are light rays, their wave-lengths are of an entirely different order to those of visible light. It is perhaps worth notice that on the electro-magnetic theory of light we might expect two different types of vibration if we suppose that the atoms in the molecule of the vibrating substance carried electrical charges. One set of vibrations would be due to the oscillations of the bodies carrying the charges, the other set to the oscillation of the charges on these bodies. The wave-length of the second set of vibrations would be commensurate with molecular dimensions; can these vibrations be the Röntgen rays? If so, we should expect them to be damped with such rapidity as to resemble electrical impulses rather than sustained vibrations.

If we turn from the rays themselves to the effects they produce, we find that the rays alter the properties of the substances through which they are passing. This change is most apparent in the effects produced on the electrical properties of the substances. A gas, for example, while transmitting these rays is a conductor of electricity. It retains its conducting properties for some little time after the rays have ceased to pass through it, but Mr. Rutherford and I have lately found that the conductivity is destroyed if a current of electricity is sent through the Röntgenised gas. The gas in this state behaves in this respect like a very dilute solution of an electrolyte. Such a solution would cease to conduct after enough electricity had been sent through it to electrolyse all the molecules of the electrolyte. When a current is passing through a gas exposed to the rays,

the current destroys and the rays produce the structure which gives conductivity to the gas ; when things have reached a steady state the rate of destruction by the current must equal the rate of production by the rays. The current can thus not exceed a definite value, otherwise more of the conducting gas would be destroyed than is produced.

This explains the very characteristic feature that in the passage of electricity through gases exposed to Röntgen rays, the current, though at first proportional to the electromotive force, soon reaches a value where it is almost constant and independent of the electromotive force, and we get to a state when a tenfold increase in the electromotive force only increases the current by a few per cent. The conductivity under the Röntgen rays varies greatly from one gas to another, the halogens and their gaseous compounds, the compounds of sulphur, and mercury vapour, are among the best conductors. It is worthy of note that those gases which are the best conductors when exposed to the rays are either elements, or compounds of elements, which have in comparison with their valency very high refractive indices.

The conductivity conferred by the rays on a gas is not destroyed by a considerable rise in temperature ; it is, for example, not destroyed if it be sucked through metal tubing raised to a red heat. The conductivity is, however, destroyed if the gas is made to bubble through water, it is also destroyed if the gas is forced through a plug of glass wool. This last effect seems to indicate that the structure which confers conductivity on the gas is of a very coarse kind, and we get confirmation of this from the fact that a very thin layer of gas exposed to the Röntgen rays does not conduct nearly so well as a thicker one. I think we have evidence from other sources that electrical conduction is a process that requires a considerable space—a space large enough to enclose a very large number of molecules.

Thus Koller found that the specific resistances of petroleum, turpentine, and distilled water, when determined from experiments made with very thin layers of these substances, was very much larger than when determined from experiments with thicker layers. Even in the case of metals there is evidence that the metal has to be of appreciable size if it is to conduct electricity. The theory of the scattering of light by small particles shows that, if we assume the truth of the electro-magnetic theory of light, the effects should be different according as the small particles are insulators or conductors. When the small particles are non-conductors, theory and experiment concur in showing that the direction of complete polarisation for the scattered light is at right angles to the direction of the incident light, while if the small particles are conductors, theory indicates that the direction of complete polarisation makes an angle of 60° with the incident light. This result is not, however, confirmed by the experiments made by Professor Threlfall on the scattering of light by very small particles of gold. He found that the gold scattered the light in just the same way as a non-conductor, giving complete polarisation at right angles to the incident light. This would seem to indicate that those very finely divided metallic particles no longer acted as conductors. Thus there seems evidence that in the case of conduction through gases, through badly conducting liquids and through metals, electric conduction is a process which requires a very considerable space and aggregations of large numbers of molecules. I have not been able to find any direct experimental evidence as to whether the same is true for electrolytes. Experiments on the resistance of thin layers of electrolytes would be of considerable interest, as according to one widely accepted view of electrolysis conduction through electrolytes, so far from being effected by aggregations of molecules, takes place by means of the ion, a structure simpler than that of the molecule, so that if this represents the process of electrolytic conduction, there would not seem room for the occurrence of an effect which occurs with every other kind of conduction.

In this building it is only fitting that some reference should be made to the question of the movement of the ether. You are all doubtless acquainted with the heroic attempts made by Professor Lodge to set the ether in motion, and how suc-

cessfully the ether resisted them. It seems to be conclusively proved that a solid body in motion does not set in motion the ether at an appreciable distance outside it; so that if the ether is disturbed at all in such a case, the disturbance is not comparable with that produced by a solid moving through an incompressible fluid, but must be more analogous to that which would be produced by the motion through the liquid of a body of very open structure, such as a piece of wire netting, where the motion of the fluid only extends to a distance comparable with the diameter of the wire, and not with that of the piece of netting. There is another class of phenomena relating to the movement of the ether which is, I think, deserving of consideration, and that is the effect of a varying electro-magnetic field in setting the ether in motion. I do not remember to have seen it pointed out that the electro-magnetic theory of light implicitly assumes that the ether is not set in motion even when acted on by mechanical forces. On the electro-magnetic theory of light such forces do exist, and the equations used are only applicable when the ether is at rest. Consider, for example, the case of a plane electric wave travelling through the ether. We have parallel to the wave front a varying electric polarisation, which on the theory is equivalent to a current; at right-angles to this, and also in the wave-front, we have a magnetic force. Now, when a current flows through a medium in a magnetic field there is a force acting on the medium at right-angles to the plane, which is parallel both to the current and to the magnetic force; there will thus be a mechanical force acting on each unit volume of the ether when transmitting an electric wave, and since this force is at right-angles to the current and to the magnetic force, it will be in the direction in which the wave is propagated. In the electro-magnetic theory of light, however, we assume that this force does not set the ether in motion, as unless we made this assumption we should have to modify our equations, as the electro-magnetic equations are not the same in a moving field as in a field at rest. In fact, a complete discussion of the transmission of electro-magnetic disturbances requires a knowledge of the constitution of the ether which we do not possess. We now assume that the ether is not set in motion by an electro-magnetic wave. If we do not make this assumption we must introduce into our equation quantities representing the components of the velocity of the ether, and unless we know the constitution of the ether, so as to be able to deduce these velocities from the forces acting on it, there will be in the equations of the electro-magnetic field more unknown quantities than we have equations to determine. It is, therefore, a very essential point in electromagnetic theory to investigate whether or not there is any motion of the ether in a varying electro-magnetic field. We have at the Cavendish Laboratory, using Professor Lodge's arrangement of interference fringes, made some experiments to see if we could detect any movement of the ether in the neighbourhood of an electric vibrator, using the spark which starts the vibrations as the source of light. The movement of the ether, if it exists, will be oscillatory, and with an undamped vibrator the average velocity would be zero; we used, therefore, a heavily damped vibrator, with which the average velocity might be expected to be finite The experiments are not complete, but so far the results are entirely negative. We also tried by the same method to see if we could detect any movement of the ether in the neighbourhood of a vacuum-tube emitting Röntgen rays, but could not find any trace of such a movement. Professor Threlfall, who independently tried the same experiment, has, I believe, arrived at the same conclusion.

Unless the ether is immovable under the mechanical forces in a varying electromagnetic field, there are a multitude of phenomena awaiting discovery. If the ether does move, then the velocity of transmission of electrical vibrations, and therefore of light, will be affected by a steady magnetic field. Such a field, even if containing nothing but ether, will behave towards light like a crystal, and the velocity of propagation will depend upon the direction of the rays. A similar result would also hold in a steady electric field. We may hope that experiments on these and similar points may throw some light on the properties of that medium which is universal, which plays so large a part in our explanation of physical phenomena, and of which we know so little.

# British Association for the Advancement of Science.

## LIVERPOOL, 1896.

# ADDRESS

### TO THE

# CHEMICAL SECTION

#### BY

### Dr. LUDWIG MOND, F.R.S.,

#### PRESIDENT OF THE SECTION.

In endeavouring to fix upon a suitable theme for the address I knew you would to-day expect from me, I have felt that I ought to give due consideration to the interests which tie this magnificent city of Liverpool, whose hospitality we enjoy this week, to Section B. of the British Association.

I have therefore chosen to give you a brief history of the manufacture of chlorine, with the progress of which this city and its neighbourhood have been very conspicuously and very honourably connected, not only as regards quantity—I believe this neighbourhood produces to-day nearly as much chlorine as the rest of this world together—but more particularly by having originated, worked out, and carried into practice several of the most important improvements ever introduced into this manufacture. I was confirmed in my choice by the fact that this manufacture has been influenced and perfected in an extraordinary degree by the rapid assimilation and application of the results of purely scientific investigations and of new scientific theories, and offers a very remarkable example of the incalculable value to our commercial interests of the progress of pure science.

The early history of chlorine is particularly interesting, as it played a most important *rôle* in the development of chemical theories. There can be no doubt that the Arabian alchemist Geber, who lived eleven hundred years ago, must have known that 'Aqua Regia,' which he prepared by distilling a mixture of salt, nitre, and vitriol, gave off on heating very corrosive, evil-smelling, greenish-yellow fumes, and all his followers throughout a thousand years must have been more or less molested by these fumes whenever they used Aqua Regia, the one solvent of the gold they attempted so persistently to produce.

But it was not until 1774 that the great Swedish chemist Scheele succeeded in establishing the character of these fumes. He discovered that on heating manganese with muriatic acid he obtained fumes very similar to those given off by 'Aqua Regia,' and found that these fumes constituted a permanent gas of yellowish-green colour, very pungent odour, very corrosive, very irritating to the respiratory organs, and which had the power of destroying organic colouring matters.

According to the views prevalent at the time, Scheele considered that the manganese had removed phlogiston from the muriatic acid, and he consequently called the gas dephlogisticated muriatic acid.

When during the next decade Lavoisier successfully attacked, and after a memorable struggle completely upset the phlogiston theory and laid the foundations of our modern chemistry, Berthollet, the eminent 'father' of physical

chemistry—the science of to-day—endeavoured to determine the place of Scheele's gas in the new theory. Lavoisier was of opinion that all acids, including muriatic acid, contain oxygen. Berthollet found that a solution of Scheele's gas in water, when exposed to the sunlight, gives off oxygen and leaves behind muriatic acid. He considered this as proof that this gas consists of muriatic acid and oxygen, and called it oxygenated muriatic acid.

In the year 1785 Berthollet conceived the idea of utilising the colour-destroying powers of this gas for bleaching purposes He prepared the gas by heating a mixture of salt, manganese, and vitriol. He used a solution of the gas in water for bleaching, and subsequently discovered that the product obtained by absorbing the gas in a solution of caustic potash possessed great advantages in practice.

This solution was prepared as early as 1789, at the chemical works on the Quai de Javelle, in Paris, and is still made and used there under the name of ' Eau de Javelle.'

James Watt, whose great mind was not entirely taken up with that greatest of all inventions—his steam-engine—by which he has benefited the human race more than any other man, but who also did excellent work in chemistry—became acquainted in Paris with Berthollet's process, and brought it to Scotland. Here it was taken up with that energy characteristic of the Scotch, and a great stride forward was made when, in 1798, Charles Tennant, the founder of the great firm, which has only recently lapsed into the United Alkali Company, began to use milk of lime in place of the more costly caustic potash, in making a bleaching liquid; and a still greater advance was made when, in the following year, Tennant proposed to absorb the chlorine by hydrate of lime, and thus to produce a dry substance, since known under the name of bleaching powder, which allowed the bleaching powers of chlorine to be transported to any distance.

In order to give you a conception of the theoretical ideas prevalent at this time, I will read to you a passage from an interesting treatise on the art of beaching published in 1799 by Higgins. In his chapter ' On bleaching with the oxygenated muriatic acid, and on the methods of preparing it,' he explains the theory of the process as follows :—

' Manganese is an oxyd, a metal saturated with oxygen gas. Common salt is composed of muriatic acid and an alkaline salt called soda, the same which barilla affords. Manganese has greater affinity to sulphuric acid than to its oxygen, and the soda of the salt greater affinity to sulphuric acid than to the muriatic acid gas ; hence it necessarily follows that these two gases (or rather their gravitating matter) must be liberated from their former union in immediate contact with each other : and although they have but a weak affinity to one another, they unite in their nascent state, that is to say, before they individually unite to caloric, and separately assume the gaseous state ; for oxygen gas and muriatic acid gas already formed will not unite when mixed, in consequence principally of the distance at which their respective atmospheres of caloric keep their gravitating particles asunder. The compound resulting from these two gases still retains the property of assuming the gaseous state, and is the oxygenated muriatic gas.'

Interesting as these views may appear, considering the time they were published, you will notice that the rôle played by the manganese in the process and the chemical nature of this substance were not at all understood. The law of multiple proportions had not yet been propounded by John Dalton, and the researches of Berzelius on the oxides of manganese were only published thirteen years later, in 1812. The green gas we are considering was still looked upon as muriatic acid, to which oxygen had been added, in contradistinction to Scheele's view, who considered it as muriatic acid, from which something, viz. phlogiston, had been abstracted.

It was Humphry Davy who had, by a series of brilliant investigations carried out in the Laboratory of the Royal Institution between 1808 and 1810, accumulated fact upon fact to prove that the gas hitherto called oxygenated muriatic acid did not contain oxygen. He announced in an historic paper, which he read before the Royal Society on July 12, 1810, his conclusion that this gas was an elementary

body, which in muriatic acid was combined with hydrogen, and for which he proposed the name 'chlorine,' derived from the Greek χλωρός, signifying 'green,' the colour by which the gas is distinguished.

The numerous communications which Humphry Davy made to the Royal Society on this subject form one of the brightest and most interesting chapters in the history of chemistry. They have recently been reprinted by the Alembic Society, and I cannot too highly recommend their study to the young students of our science.

Those who have followed the history of chemistry I need not remind how hotly and persistently Davy's views were combated by a number of the most eminent chemists of his time, led by Berzelius himself; how long the chlorine controversy divided the chemical world; how triumphantly Davy emerged from it, how completely his views were recognised; and how very instrumental they have been in advancing theoretical chemistry.

The hope, however, which Davy expressed in that same historic paper, 'that these new views would perhaps facilitate one of the greatest problems in economical chemistry, the decomposition of the muriates of soda and potash,' was not to be realised so soon. Although it had changed its name, chlorine was still for many years manufactured by heating a mixture of salt, manganese, and sulphuric acid in leaden stills, as before.

This process leaves a residue consisting of sulphate of soda and sulphate of manganese, and for some time attempts were made to recover the sulphate of soda from these residues, and to use it for the manufacture of carbonate of soda by the Le Blanc process. On the other hand, the Le Blanc process, which had been discovered and put into practice almost simultaneously with Berthollet's chlorine process, decomposed salt by sulphuric acid, and sent the muriatic acid evolved into the atmosphere, causing a great nuisance to the neighbourhood.

Naturally, therefore, when Mr. William Gossage had succeeded in devising plant for condensing this muriatic acid, the manufacturers of chlorine reverted to the original process of Scheele, and heated manganese with the muriatic acid thus obtained. Since then the manufacture of chlorine has become a bye-product of the manufacture of soda by the Le Blanc process, and remained so till very recently.

For a great many years the muriatic acid was allowed to act upon native ores of manganese in closed vessels of earthenware or stone, to which heat could be applied, either externally or internally. These native manganese ores, containing only a certain amount of peroxide, converted only a certain percentage of the muriatic acid employed into free chlorine, the rest combining with the manganese and iron contained in the ore, and forming a brown and very acid solution, which it was a great difficulty for the manufacturer to get rid of. Consequently, many attempts were made to regenerate peroxide of manganese from these waste liquors, so as to use it over again in the production of chlorine.

These, however, for a long time remained unsuccessful, because the exact conditions for super-oxydising the protoxide of manganese by means of atmospheric air were not yet known.

Meantime, viz. in 1845, Mr. Dunlop introduced into the works created by his grandfather, Mr. Charles Tennant, at St. Rollox, a new and very interesting method for producing chlorine, which was in a certain measure a return to the process used by the alchemists.

Indeed, the first part of this process consisted in decomposing a mixture of salt and nitre with oil of vitriol—a reaction that had been made use of for so many centuries! The chlorine so obtained is, however, not pure, but a mixture of chlorine with oxides of nitrogen and hydrochloric acid, which Mr. Dunlop had to find means to eliminate.

For separating the nitrous oxides, Mr. Dunlop adopted the method introduced twenty years before by the great Gay-Lussac in connection with vitriol-making, viz. absorption by sulphuric acid, and the nitro-sulphuric acid thus formed he also utilised in the same way as that obtained from the towers which still bear Gay-Lussac's illustrious name, viz. by using it in the vitriol process in lieu of nitric

acid. He then freed his chlorine gas from hydrochloric acid by washing with water, and so obtained it pure. This process possessed two distinct advantages— (1) it yielded a very much larger amount of chlorine from the same amount of salt, and (2) the nitric acid, which was used for oxidising the hydrogen in the hydrochloric acid, was not lost, because the oxides of nitrogen to which it was reduced answered the purpose for which the acid itself had previously been employed. But this process was very limited in its application, as it could only be worked to the extent to which nitric acid was used in vitriol-making.

The process has been at work at St. Rollox for over fifty years, and, as far as I know, is there still in operation; but I am not aware that it has ever been taken up elsewhere.

Within the last few years, however, several serious attempts have been made to give to this process a wider scope by regenerating nitric acid from the nitro-sulphuric acid and employing it over and over again to convert hydrochloric acid into chlorine. Quite a number of patents have been taken out for this purpose, all employing atmospheric air for reconverting the nitrous oxides into nitric acid, and differing mainly in details of apparatus and methods of work, and several of these have been put to practical test on a fairly large scale in this neighbourhood, and also in Glasgow, Middlesbrough, and elsewhere. As I do not want to keep you here the whole afternoon, I have to draw the line somewhere as to what I shall include in this brief history of the manufacture of chlorine, and have had to decide to restrict myself to those methods which have actually attained the rank of manufacturing processes on a large scale. As none of the processes just referred to have attained that position, you will excuse me for not entering into further details respecting them.

Mr. Dunlop's process only produced a very small portion of the chlorine manufactured at that time at St. Rollox, the remainder being made, as before, from native manganese and muriatic acid, leaving behind the very offensive waste liquors I have mentioned before, which increased from year to year, and became more and more difficult to get rid of. The problem of recovering from these liquors the manganese in the form of peroxide Mr. Dunlop succeeded in solving in 1855.

He neutralised the free acid and precipitated the iron present by treating these liquors with ground chalk in the cold and settling out, and in later years, filter-pressing the precipitate, which left him a solution of chloride of manganese, mixed only with chloride of calcium. This was treated with a fresh quantity of milk of chalk, but this time under pressure in closed vessels provided with agitators and heated by steam, under which conditions all the manganese was precipitated as carbonate of manganese. This precipitate was filtered off and well drained, and was then passed on iron trays mounted on carriages through long chambers, in which it was exposed to hot air at a temperature of 300° C., the process being practically made continuous, one tray at the one end being taken out of these chambers, and a fresh tray being put in at the other end. One passage through these chambers sufficed to convert the carbonate of manganese into peroxide, which was used in place of, and in the same way as, the native manganese.

The whole of the residual liquors made at the large works at St. Rollox have been treated by this process with signal success for a long number of years. For a short time the process was discontinued in favour of the Weldon process (of which I have to speak next); but after two years Dunlop's process was taken up again, and to the best of my knowledge it is still in operation to this day. It has, however, just like Mr. Dunlop's first chlorine process, never left the place of its birth (St. Rollox), although it was for a period of over ten years without a rival.

In 1866 Mr. Walter Weldon patented a modification of a process proposed by Mr. William Gossage in 1837 for recovering the manganese that had been used in the manufacture of chlorine. Mr. Gossage had proposed to treat the residual liquors of this manufacture by lime, and to oxidise the resulting protoxide of manganese by bringing it into frequent and intimate contact with atmospheric air. This process—and several modifications thereof subsequently patented—had been tried in various places without success. Mr. Weldon, however, did succeed in obtaining a very satisfactory result, possibly—even probably—because, not

being a chemist, he did not add the equivalent quantity of lime to his liquor to precipitate the manganese, but used an excess. However, Mr. Weldon, if he was not a chemist at that time, was a man of genius and of great perseverance He soon made himself a chemist, and having once got a satisfactory result, he studied every small detail of the reaction with the utmost tenacity until he had thoroughly established how this satisfactory result could be obtained on the largest scale with the greatest regularity and certainty.

He even went further, and added considerably to our theoretical knowledge of the character of manganese peroxide and similar peroxides by putting forward the view that these compounds possess the character of weak acids. He explained in this way the necessity for the presence of an excess of lime or other base if the oxidation of the precipitated protoxide of manganese by means of atmospheric air was to proceed at a sufficiently rapid rate. He pointed out that the product had to be considered as a manganite of calcium, a view which has since been thoroughly proved by the investigations of Goergen and others, and it is only fair to state that Weldon's process is not only a process for recovering the peroxide of manganese originally used, but that he introduced a new substance, viz. manganite of calcium, to be continuously used over and over again in the manufacture of chlorine.

Mr. Weldon had the good fortune that his ideas were taken up with fervency by Colonel Gamble of St. Helens, and that Colonel Gamble's manager, Mr. F. Bramwell, placed all his experience as a consummate technical chemist and engineer at Mr. Weldon's disposal, and assisted him in carrying his ideas into practice The result was that a process which many able men had tried in vain to realise for thirty years became in the hands of Mr. Weldon and his coadjutors within a few years one of the greatest successes achieved in manufacturing chemistry.

The Weldon process commences by treating the residual liquor with ground chalk or limestone, thus neutralising the free acid and precipitating any sulphuric acid and oxide of iron present. The clarified liquor is run into a tall cylindrical vessel, and milk of lime is added in sufficient quantity to precipitate all the manganese in the form of protoxide. An additional quantity of milk of lime, from one-fifth to one-third of the quantity previously used, is then introduced, and air passed through the vessel by means of an air-compressor. After a few hours all the manganese is converted into peroxide; the contents of the vessel are then run off; the mud, now everywhere known as 'Weldon mud,' is settled, and the clear liquor run to waste. The mud is then pumped into large closed stone stills, where it meets with muriatic acid, chlorine is given off, and the residual liquor treated as before.

You note that this process works without any manipulation, merely by the circulation of liquids and thick magmas which are moved by pumping machinery. As compared to older processes it also has the great advantage that it requires very little time for completing the cycle of operations, so that large quantities of chlorine can be produced by a very simple and inexpensive plant. These advantages secured for this process the quite unprecedented success that within a few years it was adopted, with a few isolated exceptions, by every large manufacturer of chlorine in the world; yet it possessed a distinct drawback, viz. that it produced considerably less chlorine from a given quantity of muriatic acid than either native manganese of good quality or Mr. Dunlop's recovered manganese. At that time, however, muriatic acid was produced as a bye-product of the Le Blanc process so largely in excess of what could be utilised that it was generally looked upon as a waste product of no value. Mr. Weldon himself was one of the very few who foresaw that this state of things could not always continue. The ammonia soda process was casting its shadow before it. Patented in 1838 by Messrs. Dyar & Hemming it was only after the lapse of thirty years (during which a number of manufacturing chemists of the highest standing had in vain endeavoured to carry it into practice) that this process was raised to the rank of a manufacturing process through the indomitable perseverance of Mr. Ernest Solvay of Brussels, and his clear perception of its practical and theoretical intricacies. A few years

later, in 1872, Mr. Weldon already gave his attention to the problem of obtaining the chlorine of the salt used in this process in the form of muriatic acid. He proposed to recover the ammonia from the ammonium chloride obtained in this manufacture by magnesia instead of lime, thus obtaining magnesium chloride instead of calcium chloride, and to produce muriatic acid from this magnesium chloride by a process patented by Clemm in 1863, viz. by evaporating the solution, heating the residue in the presence of steam and condensing the acid vapours given off.

Strange to say, this same method had been patented by Mr. Ernest Solvey within twenty-four hours before Mr. Weldon lodged his specification. It has been frequently tried with many modifications, but has never been found practicable. Soon afterwards Mr. Weldon, with the object of reducing the muriatic acid required by his first process, proposed to replace the lime in this process by magnesia, and so to produce a manganite of magnesia. After treating this with muriatic acid and liberating chlorine he proceeded to evaporate the residual liquors to dryness, during which operation all the chlorine they contain would be disengaged as hydrochloric acid and collected in condensers, while the dry residue, after being heated to dull redness in the presence of air, would be reconverted into manganite of magnesia.

This process was made the subject of long and extensive experiments at the works of Messrs. Gamble at St. Helens, but did not realise Mr. Weldon's expectations. It, however, led to some further interesting developments, to which I shall refer later on.

Those of you who were present at the last meeting of the British Association in this city will remember that this Section had the advantage of listening to a paper by Mr. Weldon on his chlorine process, and also to another highly interesting paper by Mr. Henry Deacon of Widnes 'on a new chlorine process without manganese.' And those of you who came with the then President of the Section (Professor Roscoe) to Widnes to visit the works of Messrs. Gaskell, Deacon & Co. will well remember that at these works they saw side by side Weldon's process and Deacon's process in operation, and no one present will have forgotten the thoughtful, flashing eyes and impressive face of Mr. Deacon when he explained to his visitors the theoretical views he had formed as regards his process

Mr Deacon had made a careful study of thermo-chemistry, which had been greatly developed during the preceding decade by the painstaking, accurate, and comprehensive experiments of Julius Thomsen and of Berthelot, and had led the latter to generalisations, which, although not fully accepted by scientific men, have been of immense service to manufacturing chemistry.

Mr. Deacon came to the conclusion that if a mixture of hydrochloric acid with atmospheric air was heated in the presence of a suitable substance capable of initiating the interaction of these two gases by its affinity to both, it would to a very great extent be converted into chlorine with the simultaneous formation of steam, because the formation of steam from oxygen and hydrogen gives rise to the evolution of a considerably larger quantity of heat than the combination of hydrogen and chlorine. Mr. Deacon found that the salts of copper were a very suitable substance for this purpose, and took out a patent for this process in 1868. He entrusted the study of the theoretical and practical problems connected with this process to Dr. Ferdinand Hurter, who carried them out in a manner which will always remain memorable and will never be surpassed, as an example of the application of scientific methods to manufacturing problems, and which soon placed this beautiful and simple process on a sound basis as a manufacturing operation.

In the ordinary course of manufacture the major part—about two-thirds—of the hydrochloric acid is obtained mixed with air and a certain amount of steam, but otherwise very little contaminated. Instead of condensing the muriatic acid from this mixture of gases by bringing it into contact with water, Mr. Deacon passed it through a long series of cooling pipes to condense the steam, which of course absorbed hydrochloric acid, and formed a certain quantity of strong muriatic acid. The mixture of gases was then passed through an iron superheater to raise

it to the required temperature, and thence through a mass of broken bricks impregnated with sulphate or chloride of copper contained in a chamber or cylinder called a decomposer, which was protected from loss of heat by being placed in a brick furnace kept sufficiently hot. In this apparatus from 50 to 60 per cent. of the hydrochloric acid in the mixture of gases was burnt to steam and chlorine. In order to separate this chlorine from the steam and the remaining hydrochloric acid the gases were washed with water, and subsequently with sulphuric acid. The mixture now consisted of nitrogen and oxygen, containing about 10 per cent. of chlorine gas, which could be utilised without any difficulty in the manufacture of bleach liquors and chlorate of potash, and which Mr. Deacon also succeeded in using for the manufacture of bleaching powder, by bringing it into contact in specially constructed chambers with large surfaces of hydrate of lime. Within recent years this latter object has been attained in a more expeditious and perfect manner by continuous mechanical apparatus (of which those constructed by Mr. Robert Hasenclever and Dr. Carl Langer have been the most successful), in which the hydrate of lime is transported in a continuous stream by single or double conveyors in an opposite direction to the current of dilute chlorine, and the bleaching powder formed delivered direct into casks, thereby avoiding the intensely disagreeable work of packing this offensive substance by hand.

Mr. Deacon's beautiful and scientific process thus involves still less movement of materials than the very simple process of Mr. Weldon, because in lieu of large volumes of liquids he only moves a current of gas through his apparatus, which requires a minimum of energy. The only raw material used for converting hydrochloric acid into chlorine is atmospheric air, the cheapest of all at our command. The hydrochloric acid which has not been converted into chlorine by the process is all obtained, dissolved in water, as muriatic acid, and is not lost, as in previous processes, but is still available to be converted into chlorine by other methods, or to be used for other purposes.

In spite of these distinct advantages, this process took a long time before it became adopted as widely as it undoubtedly deserved. This was mainly due to the fact that the economy in the use of muriatic acid which it effected was at the time when the process was brought out, and for many years afterwards, no object to the majority of chlorine manufacturers, who were still producing more of this commodity than they could use. Moreover, there were other reasons. The plant required for this process, although so simple in principle, is very bulky in proportion to the quantity of chlorine produced, and as I have pointed out, the process only succeeded in converting about one-third of the hydrochloric acid produced into chlorine, the remainder being obtained as muriatic acid, which had in most instances to be converted into chlorine by the Weldon process; so that the Deacon process did not constitute an entirely self-contained method for this manufacture. This defect, of small moment as long as muriatic acid was produced in excessive quantities, was only remedied by an invention of Mr. Robert Hasenclever a short number of years ago; when by the rapid development of the ammonia soda process the previously existing state of things had been completely changed, and when, at least on the Continent, muriatic acid was no longer an abundant and valueless bye-product, but, on the contrary, the alkali produced by the Le Blanc process had become a bye-product of the manufacture of chlorine. Mr. Hasenclever, in order to make the whole of the muriatic acid he produces available for conversion into chlorine by the Deacon process, introduces the liquid muriatic acid in a continuous stream into hot sulphuric acid contained in a series of stone vessels, through which he passes a current of air. He thus obtains a mixture of hydrochloric acid and air, well adapted for the Deacon process, the water of the muriatic acid remaining with the sulphuric acid, from which it is subsequently eliminated by evaporation. In this way the chlorine in the hydrochloric acid can be almost entirely obtained in its free state by the simplest imaginable means, and with the intervention of no other chemical agent than atmospheric air. Since their introduction the Deacon process has supplanted the Weldon process in nearly all the largest chlorine works in France and Germany, and is now also making very rapid progress in this country.

Mr. Weldon, when he decided to give up his manganite of magnesia process, by no means relaxed his efforts to work out a chlorine process which should utilise the whole of the muriatic acid. While working with manganite of magnesia he found that magnesia alone would answer the purpose without the presence of the peroxide of manganese. He obtained the assistance of M. Pechiney, of Salindres, and in conjunction with him worked out what has become known as the 'Weldon-Pechiney' process, which was first patented in 1884.

This process consists in neutralising muriatic acid by magnesia, concentrating the solution to a point at which it does not yet give off any hydrochloric acid, and then mixing into it a fresh quantity of magnesia so as to obtain a solid oxychloride of magnesium. This is broken up into small pieces, which are heated up rapidly to a high temperature without contact with the heating medium, while a current of air is passing through them. The oxychloride of magnesium containing a large quantity of water, this treatment yields a mixture of chlorine and hydrochloric acid with air and steam, the same as the Deacon process, and this is treated in a very similar way to eliminate the steam and the acid from the chlorine. The acid condensed is, of course, treated with a fresh quantity of magnesia, so that the whole of the chlorine which it contains is gradually obtained in the free state.

The rapid heating to a high temperature of the oxychloride of magnesium without contact with the heating medium was an extremely difficult practical problem, which has been solved by M. Pechiney and his able assistant, M. Boulouvard, in a very ingenious and entirely novel way.

They lined a large wrought-iron box with fire-bricks, and built inside of this vertical fire-brick walls with small empty spaces between them, thus forming a number of very narrow chambers, so arranged that they could all be filled from the top of the box, and emptied from the bottom. These chambers they heated to a very high temperature by passing a gas flame through them, thus storing up in the brick walls enough heat to carry out and complete the decomposition of the magnesium oxychloride, with which the chamber was filled when hot enough.

Mr. Weldon himself called this apparatus a 'baker's oven,' in which trade certainly the same principle has been employed from time immemorial; but to my knowledge it had never before been used in any chemical industry. This process has been at work at M Pechiney's large alkali works at Salindres, and is now at work in this country at the chlorate of potash works of Messrs. Allbright and Wilson at Oldbury, a manufacture for which it offers special advantages Mr. Weldon and M. Pechiney had expected that this process would become specially useful in connection with the ammonia soda process by preparing in the way proposed by Mr. Solvay and Mr Weldon in 1872 a solution of magnesium chloride as a bye-product of this manufacture; but instead of obtaining muriatic acid from this solution by Clemm's process, to treat it by the new process, so as to obtain the bulk of the chlorine at once in the free state. But M. Pechiney did no more succeed than his predecessors in recovering the ammonia by means of magnesia in a satisfactory way.

Quite recently, however, it has been applied to obtain chlorine in connection with the ammonia soda process by Dr. Pick, of Ozakowa, in Austria. He recovers the ammonia, as usual, by means of lime, and converts the solution of chloride of calcium, obtained by a process patented by Mr. Weldon in 1869, viz. by treatment with magnesia and carbonic acid under pressure, into chloride of magnesium with the formation of carbonate of lime. The magnesium chloride solution is then concentrated and treated by the Weldon-Pechiney process.

I have repeatedly referred during this brief history to the great change which has been brought about in the position of chlorine manufacture by the development of the ammonia soda process, and have pointed out that the muriatic acid which for a long time was the bye-product of the Le Blanc process, without value, thereby became gradually its main and most valuable product, while the alkali became its bye-product.

I have told you how, very early in the history of this process, Mr. Solvay and Mr. Weldon proposed means to provide for this contingency, and how Mr. Weldon continued to improve these means until the time of his death. Mr. Solvay, on his

part, also followed up the subject with that tenacity and sincerity of purpose which distinguishes him , his endeavours being mainly directed to producing chlorine direct from the chloride of calcium running away from his works by mixing it with clay and passing air through the mixture at very high temperatures, thus producing chlorine and a silicate of calcium, which could be utilised in cement-making. The very high temperatures required prevented, however, this process from becoming a practical success.

I have already told you what a complicated series of operations Dr. Pick has lately resorted to in order to obtain the chlorine from this chloride of calcium. Yet the problem of obtaining chlorine as a bye-product of the ammonia soda process presents itself as a very simple one.

This process produces a precipitate of bicarbonate of soda and a solution of chloride of ammonium by treating natural brine or an artificially made solution of salt, in which a certain amount of ammonia has been dissolved, with carbonic acid. In their original patent of 1838 Messrs. Dyar & Hemming proposed to evaporate this solution of ammonium chloride and to distil the resulting dry product with lime to recover the ammonia. Now all that seemed to be necessary to obtain the chlorine from this ammonium chloride was to substitute another oxide for lime in the distillation process, which would liberate the ammonia and form a chloride which on treatment with atmospheric air would give off its chlorine and reproduce the original oxide. The whole of the reactions for producing carbonate of soda and bleaching powder from salt would thus be reduced to their simplest possible form ; the solution of salt, as we obtain it in the form of brine direct from the soil, would be treated with ammonia and carbonic acid to produce bicarbonate and subsequently monocarbonate of soda, the limestone used for producing the carbonic acid would yield the lime required for absorbing the chlorine, and produce bleaching powder instead of being run into the rivers in combination with chlorine in the useless form of chloride of calcium, and both the ammonia used as an intermediary in the production of soda and the metallic oxide used as an intermediary in the production of chlorine would be continuously recovered.

The realisation of this fascinating problem has occupied me for a great many years. In the laboratory I obtained soon almost theoretical results. A very large number of oxides and even of salts of weak acids were found to decompose ammonium chloride in the desired way ; but the best results (as was to be clearly anticipated from thermo-chemical data) were given by oxide of nickel.

When, however, I came to carry this process out on a large scale, I met with the most formidable difficulties, which it took many years to overcome successfully.

The very fact that ammonium chloride vapour forms so readily metallic chlorides when brought in contact at an elevated temperature with metals or oxides or even silicates, led to the greatest difficulty, viz. that of constructing apparatus which would not be readily destroyed by it.

Amongst the metals we found that platinum and gold were the only ones not attacked at all. Antimony was but little attacked, and nickel resisted very well if not exposed to too high a temperature, so that it could be, and is being, used for such parts of the plant as are not directly exposed to heat. The other parts of the apparatus coming in contact with the ammonium chloride vapour I ultimately succeeded in constructing of cast and wrought iron, lined with fire-bricks or Doulton tiles, the joints between these being made by means of a cement consisting of sulphate of baryta and waterglass.

After means had been devised for preventing the breaking of the joints through the unequal expansion of the iron and the earthenware, the plant so constructed has lasted very well.

Oxide of nickel, which had proved the most suitable material for the process in the laboratory, gave equally good chemical results on the large scale, but occasionally a small quantity of nickel chloride was volatilised through local over-heating, which, however, was sufficient to gradually make up the chlorine conduits. We therefore looked out for an active material free from this objection. Theoretical considerations indicated magnesia as the next best substance, but it was found that the magnesium chloride formed was not anhydrous, but retained a certain amount

of the steam formed by the reaction, which gave rise to the formation of a con-
siderable quantity of hydrochloric acid on treatment with hot air. In conjunction
with Dr. Eschellman (who carried out the experiments for me), I succeeded in
reducing the quantity of this hydrochloric acid to a negligible amount by adding
to the magnesia a certain amount of chloride of potassium, which probably has the
effect of forming an anhydrous double chloride.

This mixture of magnesia and potassium chloride is, after the addition of a
certain quantity of china clay, made into small pills in order to give a free and
regular passage throughout their entire mass to the hot air and other gases with
which they have to be treated. In order to avoid as far as possible the handling
and consequent breaking of these pills, I vapourise the ammonium chloride in a
special apparatus, and take the vapours through these pills and subsequently pass
hot air through. and then again ammonium chloride vapour, and so on, without
the pills changing their place.

The vapourisation of the ammonium chloride is carried out in long cast-iron
retorts lined with thin Doulton tiles, and placed almost vertically in a furnace
which is kept by producer gas at a very steady and regular temperature. These
retorts are kept nearly full with ammonium chloride, so as to have as much active
heating surface as possible. From time to time a charge of ammonium chloride is
introduced through a hopper at the top of these retorts, which is closed by a nickel
plug. The ammonium chloride used is very pure, being crystallised out from its
solution as produced in the ammonia soda manufacture by a process patented by
Mr. Gustav Jarmay, which consists in lowering the temperature of these solutions
considerably below 0° C. by means of refrigerating machinery. The retorts will
therefore evaporate a very large amount of ammonium chloride before it becomes
necessary to take out through a door at their bottom the non-volatile impurities which
accumulate in them. The ammonium chloride vapour is taken from these retorts
by cast-iron pipes lined with tiles and placed in a brick channel, in which they are kept
hot, to prevent the solidification of the vapour, to large upright wrought-iron cylin-
ders which are lined with a considerable thickness of fire-bricks, and are filled with
the magnesia pills, which are, from the previous operations, left at a temperature
of about 300° C. On its passage through the pills the chlorine in the vapours is
completely retained by them, the ammonia and water vapour formed pass on and
are taken to a suitable condensing apparatus. The reaction of the ammonium
chloride vapour upon magnesia being exo-thermic, the temperature of the pills
rises during this operation, and no addition of heat is necessary to complete it.
The temperature, however, does not rise sufficiently to satisfactorily complete the
second operation, viz. the liberation of the chlorine and the re-conversion of the
magnesium chloride into magnesium oxide by means of air. This reaction is
slightly endo-thermic, and thus absorbs a small amount of heat, which has to be
provided in one way or another. I effect this by heating the pills to a somewhat
higher temperature than is required for the action of the air upon them, viz. to
600° C., by passing through them a current of a dry inert gas free from oxygen
heated by a Siemens-Cowper stove to the required temperature. I use for this
purpose the gas leaving the carbonating plant of the ammonia soda process.

This current of gas also carries out of the apparatus the small amount of
ammonia which was left in between the pills. It is washed to absorb this
ammonia, and after washing, this same gas is passed again through the Siemens-
Cowper stove, and thus constantly circulated through the apparatus, taking up the
heat from the stove and transferring it to the pills. When these have attained the
required temperature, the hot inert gas is stopped and a current of hot air passed
through, which has also been heated to 600° C. in a similar stove. The air acts
rapidly upon the magnesium chloride, and leaves the apparatus charged with 18 to
20 per cent. of chlorine and a small amount of hydrochloric acid. The chlorine
comes gradually down, and when it has reached about 3 per cent. the temperature
of the air entering the apparatus is lowered to 350° C. by the admixture of cold
air to the hot air from the stove ; and the weak chlorine leaving the apparatus is
passed through a second stove, in which its temperature is raised again to 600° C.,
and passed into another cylinder full of pills which are just ready to receive the

hot-air current. A series of four cylinders is required to procure the necessary continuity for the process.

The chlorine gas is washed with a strong solution of chloride of calcium, which completely retains all the hydrochloric acid, and is then absorbed in an apparatus invented by Dr. Carl Langer, by hydrate of lime, which is made to pass by a series of interlocked transporting twin-screws in an opposite direction to the current of gas, and produces very good and strong bleaching powder, in spite of the varying strength of the chlorine gas. The hydrochloric acid absorbed by the solution of calcium chloride can by heating this solution be readily driven out and collected.

This process has now been in operation on a considerable scale at our Works at Winnington for several years, with constantly improving results, notably with regard to the loss of ammonia, which has gradually been reduced to a small amount. The process has fully attained my object, viz. to enable the ammonia soda process to compete, not only in the production of carbonate of soda, but also in the production of bleaching powder, with the Le Blanc process.

Nevertheless. I have hesitated to extend this process as rapidly as I should otherwise have done, because very shortly after I had overcome all its difficulties, entirely different methods from those hitherto employed for the manufacture of chlorine were actively pushed forward in different parts of the globe, for which great advantages were claimed, but the real importance and capabilities of which were and are up to this date very difficult to judge. I refer to the processes for producing chlorine by electrolysis.

During the first decade of this century, Humphry Davy had by innumerable experiments established all the leading facts concerning the decomposing action of an electric current upon chemical compounds. Amongst these he was the first to discover that solutions of alkaline chlorides, when submitted to the action of a current, yield chlorine. His successor at the Royal Institution, Michael Faraday, worked out and proved the fundamental law of electrolysis, known to everybody as 'Faraday's Law,' which has enabled us to calculate exactly the amount of current required to produce by electrolysis any definite quantity of chlorine. Naturally, since these two eminent men had so clearly shown the way, numerous inventors have endeavoured to work out processes based on these principles for the production of chlorine on a manufacturing scale, but only during the last few years have these met with any measure of success.

It has taken all this time for the classical work of Faraday on electro-magnetism to develop into the modern magneto-electric machine, capable of producing electricity in sufficient quantity to make it available for chemical operations on a large scale; for you must keep in mind that an electric installation sufficient to light a large town will only produce a very moderate quantity of chemicals.

In applying electricity to the production of chlorine, various ways have been followed, both as to the raw materials and as to the apparatus employed. While most inventors have proposed to electrolyse a solution of chloride of sodium, and to produce thereby chlorine and caustic soda, I am not aware that up to this day any quantity of caustic soda made by electrolysis has been put on to the market.

Only two electrolytic works producing chlorine on a really large scale are in operation to-day. Both electrolyse chloride of potassium, producing as a bye-product caustic potash, which is of very much higher value than caustic soda, and of which a larger quantity is obtained for the same amount of current expended. These works are situated in the neighbourhood of Stassfurt, the important centre of the chloride of potassium manufacture. The details of the plant they employ are kept secret, but it is known that they use cells with porous diaphragms of special construction, for which great durability is claimed. There are at this moment a considerable number of smaller works in existence, or in course of erection in various countries, intended to carry into practice the production of chlorine by electrolysis by numerous methods, differing mainly in the details of the cells to be used; but some of them also involving what may be called new principles. The most interesting of these are the processes in which mercury is used alternately as cathode and anode, and salt as electrolyte. They aim at obtaining

in the first instance chlorine and an amalgam of sodium, and subsequently converting the latter into caustic soda by contact with water, which certainly has the advantage of producing a very pure solution of caustic soda. Mr. Hamilton Castner has carried out this idea most successfully by a very beautiful decomposing cell, which is divided into various compartments, and so arranged that by slightly rocking the cell the mercury charged with sodium in one compartment passes into another, where it gives up the sodium to water, and then returns to the first compartment, to be recharged with sodium. His process has been at work on a small scale for some time at Oldbury near Birmingham, and works for carrying it out on a large scale are now being erected on the banks of the Mersey, and also in Germany and America.

Entirely different from the foregoing, but still belonging to our subject, are methods which propose to electrolyse the chlorides of heavy metals (zinc, lead, copper, &c.) obtained in metallurgical operations or specially prepared for the purpose, among which the processes of Dr. Carl Hoepfner deserve special attention. They eliminate from the electrolyte immediately both the products of electrolysis, chlorine on one side and zinc and copper on the other, and thus avoid all secondary reactions, which have been the great difficulty in the electrolysis of alkaline chlorides.

All these processes have, however, still to stand the test of time before a final opinion can be arrived at as to the effect they will have upon the manufacture of chlorine, the history of which we have been following, and this must be my excuse for not going into further details. I have endeavoured to give you a brief history of the past of the manufacture of chlorine, but I will to-day not attempt to deal with its future! Yet I cannot leave my subject without stating the remarkable fact that every one of these processes which I have described to you is still at work to this day, even those of Scheele and Berthollet, all finding a sphere of usefulness under the widely varying conditions under which the manufacture of chlorine is carried on in different parts of the world.

Let me express a hope that a hundred years hence the same will be said of the processes now emerging and the processes still to spring out of the inventor's mind. Rapid and varied as has been the development of this manufacture, I cannot suppose that its progress is near its end, and that Nature has revealed to us all her secrets as to how to procure chlorine with the least expenditure of trouble and energy. I do not believe that industrial chemistry will in future be diverted from this Section and have to wander to Section A. under the ægis of applied electricity. I do not believe that the easiest way of effecting chemical changes will ultimately be found in transforming heat and chemical affinity into electricity, tearing up chemical compounds by this powerful medium, and then to recombine their constituents in such form as we may require them. I am sure there is plenty of scope for the manufacturing chemist to solve the problems before him by purely chemical means, of some of which we may as little dream to-day as a few years ago it could have been imagined that nickel would be extracted from its ores by means of carbon-monoxide.

At a meeting of this Association which brings before us an entirely new form of energy, the Röntgen rays, which have enabled us to see through doors and walls and to look inside the human body; which brings before us a new form of matter, represented by Argon and Helium, which, as their discoverers, Lord Rayleigh and Professor Ramsay, have now abundantly proved, are certainly elementary bodies, inasmuch as they cannot be split up further, but are not chemical elements, as they possess no chemical affinity and do not enter into combinations—at a meeting at which such astounding and unexpected secrets of nature are revealed to us, who would call in doubt that, notwithstanding the immense progress pure and applied science have made during this century, new and greater and farther-reaching discoveries are still in store for ages to come?

# British Association for the Advancement of Science.

## LIVERPOOL 1896

# ADDRESS

### TO THE

# GEOLOGICAL SECTION

### BY

### J. E. MARR, M.A., F.R.S., Sec. G.S.,

#### PRESIDENT OF THE SECTION.

THE feelings of one who, being but little versed in the economic applications of his science, is called upon to address a meeting of the Association held in a large industrial centre, might, under ordinary circumstances, be of no very pleasant character; but I take courage when I remember that those connected with my native county, in which we are now gathered, have taken prominent part in advancing branches of our science which are not directly concerned with industrial affairs. I am reminded, for instance, that one amongst you, himself a busy professional man, has in his book on 'The Origin of Mountain Ranges' given to the world a theoretical work of the highest value; that, on the opposite side of the county, those who are responsible for the formation and management of that excellent educational institution, the Ancoats Museum, have wisely recognised the value of some knowledge of geology as a means of quickening our appreciation of the beauties of Nature; and that one who has done solid service to geology by his teachings, who has kept before us the relationship of our science to that which is beautiful—I refer to the distinguished author of 'Modern Painters'—has chosen the northern part of the county for his home, and has illustrated his teaching afresh by reference to the rocks of the lovely district around him. Nor can I help referring to one who has recently passed away—the late Sir Joseph Prestwich—the last link between the pioneers of our science and the geologists of the present day, who, though born in London, was of Lancashire family, and whom we may surely therefore claim as one of Lancashire's worthies. With these evidences of the catholicity of taste on the part of geologists connected with the county, I feel free to choose my own subject for this address, and, my time being occupied to a large extent with academic work, I may be pardoned for treating that subject in academic fashion. As I have paid considerable attention to the branch of the science which bears the somewhat uncouth designation of stratigraphical geology, I propose to take the present state of our knowledge of this branch as my theme.

Of the four great divisions of geology, petrology may be claimed as being largely of German origin, the great impetus to its study having been given by Werner and his teachings. Palæontology may be as justly claimed by the French nation, Cuvier having been to so great an extent responsible for placing it upon a scientific basis. Physical geology we may partly regard as our own, the principles laid down by Hutton and supported by Playfair having received illustration from a host of British writers, amongst whom may be mentioned Jukes, Ramsay, and

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the brothers Geikie ; but the grand principles of physical geology have been so largely illustrated by the magnificent and simple features displayed on the other side of the Atlantic, that we may well refer to our American brethren as leaders in this branch of study. The fourth branch, stratigraphical geology, is essentially British as regards origin, and, as everyone is aware, its scientific principles were established by William Smith, who was not only the father of English geology, but of stratigraphical geology in general.

Few will deny that stratigraphical geology is the highest branch of the science, for, as has been well said, it 'gathers up the sum of all that is made known by the other departments of the science, and makes it subservient to the interpretation of the geological history of the earth ' The object of the stratigraphical geologist is to obtain information concerning all physical, climatic, and biological events which have occurred during each period of the past, and to arrange them in chronological order, so as to write a connected history of the earth. If all of this information were at our disposal, we could write a complete earth-history, and the task of the geologist would be ended. As it is, we have barely crossed the threshold of discovery, and the 'imperfection of the geological record,' like the 'glorious un- certainty ' of our national game, gives geology one of its great charms. Before passing on to consider more particularly the present state of the subject of our study, a few remarks upon this imperfection of the geological record may not be out of place, seeing that the term has been used by so many modern writers, and its exact signification occasionally misunderstood. The imperfection of the palæontological record is usually understood by the term when used, and it will be considered here as an illustration of the incompleteness of our knowledge of earth- history ; but it must be remembered that the imperfection of the physical record is equally striking, as will be insisted on more fully in the sequel.

Specially prominent amongst the points upon which we are ignorant stands the nature of the Precambrian faunas The extraordinary complexity of the earliest known Cambrian fauna has long been a matter for surprise, and the recent dis- coveries in connection with the *Olenellus* fauna do not diminish the feeling.[1] After commenting upon the varied nature of the earliest known fauna, the late Professor Huxley, in his Address to the Geological Society in 1862, stated that ' any admissible hypothesis of progressive modification must be compatible with persistence without progression, through indefinite periods. . . . Should such an hypothesis eventually be proved to be true, . . . the conclusion will inevitably present itself, that the Palæozoic, Mesozoic, and Cainozoic faunæ and floræ, taken together, bear somewhat the same proportion to the whole series of living beings which have occupied this globe, as the existing fauna and flora do to them.' Whether or not this estimate is correct, all geologists will agree that a vast period of time must have elapsed before the Cambrian period, and yet our ignorance of faunas existing prior to the time when the *Olenellus* fauna occupied the Cambrian seas is almost complete. True, many Precambrian fossils have been described at various times, but, in the opinion of many competent judges, the organic nature of each one of these requires confirmation. I need not, however, enlarge upon this matter, for I am glad to say we have amongst us a geologist who will at a later stage read a paper before this Section upon the subject of Precambrian fossils, and there is no one better able, owing to his intimate acquaintance with the actual relics, to present fairly and impartially the arguments which have been advanced in favour of the organic origin of the objects which have been appealed to as evidences of organisms of Precambrian age than our revered co-worker from Canada, Sir J. William Dawson We may look forward with confidence to the future discovery of many faunas older than those of which we now possess certain

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[1] Dr C D Walcott, in his monograph on 'The Fauna of the Lower Cambrian or Olenellus Zone ' (Washington 1890), records the following great groups as represented in the Olenellus beds of America:—Spongiæ, Hydrozoa, Actinozoa, Echinodermata, Annelida ? (trails, burrows, and tracks), Brachiopoda, Lamellibranchiata, Gasteropoda, Pteropoda, Crustacea, and Trilobita. Others are known as occurring in beds of the same age in the Old World

and the biologist must not look to the geologist for direct information concerning the dawn of life upon the earth.

Proceeding now to a consideration of the faunas of the rocks formed after Precambrian times, a rough test of the imperfection of the record may be made by examining the gaps which occur in the vertical distribution of forms of life. If our knowledge of ancient faunas were very incomplete, we ought to meet with many cases of recurrence of forms after their apparent disappearance from intervening strata of considerable thickness, and many such cases have actually been described by that eminent palæontologist, M. Barrande, amongst the Palæozoic rocks of Bohemia, though even these are gradually being reduced in number owing to recent discoveries; indeed, in the case of the marine faunas, marked cases of recurrence are comparatively rare, and the occurrence of each form is generally fairly unbroken from its first appearance to its final extinction, thus showing that the imperfection of the record is by no means so marked as might be supposed. Freshwater and terrestrial forms naturally furnish a large percentage of cases of recurrence, owing to the comparative rarity with which deposits containing such organisms are preserved amongst the strata.

A brief consideration of the main reasons for the present imperfection of our knowledge of the faunas of rocks formed subsequently to Precambrian times may be useful, and suggestive of lines along which future work may be carried out. That detailed work in tracts of country which are yet unexplored, or have been but imperfectly examined by the geologist, will add largely to our stock of information, needs only to be mentioned; the probable importance of work of this kind in the future may be inferred from a consideration of the great increase of our knowledge of the Permo-Carboniferous faunas, as the result of recent labours in remote regions. It is specially desirable that the ancient faunas and floras of tropical regions should be more fully made known, as a study of these will probably throw considerable light upon the influence of climate upon the geographical distribution of organisms in past times. The old floras and faunas of Arctic regions are becoming fairly well known, thanks to the zeal with which the Arctic regions have been explored. But, confining our attention to the geology of our own country, much remains to be done even here, and local observers especially have opportunities of adding largely to our stock of knowledge, a task they have performed so well in the past. To give examples of the value of such work, our knowledge of the fauna of the Cambrian rocks of Britain is largely due to the present President of the Geological Society, when resident at St. David's, whilst the magnificent fauna of the Wenlock limestone would have been far less perfectly known than it is, if it were not for the collections of men like the late Colonel Fletcher and the late Dr. Grindrod. Again, the existence of the rich fauna of the Cambridge Greensand would have been unsuspected, had not the bed known by that name been worked for the phosphatic nodules which it contains.

It is very desirable that large collections of varieties of species should be made, for in this matter the record is very imperfect. There has been, and, I fear, is still, a tendency to reject specimens when their characters do not conform with those given in specific descriptions, and thus much valuable material is lost. Local observers should be specially careful to search for varieties, which may be very abundant in places where the conditions were favourable for their production, though rare or unknown elsewhere. Thus, I find the late Mr. W. Keeping remarking that 'it is noteworthy that at Upware, and indeed all other places known to me, the species of *Brachiopoda* [of the *Neocomian* beds] maintain much more distinctness and isolation from one another than at Brickhill.' [1] The latter place appears to be one where conditions were exceptionally favourable in *Neocomian* times for the production of intermediate forms.

A mere knowledge of varieties is, however, of no great use to the collector without a general acquaintance with the morphology of the organisms whose remains he extracts from the earth's strata, and one who has this can do signal

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[1] W. Keeping, Sedgwick Essay : *The Fossils and Palæontological Affinities of the Neocomian Deposits of Upware and Brickhill.* Cambridge, 1883.

service to the science. It is specially important that local observers should be willing to devote themselves to the study of particular groups of organisms, and to collect large suites of specimens of the group they have chosen for study. With a group like the graptolites, for instance, the specimens which are apparently best preserved are often of little value from a morphological point of view, and fragments frequently furnish more information than more complete specimens. These fragments seldom find their way to our museums, and accordingly we may examine a large suite of graptolites in those museums without finding any examples showing particular structures of importance, such as the sac-like bodies carried by many of these creatures. As an illustration of the value of work done by one who has made a special study of a particular group of organisms, I may refer to the remarkable success achieved by the late Mr. Norman Glass in developing the calcareous supports of the brachial processes of Brachiopods. Work of this character will greatly reduce the imperfection of the record from the biologists' point of view.

The importance of detailed work leads one to comment upon the general methods of research which have been largely adopted in the case of the stratified rocks. The principle that strata are identifiable by their included organisms is the basis of modern work, as it was of that which was achieved by the father of English Geology, and the identification of strata in this manner has of recent years been carried out in very great detail, notwithstanding the attempt on the part of some well-known writers to show that correlation of strata in great detail is impossible. The objection to this detailed work is mainly founded upon the fact that it must take time for an organism or group of organisms to migrate from one area to another, and therefore it was stated that they cannot have lived contemporaneously in two remote areas. But the force of this objection is practically done away with if it can be shown that the time taken for migration is exceedingly short as compared with the time of duration of an organism or group of organisms upon the earth, and this has been shown in the only possible way—namely, by accumulating a very great amount of evidence as the result of observation. The eminent writers referred to above, who were not trained geologists, never properly grasped the vast periods of time which must have elapsed during the occurrence of the events which it is the geologist's province to study. An historian would speak of events which began at noon on a certain day and ended at midnight at the close of that day as contemporaneous with events which commenced and ended five minutes later, and this is quite on a par with what the geologist does when correlating strata. Nevertheless, there are many people who still view the task of correlating minute subdivisions of stratified systems with one another, with a certain amount of suspicion, if not with positive antipathy; but the work must be done for all that. Brilliant generalisations are attractive as well as valuable, but the steady accumulation of facts is as necessary for the advancement of the science as it was in the days when the Geological Society was founded, and its members applied themselves 'to multiply and record observations, and patiently to await the result at some future period.' I have already suggested a resemblance between geology and cricket, and I may be permitted to point out that just as in the game the free-hitter wins the applause, though the patient 'stone-waller' often wins the match, so, in the science, the man apt at brilliant generalisations gains the approval of the general public, but the patient recorder of apparently insignificant details adds matter of permanent value to the stores of our knowledge. In the case of stratigraphical geology, if we were compelled to be content with correlation of systems only, and were unable to ascertain which of the smaller series and stages were contemporaneous, but could only speak of these as 'homotaxial,' we should be in much the same position as the would-be antiquary who was content to consider objects fashioned by the Romans as contemporaneous with those of mediæval times. Under such circumstances geology would indeed be an uncertain science, and we should labour in the field, knowing that a satisfactory earth-history would never be written. Let us hope that a brighter future is in store for us, and let me urge my countrymen to continue to study the minute subdivisions of the strata, lest they be left behind by

the geologists of other countries, to whom the necessity for this kind of study is apparent, and who are carrying it on with great success.

The value of detailed work on the part of the stratigraphical geologist is best grasped if we consder the recent advance that has been made in our science owing to the more or less exhaustive survey of the strata of various areas, and the application of the results obtained to the elucidation of earth's history  A review of this nature will enable us not only to see what has been done, but also to detect lines of inquiry which it will be useful to pursue in the future ; but it is obvious that the subject is so wide that little more can be attempted than to touch lightly upon some of the more prominent questions.  A work might well be written treating of the matters which I propose to notice.  We have all read our ' Principles of Geology,' or 'The Modern Changes of the Earth and its Inhabitants considered as illustrative of Geology,' to quote the alternative title ; some day we may have a book written about the ancient changes of the earth and its inhabitants considered as illustrative of geography.

Commencing with a glance at the light thrown on inorganic changes by a detailed examination of the strata, I may briefly allude to advances which have recently been made in the study of denudation.  The minor faults, which can only be detected when the small subdivisions of rock-groups are followed out carefully on the ground, have been shown to be of great importance in defining the direction in which the agents of denudation have operated, as demonstrated by Professor W. C. Brogger, for instance, in the case of the Christiania Fjord ; [1] and I have recently endeavoured to prove that certain valleys in the English Lake District have been determined by shattered belts of country, the existence of which is shown by following thin bands of strata along their outcrop.  The importance of the study of the strata in connection with the genesis and subsequent changes of river-systems is admirably brought out in Professor W. M Davis's paper on ' The Development of certain English Rivers,' [2] a paper which should be read by all physical geologists ; it is, indeed, a starting-point for kindred work which remains especially for local observers to accomplish.  Study of this kind not only adds to our knowledge of the work of geological agencies, but helps to diminish the imperfection of the record, for the nature of river-systems, when rightly understood, enables us to detect the former presence of deposits over areas from which they have long since been removed by denudation.

An intimate acquaintance with the lithological characters of the strata of a district affords valuable information in connection with the subject of glacial denudation.  The direction of glacial transport over the British Isles has been largely inferred from a study of the distribution of boulders of igneous rock, whilst those of sedimentary rock have been less carefully observed.  The importance of the latter is well shown by the work which has been done in Northern Europe in tracing the Scandinavian boulders to their sources, a task which could not have been performed successfully if the Scandinavian strata had not been studied in great detail [3]  I shall presently have more to say with regard to work connected with the lithological characters of the sediments.  Whilst mentioning glacial denudation, let me allude to a piece of work which should be done in great detail, though it is not, strictly speaking, connected with stratigraphy, namely, the mapping of the rocks around asserted ' rock-basins.'  I can find no actual proof of the occurrence of such basins in Britain, and it is very desirable that the solid rocks and the drift should be carefully inserted on large-scale maps, not only all around the shores of several lakes, but also between the lakes and the sea, in order to ascertain whether the lakes are really held in rock-basins.  Until this work

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[1] W. C Brogger, *Nyt. Mag for Naturvidensk.* vol. xxx. (1886), p 79.

[2] W M Davis, *Geograph. Journ* vol v. (1895) p. 127.

[3] It is desirable that the boulders of sedimentary rock imbedded in the drifts of East Anglia should be carefully examined and fossils collected from them.  The calcareous strata associated with the Alum Shales of Scandinavia and the strata of the Orthoceras-Limestone of that region may be expected to be represented amongst the boulders.

is done, however probable the occurrence of rock-basins in Britain may he considered to be, their actual existence cannot be accepted as proved.

When referring to the subject of denudation, mention was made a moment ago of the study of the lithological character of the sediments. Admirable work in this direction was carried out years ago by one who may be said to have largely changed the direction of advance of geology in this country owing to his researches 'On the Microscopical Structure of Crystals, indicating the Origin of Minerals and Rocks' I refer, of course, to Dr. H. C. Sorby. But since our attention has been so largely directed to petrology, the study of the igneous and metamorphic rocks has been most zealously pursued, whilst that of the sediments has been singularly little heeded, with few exceptions, prominent amongst which is the work of Mr. Maynard Hutchings, the results of which have been recently published in the 'Geological Magazine,' though we must all hope that the details which have hitherto been supplied to us, valuable as they are, are only a foretaste of what is to follow from the pen of this able observer Descriptions of the lithological changes which occur in a vertical series of sediments, as well as of those which are observed when any particular band is traced laterally, will no doubt throw light upon a number of interesting questions.

Careful work amongst the ancient sediments, especially those which are of organic origin, has strikingly illustrated the general identity of characters, and therefore of methods of formation, of deposits laid down on the sea-floors of past times and those which are at present in course of construction Globigerine-oozes have been detected at various horizons and in many countries. Professor H. Alleyne Nicholson [1] has described a pteropod-ooze of Devonian age in the Hamilton Limestone of Canada, which is largely composed of the tests of *Styliola*: and to Dr. G. J. Hinde we owe the discovery of a large number of radiolarian cherts of Palæozoic and Neozoic ages in various parts of the globe. The extreme thinness of many argillaceous deposits, which are represented elsewhere by hundreds of feet of strata, suggests that some of them, at any rate, may be analogous to the deep-sea clays of modern oceans, though in the case of deposits of this nature we must depend to a large extent upon negative evidence. The uniformity of character of thin marine deposits over wide areas is in itself evidence of their formation at some distance from the land ; but although the proofs of origin of ancient sediments far from coast-lines may be looked upon as permanently established, the evidence for their deposition at great depths below the ocean's surface might be advantageously increased in the case of many of them. The fairly modern sediments, containing genera which are still in existence, are more likely to furnish satisfactory proofs of a deep-sea origin than are more ancient deposits. Thus the existence of *Archæopneustes* and *Cystechinus* in the oceanic series of Barbadoes, as described by Dr. J. G Gregory, furnishes strong proofs of the deep-sea character of the deposits, whilst the only actual argument in favour of the deep-sea character of certain Palæozoic sediments has been put forward by Professor Suess, who notes the similarity of certain structures of creatures in ancient rocks to those possessed by modern deep-sea crustacea, especially the co-existence of trilobites which are blind with those which have enormously developed eyes.

A question which has been very prominently brought to the fore in recent years is that of the mode of formation of certain coral-reefs. The theory of Charles Darwin, lately so widely accepted as an explanation of the mode of formation of barrier-reefs and atolls, has been, as is well known, criticised by Dr. Murray, with the result that a large number of valuable observations have been recently made on modern reefs, especially by biologists, as a contribution to the study of reef formation Nor have geologists been inactive Dr. E. Mojsisovics and Professor Dupont, to mention two prominent observers, have described knoll-like masses of limestone more or less analogous, as regards structure, to modern coral-reefs. They consider that these have been formed by corals, and indeed Dupont maintains that the atoll-shape is still recognisable in ancient Devonian

Nicholson and Lydekker, *Manual of Palæontology*, chap. ii.

coral-reefs in Belgium [1]  I would observe that all cases of 'knoll-reefs' of this
character have been described in districts which furnish proofs of having been
subjected to considerable orogenic disturbance, subsequent to the formation of the
rocks composing the knoll-shaped masses, whilst in areas which have not been
affected by violent earth-foldings, the reef-building corals, so far as I have been
able to ascertain, give rise to sheet-like masses, such as should be produced accord-
ing to Dr. Murray's theory.   I would mention especially the reefs of the Corallian
Rocks of England, and also some admirable examples seen amongst the Carbo-
niferous Limestone strata of the great western escarpment of the Pennine Chain
which faces the Eden Valley in the neighbourhood of Melmerby in Cumberland.
Considering the number of dissected coral-reefs which exist amongst the strata of
the earth's crust, and the striking way in which their structure is often displayed,
it is rather remarkable that comparatively little attention has been paid to them
by geologists in general, when the subject has been so prominently brought before
the scientific world, for we must surely admit that we are much more likely to gain
important information, shedding light upon the methods of reef-formation, by a
study of such dissected reefs, than by making a few bore-holes on some special
coral island.   I would specially recommend geologists to make a detailed study of
the British coral-reefs of Silurian, Devonian, Carboniferous, and Jurassic ages.

Turning now to organic deposits of vegetable origin, we must, as the result of
detailed work, be prepared to admit the inapplicability of any one theory of the
formation of coal seams.   The 'growth-in-place' theory may be considered fairly
well established for some coals, such as the spore-coals, whilst the 'drift' theory
furnishes an equally satisfactory explanation of the formation of cannel-coal.
It is now clear that the application of the general term *coal* to a number of
materials of diverse nature, and probably of diverse origin, was largely responsible
for the dragging-out of a controversy, in which the champions of either side
endeavoured to explain the origin of all coal in one particular way.

The stratigraphical geologist, attempting to restore the physical geography of
former periods, naturally pays much attention to the positions of ancient coast-
lines ; indeed, all teachers find it impossible to give an intelligible account of the
stratified rocks without some reference to the distribution of land and sea at the
time of their formation.   The general position of land-masses at various times has
been ascertained in several parts of the world, but much more information must be
gathered together before our restorations of ancient sea-margins approximate to
the truth.   The Carboniferous rocks of Britain have been specially studied with
reference to the distribution of land and water during the period of their accumu-
lation, and yet we find that owing to the erroneous identification of certain rocks
of Devonshire as grits or sandstones, which Dr. Hinde has shown to be radiolarian
cherts, land was supposed to lie at no great distance south of this region in Lower
Carboniferous times, whereas the probabilities are in favour of the existence of an
open ocean at a considerable distance from any land in that direction.   This case
furnishes us with an excellent warning against generalisation upon insufficient
data.

As a result of detailed study of the strata, the effects of earth-movements
have been largely made known to us, especially of those comparatively
local disturbances spoken of as orogenic, which are mainly connected with
mountain-building, whilst information concerning the more widely spread
epeirogenic movements is also furnished by a study of the stratified rocks.   The
structure of the Alps, of the North-West Highlands of Scotland, and of the
uplifted tracts of North America is now familiar to geologists, whilst the study
of comparatively recent sediments has proved the existence of widespread and
extensive movements in times which are geologically modern ; for instance, the
deep-water deposits of late Tertiary age found in the West Indies indicate
the occurrence of considerable uplift in that region.   But a great amount of

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[1] Similar knoll-like masses have been described in this country by Mr. R. H.
Tiddeman, as occurring in the Craven district of Yorkshire, but he does not attribute
their formation to coral-growth to any great extent.

work yet remains to be done in this connection, especially concerning horizontal distortion of masses of the earth's crust, owing to more rapid horizontal advance of one portion than of another, during periods of movement. Not until we gather together a large amount of information derived from actual inspection of the rocks shall we be able to frame satisfactory theories of earth-movement, and in the meantime we are largely dependent upon the speculations of the physicist, often founded upon very imperfect data, on which is built an imposing superstructure of mathematical reasoning  We have been told that our continents and ocean-basins have been to a great extent permanent as regards position through long geological ages; we now reply by pointing to deep-sea sediments of nearly all geological periods, which have been uplifted from the ocean-abysses to form portions of our continents; and as the result of study of the distribution of fossil organisms, we can point almost as confidently to the sites of old continents now sunk down into the ocean depths. It seems clear that our knowledge of the causes of earth-movements is still in its infancy, and that we must be content to wait awhile, until we have further information at our disposal.

Recent work has proved the intimate connection betwixt earth-movement and the emission and intrusion of igneous rocks, and the study of igneous rocks has advanced beyond the petrographical stage; the rocks are now made to contribute their share towards the history of different geological periods. The part which volcanic action has played in the actual formation of the earth's crust is well exemplified in Sir Archibald Geikie's Presidential Addresses to the Geological Society, wherein he treats of the former volcanic history of the British Isles.[1] The way in which extruded material contributes to the formation of sedimentary masses has, perhaps, not been fully grasped by many writers, who frequently seem to assume that deposition is a measure of denudation, and *vice versâ*, whereas deposition is only a measure of denudation, and the material which has been ejected in a fragmental condition from the earth's interior, which in some places forms a very considerable percentage of the total amount of sediment.

The intruded rocks also throw much light on past earth-history, and I cannot give a better illustration of the valuable information which they may furnish to the stratigraphical geologist, when rightly studied, than by referring to the excellent and suggestive work by my colleague, Mr. Alfred Harker, on the Bala Volcanic Rocks of Carnarvonshire.[2]

Perhaps the most striking instance of the effect which detailed stratigraphical work has produced on geological thought is supplied by the study of the crystalline schists. Our knowledge of the great bulk of the rocks which enter into the formation of a schistose complex is not very great, but the mode of production of many of them is now well known, and the crude speculations of some of the early geologists are now making way for theories founded on careful and minute observations in the field as well as in the laboratory. Recent work amongst the crystalline schists shows, furthermore, how careful we should be not to assume that because we have got at the truth, we have therefore ascertained the whole truth. We all remember how potent a factor dynamic metamorphism was supposed to be, owing to discoveries made in the greatly disturbed rocks of Scotland and Switzerland; and the action of heat was almost ignored by some writers, except as a minor factor, in the production of metamorphic change. The latest studies amongst the foliated rocks tend to show that heat does play a most important part in the manufacture of schists. The detailed work of Mr. George Barrow, in North-East Forfarshire,[3] has already thrown a flood of light upon the origin of certain schists, and their connection with igneous rocks, and geologists will look forward with eagerness to further studies of the puzzling Highland rocks by this keen observer.

The subject of former climatic conditions is one in which the geologist has very largely depended upon followers of other branches of science for light, and yet it is one peculiarly within the domain of the stratigraphical geologist; and

[1] Sir A. Geikie, *Quart. Journ. Geol. Soc.* vols. xlvii. and xlviii

[2] Alf. Harker, *Sedgwick Essay* for 1888 (Camb. Univ Press, 1889).

[3] G. Barrow, *Quart. Journ. Geol. Soc.* vol. xlix. (1893), p. 330.

information which has already been furnished concerning former climatic conditions, as the result of careful study of the strata, is probably only an earnest of what is to follow when the specialist in climatology pays attention to the records of the rocks, and avoids the theories elaborated in the student's sanctum. The recognition of an Ice Age in Pleistocene times at once proved the fallacy of the supposition that there has been a gradual fall in temperature throughout geological ages without any subsequent rise, and accordingly most theories which have been put forward to account for former climatic change have been advanced with special reference to the Glacial period or periods, although there are many other interesting matters connected with climate with which the geologist has to deal. Nevertheless, the occurrence of glacial periods is a matter of very great interest, and one which has deservedly received much attention, though the extremely plausible hypothesis of Croll, and the clear manner in which it has been presented to general readers, tended to throw other views into the shade, until quite recently, when this hypothesis has been controverted from the point of view of the physicist. In the meantime considerable advance has been made in our actual knowledge, and this year, probably for the first time, and as the result of the masterly *résumé* of Professor Edgworth David,[1] the bulk of British geologists are prepared to admit that there has been more than one glacial period, and that the evidence of glacial conditions in the southern hemisphere in Permo-Carboniferous times is established. Croll's hypothesis of course requires the recurrence of glacial periods, but leaving out of account arguments not of a geological character, which have been advanced against this hypothesis, the objection raised by Messrs. Gray and Kendall,[2] that in the case of the Pleistocene Ice Age ' the cold conditions came on with extreme slowness, the refrigerations being progressive from the Eocene period to the climax,' seems to me to be a fatal one. At the same time, rather than asking with the above writers ' the aid of astronomers and physicists in the solution of ' this problem, I would direct the attention of stratigraphical geologists to it, believing that, by steady accumulation of facts, they are more likely than anyone else to furnish the true clue to the solution of the glacial problem.

I have elsewhere called attention to marked changes in the faunas of the sedimentary rocks when passing from lower to higher levels, without the evidence of any apparent physical break, or any apparent change in the physical conditions, so far as can be judged from the lithological characters of the strata, and have suggested that such sudden faunistic variations may be due to climate. I refer to the matter as one which may well occupy the attention of local observers.

One of the most interesting points connected with climatic conditions is that of the former general lateral distribution of organisms, and its dependence upon the distribution of climatic zones. The well-known work of the late Dr. Neumayr[3] has, in the opinion of many geologists, established the existence of climatic zones whose boundaries ran practically parallel with the equator in Jurassic and Cretaceous times, and the possible existence of similar climatic zones in Palæozoic times has been elsewhere suggested; but it is very desirable that much more work should be done upon this subject, and it can only be carried out by paying close attention to the vertical and lateral distribution of organisms in the stratified rocks.

So far we have chiefly considered the importance of stratigraphical geology in connection with the inorganic side of nature. We now come to the bearing of detailed stratigraphical work upon questions concerning the life of the globe, and here the evidence furnished by the geologist particularly appeals to the general educated public as well as to students of other sciences.

[1] T. W. E. David, 'Evidences of Glacial Action in Australia in Permo-Carboniferous Time,' *Quart. Journ. Geol. Soc.* vol. lii. p 289.

[2] J W Gray, and P. F. Kendall, 'The Cause of an Ice Age,' *Brit. Assoc Rep* (1892), p. 708

[3] M. Neumayr, 'Ueber klimatische Zonen wahrend der Jura- und Kreidezeit,' *Denkschr. der math.-naturwissen. Classe der k. k Akad. der Wissenschaften*, vol. xlvii. **Vienna, 1883.**

Attention has just been directed to the probable importance of former climatic changes in determining the distribution of organisms, but the whole subject of the geographical distribution of organisms during former geological periods, though it has already received a considerable amount of attention, will doubtless have much further light thrown upon it as the result of careful observations carried out amongst the stratified rocks.

So long ago as 1853, Pictet laid it down as a palæontological law that 'the geographical distribution of species found in the strata was more extended than the range of species of existing faunas.' One would naturally expect that at a time when the diversity of animal organisation was not so great as it now is, the species, having fewer enemies with which to cope, and on the whole not too complex organisations to be affected by outward circumstances, would spread further laterally than they now do; but as we know that in earliest Cambrian times the diversity of organisation was very considerable, it is doubtful whether any appreciable difference would be exerted upon lateral distribution then and now, owing to this cause. At the time at which Pictet wrote, the rich fauna of the deeper parts of the oceans, with its many widely distributed forms of life, was unknown, and the range in space of early organisms must have then struck everyone who thought upon the subject as being greater than that of the shallow-water organisms of existing seas, which were alone known. It is by no means clear, however, with our present knowledge, that Pictet's supposed law holds good, and it will require a considerable amount of work before it can be shown to be even apparently true. Our lists of the fossils of different areas are not sufficiently complete to allow us to generalise with safety, but a comparison of the faunas of Australia and Britain indicates a larger percentage of forms common to the two areas, as we examine higher groups of the geological column. If this indication be fully borne out by further work, it will not prove the actual truth of the law, for the apparent wider distribution of ancient forms of life might be due to the greater probability of elevation of ancient deep-sea sediments than of more modern ones which have not been subjected to so many elevatory movements. Still, if the law be apparently true, it is a matter of some importance to geologists; and I have touched upon the matter here in order once again to emphasise the possibility of correlating comparatively small thicknesses of strata in distant regions by their included organisms.

Mention of Pictet's laws, one of which states that fossil animals were constructed upon the same plan as existing ones, leads me to remark upon the frequent assumption that certain fossils are closely related to living groups, when the resemblances between the hard parts of the living and extinct forms are only of the most general character. There is a natural tendency to compare a fossil with its nearest living ally, but the comparison has probably been often pushed too far, with the result that biologists have frequently been led to look for the ancestors of one living group exclusively amongst forms of life which are closely related to those of another living group. The result of detailed work is to bring out more and more prominently the very important differences between some ancient forms and any living creature, and to throw doubts on certain comparisons; thus I find several of the well-known fossils of the Old Red Sandstone, formerly referred without hesitation to the fishes, are now doubtfully placed in that class.

The importance of detailed observation in the field is becoming every day more apparent, and the specialist who remains in his museum examining the collections amassed by the labours of others, and never notes the mode of occurrence of fossils in the strata, will perhaps soon be extinct, himself an illustration of the principle of the survival of the fittest. In the first place, such a worker can never grasp the true significance of the changes wrought on fossil relics after they have become entombed in the strata, especially amongst those rocks which have been subjected to profound earth-movements, and it is to be feared that many 'species' are still retained in our fossil lists, whose supposed specific characters are due to distortion by pressure. But a point of greater importance is, that one who confines his attention to museums, cannot, unless the information supplied to

him be very full, distinguish the differences between fossils which are variations from a contemporaneous dominant form, such as 'sports,' and those which have been termed 'mutations,' which existed at a later period than the forms which they resemble. The value of the latter, to those who are attempting to work out phylogenies is obvious, and their nature can only be determined as the result of very laborious and accurate field-work, but such labour in such a cause is well worth performing. The student of phylogeny has had sufficient warning of the dangers which beset his path, from an inspection of the various phylogenetic trees, constructed mainly after study of existing beings only, so

> '. . like the borealis race,
> That flit ere you can point their place,'

but recent researches amongst various groups of fossil organisms have further illustrated the danger of theorising upon insufficient data, especially suggestive being the discovery of closely similar forms which were formerly considered to be much more nearly related than now proves to be the case; thus Dr. Mojsisovics[1] has shown that Ammonites once referred to the same species are specifically distinct, though their hard parts have acquired similar structures, sometimes contemporaneously, sometimes at different times, and Mr. S. S. Buckman[2] has observed the same thing, which he speaks of as 'heterogenetic homœomorphy' in the case of certain brachiopods, whilst Prof. H. A. Nicholson and I[3] have given reasons for supposing that such heterogenetic homœomorphy, in the case of the graptolites, has sometimes caused the inclusion in one genus of forms which have arisen from two distinct genera. As the result of careful work, dangers of the nature here suggested will be avoided, and our chances of indicating lines of descent correctly will be much increased. It must be remembered that however plausible the lines of descent indicated by students of recent forms may be, the actual links in the chains can only be discovered by examination of the rocks, and it is greatly to be desired that more of our geologists, who have had a thorough training in the field, should receive in addition one as thorough in the zoological laboratory. Shall I be forgiven if I venture on the opinion that a certain suspicion which some of my zoological fellow countrymen have of geological methods, is due to their comparative ignorance of palæontology, and that it is as important for them to obtain some knowledge of the principles of geology as it is for the stratigraphical palæontologist to study the soft parts of creatures whose relatives he finds in the stratified rocks?

The main lines along which the organisms of some of the larger groups have been developed, have already been indicated by several palæontologists, and detailed work has been carried out in several cases As examples, let me allude to the trilobites, of which a satisfactory natural classification was outlined by the great Barrande in those volumes of his monumental work which deal with the fossils of this order, whilst further indication of their natural inter-relationships has been furnished by Messrs. C. D. Walcott, G. F. Matthew, and others; to the graptolites, whose relationships have been largely worked out by Professor C. Lapworth, *facile princeps* amongst students of the *Graptolitoidea*, to whom we look for a full account of the phylogeny of the group; to the brachiopods, which have been so ably treated by Dr. C. E. Beecher,[4] largely from a study of recent forms, but also after careful study of those preserved in the fossil state; and to the echinids and lamellibranchs, whose history is being extensively elucidated by Dr. R. T. Jackson,[5] by methods somewhat similar to those pursued by Dr. Beecher.

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[1] E. Mojsisovics, *Abhandl der k. k. geol. Reichsanst.* vol. vi (1893).

[2] S. S. Buckman, *Quart Journ. Geol Soc.* vol li (1895), p 456

[3] H. A. Nicholson, and J. E Marr. *Geol. Mag* Dec 4, vol. ii. (1895), p. 531

[4] C. E. Beecher, 'Development of the Brachiopoda,' *Amer. Journ Sci*, Ser. iii, vol. xli. (1891) p 343, and vol. xliv. (1892) p 133

[5] R. T. Jackson, 'Phylogeny of the Pelecypoda,' *Mem Boston Soc. Nat Hist*, vol. iv (1890) p. 277; and 'Studies of Palæechinoidea,' *Bull. Geol. Soc Amer* vol. vii. (1896) p. 171.

I might give other instances,[1] but have chosen some striking ones, four of which especially illustrate the great advances which are being made in the study of the palæontology of the invertebrates by our American brethren.

I have occupied the main part of my address with reasons for the need of conducting stratigraphical work with minute accuracy. Many of you may suppose that the necessity for working in this way is so obvious that it is a work of supererogation to insist upon it at great length; but experience has taught me that many geologists consider that close attention to details is apt to deter workers from arriving at important generalisations, in the present state of our science. A review of the past history of the science shows that William Smith, and those who followed after him, obtained their most important results by steady application to details, and subsequent generalisation, whilst the work of those who theorise on insufficient data is apt to be of little avail, though often demanding attention on account of its very daring, and because of the power of some writers to place erroneous views in an attractive light, just as

> . . the sun can fling
> Colours as bright on exhalations bred
> By weedy pool or pestilential swamp,
> As on the rivulet, sparkling where it runs,
> Or the pellucid lake '

Nor is there any reason to suppose that it will be otherwise in the future, and I am not one of those who consider that the brilliant discoveries were the exclusive reward of the pioneers in our science, and that labourers of the present day must be contented with the gleanings of their harvest; on the contrary, the discoveries which await the geologist will probably be as striking as are those which he has made in the past. The onward march of science is a rhythmic movement, with now a period of steady labour, anon a more rapid advance in our knowledge. It would perhaps be going too far to say that, so far as our science is concerned, we are living in a period rather of the former than of the latter character, though no great geological discovery has recently affected human thought in the way in which it was affected by the proofs of the antiquity of man, and by the publication of 'The Origin of Species.' If, however, we are to some extent gathering materials, rather than drawing far-reaching conclusions from them, I believe this is largely due to the great expansion which our science has undergone in recent years. It has been said that geology is ' not so much one science, as the application of all the physical sciences to the examination and description of the structure of the earth, the investigation of the agencies concerned in the production of that structure, and the history of their action ; ' and the application of other sciences to the elucidation of the history of our globe has been so greatly extended of recent years, that we are apt to lose sight of the fact that geology is in itself a science, and that it is the special province of the geologist to get his facts at first hand from examination of the earth. The spectroscope and the telescope tell the geologist much ; but his proper instrument is the hammer, and the motto of every geologist should be that which has been adopted for the Geological Congress, ' *Mente et malleo.*'

At the risk of being compared to a child playing with edged tools, I cannot help referring to the bearing of modern stratigraphical research on the suggested replacement of a school of uniformitarianism by one of evolution. The distinguished advocate of Evolutionism, who addressed the Geological Society in 1869 upon the modern schools of geological thought, spoke of the school of evolution as though it were midway between those of uniformitarianism and catastrophism, as

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[1] *E.g.* The following papers treating of the Cephalopoda :—A. Hyatt, ' Genesis of the Arietidæ,' Smithsonian Contributions, vol. xxvi (1889), M. Neumayr, Jura-Studien I, ' Ueber Phylloceraten,' *Jahrb. der k. k. Geol. Reichsanst.* vol. xxi. (1871), p. 297, L. Würtenberger, 'Studien über die Stammesgeschichte der Ammoniten,' Leipzig, 1880, S. S. Buckman, ' A Monograph of the Inferior Oolite Ammonites of the British Islands,' 1887—(*Monogr. Palæontographical Soc* )

indeed it is logically, though, considering the tenets of the upholders of catastro-phism, as opposed to those of uniformitarianism, at the time of that address, there is no doubt that evolutionism was rather a modification of the uniformitarianism of the period than intermediate between it and catastrophism, which was then practically extinct, at any rate in Britain  One of my predecessors in this chair, speaking upon this subject, says that 'the good old British ship " Uniformity," built by Hutton and refitted by Lyell, has won so many glorious victories in the past, and appears still to be in such excellent fighting trim, that I see no reason why she should haul down her colours, either to " catastrophe " or "evolution."'
It may be so, but I doubt the expediency of nailing those colours to the mast. That Lyell, in his great work, proved that the agents now in operation, working with the same activity as that which they exhibit at the present day, *might* pro-duce the phenomena exhibited by the stratified rocks, seems to be generally admitted, but that is not the same thing as proving that they *did* so produce them. Such proof can only be acquired by that detailed examination of the strata which I have advocated in this address, and at the time that the last edition of the 'Principles' appeared, our knowledge of the strata was far less complete than it has subsequently become.  It appears to me that we should keep our eyes open to the possibility of many phenomena presented by rocks, even newer than the Archæan rocks, having been produced under different conditions from those now prevalent.  The depths and salinity of the oceans, the heights and extent of con-tinents, the conditions of volcanic action, and many other things may have been markedly different from what they are at present, and it is surely unphilosophical to assume conditions to have been generally similar to those of the present day, on the slender data at our disposal.  Lastly, uniformitarianism, in its strictest sense, is opposed to rhythmic recurrence of events.  'Rhythm is the rule with nature; she abhors uniformity more than she does a vacuum,' wrote Professor Tyndall, many years ago, and the remark is worth noting by geologists  Why have we no undoubted signs of glacial epochs amongst the strata from early Cambrian times to the Great Ice Period, except in Permo-Carboniferous times?  Is there not an apparent if not a real absence of manifestation of volcanic activity over wide areas of the earth in Mesozoic times?  Were not Devonian, Permo-Triassic, and Miocene times periods of mountain-building over exceptionally wide areas, whilst the intervening periods were rather marked by quiet depression and sedimentation?  A study of the evidence available in connection with questions like these suggests rhythmic recurrence.  Without any desire to advocate hasty departure from our present methods of research, I think it should be clearly recognised that evolution may have been an important factor in changing the conditions even of those times of which the geologist has more direct knowledge.  In this, as in many other ques-tions, it is best to preserve an open mind; indeed, I think that geologists will do well to rest satisfied without an explanation to many problems, amongst them the one just referred to; and that working hypotheses, though useful, are better retained in the manuscript notebooks of the workers than published in the 'Transactions of Learned Societies, whence they filter out into popular works,'to the great delight of a sceptical public should they happen to be overthrown

May I trespass upon your patience for one moment longer?  As a teacher of geology, with many years' experience in and out of a large University, I have come to the conclusion that geology is becoming more generally recognised as a valuable instrument of education.  The memory, the reasoning faculties, and the powers of observation are alike quickened.  The work in the open air, which is inseparable from a right understanding of the science, keeps the body in healthy condition.  But over and above these benefits, the communing with nature, often in her most impressive moods, and the insignificance of events in a man's lifetime, as compared with the ceaseless changes through the long æons which have gone before, so influence man's moral nature, that they drive out his meaner thoughts and make him 'live in charity with all men.'

# British Association for the Advancement of Science.

## LIVERPOOL, 1896.

# ADDRESS

### TO THE

# ZOOLOGICAL SECTION

#### BY

### Professor E. B. POULTON, M.A., F.R.S., F.L.S.,

#### PRESIDENT OF THE SECTION.

A VERY brief study of the proceedings of this Section in byegone years will show that Presidents have exercised a very wide choice in the selection of subjects. At the last Meeting of the Association in this city in 1870 the Biological Section had as its President the late Professor Rolleston, a man whose remarkable personality made a deep impression upon all who came under his influence, as I have the strongest reason for remembering, inasmuch as he was my first teacher in zoology, and I attended his lectures when but little over seventeen. His address was most characteristic, glancing over a great variety of subjects, literary as well as scientific, and abounding in quotations from several languages, living and dead. A very different style of address was that delivered by the distinguished zoologist who presided over the Meeting. Professor Huxley took as his subject ' The History of the Rise and Progress of a Single Biological Doctrine.'

Of these two types I selected the latter as my example, and especially desired to attempt the discussion, however inadequate, of some difficulty which confronts the zoologist at the very outset, when he begins to reason from the facts around him—a difficulty which is equally obvious and of equal moment to the highly-trained investigator and the man who is keenly interested in the results obtained by others, but cannot himself lay claim to the position and authority of a skilled observer—to the naturalist and to one who follows some other branch of knowledge, but is interested in the progress of a sister science

Two such difficulties were alluded to by Lord Salisbury in his interesting presidential address to the British Association at Oxford in 1894, when he spoke of ' two of the strongest objections to the Darwinian explanation ' of evolution—viz. the theory of natural selection—as appearing ' still to retain all their force.' The first of these objections was the insufficiency of the time during which the earth has been in a habitable state, as calculated by Lord Kelvin and Professor Tait, 100 million years being conceded by the former, but only 10 million by the latter. Lord Salisbury quite rightly stated that for the evolution of the organic world as we know it by the slow process of natural selection at least many hundred million years are required ; whereas, ' if the mathematicians are right, the biologists cannot have what they demand. . . The jelly-fish would have been dissipated in steam long before he had had a chance of displaying the advantageous variation which was to make him the ancestor of the human race.'

The second objection was that ' we cannot demonstrate the process of natural selection in detail; we cannot even, with more or less ease, imagine it.' ' In natural selection who is to supply the breeder's place ? ' ' There would be nothing

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but mere chance to secure that the advantageously varied bridegroom at one end of the wood should meet the bride, who by a happy contingency had been advantageously varied in the same direction at the same time at the other end of the wood. It would be a mere chance if they ever knew of each other's existence—a still more unlikely chance that they should resist on both sides all temptations to a less advantageous alliance. But unless they did so the new breed would never even begin, let alone the question of its perpetuation after it had begun.'

Professor Huxley, in seconding the vote of thanks to the President, said that he could imagine that certain parts of the address might raise a very good discussion in one of the Sections, and I have little doubt that he referred to these criticisms and to this Section. When I had to face the duty of preparing this address, I could find no subjects better than those provided by Lord Salisbury.

At first the second objection seemed to offer the more attractive subject. It was clear that the theory of natural selection as held by Darwin was misconceived by the speaker, and that the criticism was ill-aimed. Darwin and Wallace, from the very first, considered that the minute differences which separate individuals were of far more importance than the large single variations which occasionally arise— Lord Salisbury's advantageously varied bride and bridegroom at opposite ends of the wood. In fact, after Fleeming Jenkins's criticisms in the 'North British Review' for June 1867, Darwin abandoned these large single variations altogether. Thus he wrote in a letter to Wallace (February 2, 1869): 'I always thought individual differences more important; but I was blind, and thought single variations might be preserved much oftener than I now see is possible or probable. I mentioned this in my former note merely because I believed that you had come to a similar conclusion, and I like much to be in accord with you.'[1] Hence we may infer that the other great discoverer of natural selection had come to the same conclusion at an even earlier date. But this fact removes the whole point from the criticism I have just quoted. According to the Darwin-Wallace theory of natural selection, individuals sufficiently advantageously varied to become the material for a fresh advance when an advance became necessary, and at other times sufficient to maintain the ground previously gained—such individuals existed not only at the opposite ends of the wood, but were common enough in every colony within its confines. The mere fact that an individual had been able to reach the condition of a possible bride or bridegroom would count for much. Few will dispute that such individuals 'have already successfully run the gauntlet of by far the greatest dangers which beset the higher animals [and, it may be added, the lower animals also]—the dangers of youth. Natural selection has already pronounced a satisfactory verdict upon the vast majority of animals which have reached maturity.'[2]

But the criticism retains much force when applied to another theory of evolution by the selection of large and conspicuous variations, a theory which certain writers have all along sought to add to or substitute for that of Darwin. Thus Huxley from the very first considered that Darwin had burdened himself unnecessarily in rejecting *per saltum* evolution so unreservedly.[3] And recently this view has been revived by Bateson's work on variation and by the writings of Francis Galton. I had at first intended to attempt a discussion of this view, together with Lord Salisbury's and other objections which may be urged against it; but the more the two were considered, the more pressing became the claims of the criticism alluded to at first—the argument that the history of our planet does not allow sufficient time for a process which all its advocates admit to be extremely slow in its operation. I select this subject because of its transcendent importance in relation to organic evolution, and because I hope to show that the naturalist has something of weight to contribute to the controversy which has been waged intermittently ever since Lord Kelvin's paper 'On Geological Time'[4] appeared in 1868. It has

[1] *Life and Letters*, vol iii.
[2] Poulton, *Colours of Animals*, p. 308.
[3] See his letter to Darwin, November 23, 1859 : *Life and Letters*, vol. ii.
[4] *Trans. Geol. Soc.*, Glasgow, vol. iii    See also ' On the Age of the Sun's Heat,' Macmillan, March 1862   reprinted as Appendix to Thomson and Tait, *Natural Philo-*

been urged by the great worker and teacher who occupied the Presidential Chair of this Association when it last met in this city that biologists have no right to take part in this discussion. In his Anniversary Address to the Geological Society in 1869 Huxley said : ' Biology takes her time from geology. . . . If the geological clock is wrong, all the naturalist will have to do is to modify his notions of the rapidity of change accordingly.' This contention is obviously true as regards the time which has elapsed since the earliest fossiliferous rocks were laid down. For the duration of the three great periods we must look to the geologist ; but the question as to whether the whole of organic evolution is comprised within these limits, or, if not, what proportion of it is so contained, is a question for the naturalist. The naturalist alone can tell the geologist whether his estimate is sufficient, or whether it must be multiplied by a small or by some unknown but certainly high figure, in order to account for the evolution of the earliest forms of life known in the rocks. This, I submit, is a most important contribution to the discussion.

Before proceeding further it is right to point out that obviously these arguments will have no weight with those who do not believe that evolution is a reality. But although the causes of evolution are greatly debated, it may be assumed that there is no perceptible difference of opinion as to evolution itself, and this common ground will bear the weight of all the zoological arguments we shall consider to-day.

It will be of interest to consider first how the matter presented itself to naturalists before the beginning of this controversy on the age of the habitable earth. I will content myself with quotations from three great writers on biological problems—men of extremely different types of mind, who yet agreed in their conclusions on this subject.
In the original edition of the ' Origin of Species,' (1859), Darwin, arguing from the presence of trilobites, Nautilus, Lingula, &c., in the earliest fossiliferous rocks, comes to the following conclusion (pages 306, 307) : ' Consequently, if my theory be true, it is indisputable that before the lowest Silurian stratum was deposited long periods elapsed, as long as, or probably far longer than, the whole interval from the Silurian age to the present day ; and that during these vast yet quite unknown periods of time the world swarmed with living creatures.'
The depth of his conviction in the validity of this conclusion is seen in the fact that the passage remains substantially the same in later editions, in which, however, Cambrian is substituted for Silurian, while the words ' yet quite unknown ' are omitted, as a concession, no doubt, to Lord Kelvin's calculations, which he then proceeds to discuss, admitting as possible a more rapid change in organic life, induced by more violent physical changes.[1]
We know, however, that such concessions troubled him much, and that he was really giving up what his judgment still approved. Thus he wrote to Wallace on April 14, 1869 : ' Thomson's views of the recent age of the world have been for some time one of my sorest troubles . . .' And again, on July 12, 1871, alluding to Mivart's criticisms, he says : ' I can say nothing more about missing links than what I have said. I should rely much on pre-Silurian times, but then comes Sir W. Thomson, like an odious spectre.'
Huxley's demands for time in order to account for pre-Cambrian evolution, as he conceived it, were far more extensive. Although in 1869 he bade the naturalist stand aside and take no part in the controversy, he had nevertheless spoken as a naturalist in 1862, when, at the close of another Anniversary Address to the same Society, he argued from the prevalence of persistent types ' that any admissible hypothesis of progressive modification must be compatible with persistence without progression through indefinite periods ; ' and then maintained

sophy, vol. i. part 2, second edition ; and ' On the Secular Cooling of the Earth,' Royal Society of Edinburgh, 1862.
[1] 6th ed., 1872, p. 286.

that ' should such an hypothesis eventually be proved to be true . . . the conclusion will inevitably present itself that the Palæozoic, Mesozoic, and Cainozoic faunæ and floræ, taken together, bear somewhat the same proportion to the whole series of living beings which have occupied this globe as the existing fauna and flora do to them.'

Herbert Spencer, in his article on 'Illogical Geology' in the 'Universal Review' for July 1859,[1] uses these words: 'Only the last chapter of the earth's history has come down to us The many previous chapters, stretching back to a time immeasurably remote, have been burnt, and with them all the records of life we may presume they contained' Indeed, so brief and unimportant does Herbert Spencer consider this last chapter to have been that he is puzzled to account for ' such evidences of progression as exist '; and finally concludes that they are of no significance in relation to the doctrine of evolution, but probably represent the succession of forms by which a newly upheaved land would be peopled. He argues that the earliest immigrants would be the lower forms of animal and vegetable life, and that these would be followed by an irregular succession of higher and higher forms, which ' would thus simulate the succession presented by our own sedimentary series.'

We see, then, what these three great writers on evolution thought on this subject: they were all convinced that the time during which the geologists concluded that the fossiliferous rocks had been formed was utterly insufficient to account for organic evolution.

Our object to-day is first to consider the objections raised by physicists against the time demanded by the geologist, and still more against its multiplication by the student of organic evolution ; secondly, to inquire whether the present state of palæontological and zoological knowledge increases or diminishes the weight of the threefold opinion quoted above—an opinion formed on far more slender evidence than that which is now available. And if we find this opinion sustained, it must be considered to have a very important bearing upon the controversy.

The arguments of the physicists are three :—

First, the argument from the observed secular change in the length of the day the most important element of which is due to tidal retardation. It has been known for a very long time that the tides are slowly increasing the length of our day. Huxley explains the reason with his usual lucidity : 'That this must be so is obvious, if one considers, roughly, that the tides result from the pull which the sun and the moon exert upon the sea, causing it to act as a sort of break upon the solid earth.'[2]

A liquid earth takes a shape which follows from its rate of revolution, and from which, therefore, its rate of revolution can be calculated.

The liquid earth consolidated in the form it last assumed, and this shape has persisted until now, and informs us of the rate of revolution at the time of consolidation Comparing this with the present rate, and knowing the amount of lengthening in a given time due to tidal friction, we can calculate the date of consolidation as certainly less than 1,000 million years ago.

This argument is fallacious, as many mathematicians have shown. The present shape tells us nothing of the length of the day at the date of consolidation ; for the earth, even when solid, will alter its form when exposed for a long time to the action of great forces. As Professor Perry said in a letter to Professor Tait[3]: ' I know that solid rock is not like cobbler's wax, but 1000 million years is a very long time, and the forces are great.' Furthermore, we know that the earth is always altering its shape, and that whole coast-lines are slowly rising or falling, and that this has been true, at any rate, during the formation of the stratified rocks.

This argument is dead and gone. We are, indeed, tempted to wonder that the

[1] Reprinted in his *Essays*, 1868, vol i., pp. 324–376
[2] Anniv. Address to Geol. Soc., 1869.
[3] *Nature*, Jan. 3, 1895.

physicist, who was looking about for arguments by which to revise what he conceived to be the hasty conclusions of the geologist as to the age of the earth, should have exposed himself to such an obvious retort in basing his own conclusions as to its age on the assumption that the earth, which we know to be always changing in shape, has been unable to alter its equatorial radius by a few miles under the action of tremendous forces constantly tending to alter it, and having 1,000 million years in which to do the work.

With this flaw in the case it is hardly necessary to insist on our great uncertainty as to the rate at which the tides are lengthening the day.

The spectacle presented by the geologist and biologist, deeply shocked at Lord Kelvin's extreme uniformitarianism in the domain of astronomy and cosmic physics, is altogether too comforting to be passed by without remark; but in thus indulging in a friendly *tu quoque*, I am quite sure that I am speaking for every member of this Section in saying that we are in no way behind the members of Section A in our pride and admiration at the noble work which he has done for science, and we are glad to take this opportunity of congratulating him on the half-century of work and teaching—both equally fruitful—which has reached its completion in the present year.

The second argument is based upon the cooling of the earth, and this is the one brought forward and explained by Lord Salisbury in his Presidential Address. It has been the argument on which perhaps the chief reliance has been placed, and of which the data—so it was believed—were the least open to doubt.

On the Sunday during the meeting of the British Association at Leeds (1890), I went for a walk with Professor Perry, and asked him to explain the physical reasons for limiting the age of the earth to a period which the students of other sciences considered to be very inadequate. He gave me an account of the data on which Lord Kelvin relied in constructing this second argument, and expressed the strong opinion that they were perfectly sound, while, as for the mathematics, it might be taken for granted, he said, that they were entirely correct. He did not attach much weight to the other arguments, which he regarded as merely offering support to the second.

This little piece of personal history is of interest, inasmuch as Professor Perry has now provided us with a satisfactory answer to the line of reasoning which so fully satisfied him in 1890. And he was led to a critical examination of the subject by the attitude taken up by Lord Salisbury in 1894. Professor Perry was not present at the meeting, but when he read the President's address, and saw how other conclusions were ruled out of court, how the only theory of evolution which commands anything approaching universal assent was set on one side because of certain assumptions as to the way in which the earth was believed to have cooled, he was seized with a desire to sift these assumptions, and to inquire whether they would bear the weight of such far-reaching conclusions. Before giving the results of his examination, it is necessary to give a brief account of the argument on which so much has been built.

Lord Kelvin assumed that the earth is a homogeneous mass of rock similar to that with which we are familiar on the surface. Assuming, further, that the temperature increases, on the average, 1° F. for every 50 feet of depth near the surface everywhere, he concluded that the earth would have occupied not less than twenty, nor more than four hundred, million years in reaching its present condition from the time when it first began to consolidate and possessed a uniform temperature of 7,000° F.

If, in the statement of the argument, we substitute for the assumption of a homogeneous earth an earth which conducts heat better internally than it does toward the surface, Professor Perry, whose calculations have been verified by Mr. O. Heaviside, finds that the time of cooling has to be lengthened to an extent which depends upon the value assigned to the internal conducting power. If, for instance, we assume that the deeper part of the earth conducts ten times as well as the outer part, Lord Kelvin's age would require to be multiplied by fifty-six. Even if the conductivity be the same throughout, the increase of density in the

deeper part, by augmenting the capacity for heat of unit volume, implies a longer age than that conceded by Lord Kelvin. If the interior of the earth be fluid or contain fluid in a honeycomb structure, the rate at which heat can travel would be immensely increased by convection currents, and the age would have to be correspondingly lengthened. If, furthermore, such conditions, although not obtaining now, did obtain in past times, they will have operated in the same direction.

Professor Tait, in his letter to Professor Perry (published in 'Nature' of January 3, 1895), takes the entirely indefensible position that the latter is bound to prove the higher internal conductivity. The obligation is all on the other side, and rests with those who have pressed their conclusions hard and carried them far. These conclusions have been, as Darwin found them, one of our 'sorest troubles'; but when it is admitted that there is just as much to be said for another set of assumptions leading to entirely different conclusions, our troubles are at an end, and we cease to be terrified by an array of symbols, however unintelligible to us. It would seem that Professor Tait, without, as far as I can learn, publishing any independent calculation of the age of the earth, has lent the weight of his authority to a period of 10 million years, or half of Lord Kelvin's minimum. But in making this suggestion he apparently feels neither interest nor responsibility in establishing the data of the calculations which he borrowed to obtain therefrom a very different result from that obtained by their author.

Professor Perry's object was not to substitute a more correct age for that obtained by Lord Kelvin, but rather to show that the data from which the true age could be calculated are not really available. We obtain different results by making different assumptions, and there is no sufficient evidence for accepting one assumption rather than another. Nevertheless, there is some evidence which indicates that the interior of the earth in all probability conducts better than the surface. Its far higher density is consistent with the belief that it is rich in metals, free or combined. Professor Schuster concludes that the internal electric conductivity must be considerably greater than the external. Geologists have argued from the amount of folding to which the crust has been subjected that cooling must have taken place to a greater depth than 120 miles, as assumed in Lord Kelvin's argument. Professor Perry's assumption would involve cooling to a much greater depth.

Professor Perry's conclusion that the age of the habitable earth is lengthened by increased conductivity is the very reverse of that to which we should be led by a superficial examination of the case. Professor Tait, indeed, in the letter to which I have already alluded, has said: 'Why, then, drag in mathematics at all, since it is absolutely obvious that the better conductor the interior in comparison with the skin, the longer ago must it have been when the whole was at 7000 F., the state of the skin being as at present?' Professor Perry, in reply, pointed out that one mathematician who had refuted the tidal retardation argument [1] had assumed that the conditions described by Professor Tait would have involved a shorter period of time. And it is probable that Lord Kelvin thought the same; for he had assumed conditions which would give the result— so he believed at the time—most acceptable to the geologist and biologist. Professor Perry's conclusion is very far from obvious, and without the mathematical reasoning would not be arrived at by the vast majority of thinking men.

The 'natural man' without mathematics would say, so far from this being 'absolutely obvious,' it is quite clear that increased conductivity, favouring escape of heat, would lead to more rapid cooling, and would make Lord Kelvin's age even shorter.

The argument can, however, be put clearly without mathematics, and, with Professor Perry's help, I am able to state it in a few words. Lord Kelvin's assumption of an earth resembling the surface rock in its relations to heat leads to the present condition of things, namely, a surface gradient of 1° F. for every 50 feet, in 100,000,000 years, more or less. Deeper than 150 miles he imagines

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[1] Rev. M. H. Close in *R. Dublin Soc.*, February 1878.

that there has been almost no cooling. If, however, we take one of the cases put by Professor Perry, and assume that below a depth of four miles there is ten times the conductivity, we find that after a period of 10,000,000,000 years the gradient at the surface is still 1° F. for every 50 feet; but that we have to descend to a depth of 1,500 miles before we find the initial temperature of 7,000° F. undiminished by cooling. In fact the earth, as a whole, has cooled far more quickly than under Lord Kelvin's conditions, the greater conductivity enabling a far larger amount of the internal heat to escape; but in escaping it has kept up the temperature gradient at the surface.

Lord Kelvin, replying to Professor Perry's criticisms, quite admits that the age at which he had arrived by the use of this argument may be insufficient. Thus, he says, in his letter [1]: 'I thought my range from 20 millions to 400 millions was probably wide enough, but it is quite possible that I should have put the superior limit a good deal higher, perhaps 4,000 instead of 400.'

The third argument was suggested by Helmholtz, and depends on the life of the sun. If the energy of the sun is due only to the mutual gravitation of its parts, and if the sun is now of uniform density, 'the amount of heat generated by his contraction to his present volume would have been sufficient to last 18 million years at his present rate of radiation.' [2] Lord Kelvin rejects the assumption of uniform density, and is, in consequence of this change, able to offer a much higher upward limit of 500 million years.

This argument also implies the strictest uniformitarianism as regards the sun. We know that other suns may suddenly gain a great accession of energy, so that their radiation is immensely increased. We only detect such changes when they are large and sudden, but they prepare us to believe that smaller accessions may be much more frequent, and perhaps a normal occurrence in the evolution of a sun. Such accessions may have followed from the convergence of a stream of meteors. Again, it is possible that the radiation of the sun may have been diminished and his energy conserved by a solar atmosphere.

Newcomb has objected to these two possible modes by which the life of the sun may have been greatly lengthened, that a lessening of the sun's heat by under a quarter would cause all the water on the earth to freeze, while an increase of much over half would probably boil it all away. But such changes in the amount of radiation received would follow from a greater distance from the sun of 15½ per cent., and a greater proximity to him of 18·4 per cent., respectively Venus is inside the latter limit, and Mars outside the former, and yet it would be a very large assumption to conclude that all the water in the former is steam, and all in the latter ice. Indeed, the existence of water and the melting of snow on Mars are considered to be thoroughly well authenticated. It is further possible that in a time of lessened solar radiation the earth may have possessed an atmosphere which would retain a larger proportion of the sun's heat; and the internal heat of the earth itself, great lakes of lava under a canopy of cloud for example, may have played an important part in supplying warmth.

Again we have a greater age if there was more energy available than in Helmholtz's hypothesis. Lord Kelvin maintains that this is improbable because of the slow rotation of the sun, but Perry has given reasons for an opposite conclusion.

The collapse of the first argument of tidal retardation, and of the second of the cooling of the earth, warn us to beware of a conclusion founded on the assumption that the sun's energy depends, and has ever depended, on a single source of which we know the beginning and the end It may be safely maintained that such a conclusion has not that degree of certainty which justifies the followers of one science in assuming that the conclusion of other sciences must be wrong, and in disregarding the evidence brought forward by workers in other lines of research.

We must freely admit that this third argument has not yet fully shared the fate

[1] *Nature*, January 3, 1895.
[2] Newcomb's *Popular Astronomy*, p. 523.

of the two other lines of reasoning.   Indeed, Professor George Darwin, although
attacking these latter, agrees with Lord Kelvin in regarding 500 million years as
the maximum life of the sun [1]

We may observe, too, that 500 million years is by no means to be despised: a
great deal may happen in such a period of time.   Although I should be very sorry
to say that it is sufficient, it is a very different offer from Professor Tait's
10 million.

In drawing up this account of the physical arguments, I owe almost everything
to Professor Perry for his articles in 'Nature' (January 3 and April 18, 1895),
and his kindness in explaining any difficulties that arose.   I have thought it right
to enter into these arguments in some detail, and to consume a considerable pro-
portion of our time in their discussion.   This was imperatively necessary, because
they claimed to stand as barriers across our path, and, so long as they were admitted
to be impassable, any further progress was out of the question.   What I hope has
been an unbiassed examination has shown that, as barriers, they are more imposing
than effective, and we are free to proceed, and to look for the conclusions warranted
by our own evidence.   In this matter we are at one with the geologists; for, as
has been already pointed out, we rely on them for an estimate of the time occupied
by the deposition of the stratified rocks, while they rely on us for a conclusion as to
how far this period is sufficient for the whole of organic evolution.

First, then, we must briefly consider the geological argument, and I cannot do
better than take the case as put by Sir Archibald Geikie in his Presidential Address
to this Association at Edinburgh in 1892.

Arguing from the amount of material removed from the land by denuding
agencies, and carried down to the sea by rivers, he showed that the time required
to reduce the height of the land by one foot, varies, according to the activity of the
agencies at work, from 730 years to 6,800 years.   But this also supplies a measure
of the rate of deposition of rock, for the same material is laid down elsewhere,
and would of course add the same height of one foot to some other area equal in
size to that from which it was removed.

The next datum to be obtained is the total thickness of the stratified rocks
from the Cambrian system to the present day.   'On a reasonable computation
these stratified masses, where most fully developed, attain a united thickness of
not less than 100,000 feet.   If they were all laid down at the most rapid recorded
rate of denudation, they would require a period of seventy-three millions of years
for their completion.   If they were laid down at the slowest rate, they would
demand a period of not less than 680 millions.'

The argument that geological agencies acted much more vigorously in past
times he entirely refuted by pointing to the character of the deposits of which the
stratified series is composed.   'We can see no proof whatever, nor even any
evidence which suggests that on the whole the rate of waste and sedimentation
was more rapid during Mesozoic and Palæozoic time than it is to-day.   Had there
been any marked difference in this rate from ancient to modern times, it would be
incredible that no clear proof of it should have been recorded in the crust of the
earth.'

It may therefore be inferred that the rate of deposition was no nearer the more
rapid than the slower of the rates recorded above, and, if so, the stratified rocks
would have been laid down in about 400 million years.

There are other arguments favouring the uniformity of conditions throughout
the time during which the stratified rocks were laid down, in addition to those
which are purely geological and depend upon the character of the rocks themselves.
Although more biological than geological, these arguments are best considered
here.

The geological agency to which attention is chiefly directed by those who desire
to hurry up the phenomena of rock formation is that of the tides.   But it seems

certain that the tides were not sufficiently higher in Silurian times to prevent the deposition of certain beds of great thickness under conditions as tranquil as any of which we have evidence in the case of a formation extending over a large area. From the character of the organic remains it is known that these beds were laid down in the sea, and there are the strongest grounds for believing that they were accumulated along shores and in fairly shallow water. The remains of extremely delicate organisms are found in immense numbers, and over a very large area. The recent discovery, in the Silurian system of America, of trilobites, with their long delicate antennæ perfectly preserved, proves that in one locality (Rome, New York State) the tranquillity of deposition was quite as profound as in any locality yet discovered on this side of the Atlantic

There are, then, among the older Palæozoic rocks a set of deposits than which we can imagine none better calculated to test the force of the tides; and we find that they supply evidence for exceptional tranquillity of conditions over a long period of time.

There is other evidence of the permanence, throughout the time during which the stratified rocks were deposited, of conditions not very dissimilar from those which obtain to-day. Thus the attachments of marine organisms, which are permanently rooted to the bottom or on the shores, did not differ in strength from those which we now find—an indication that the strains due to the movements of the sea did not greatly differ in the past

We have evidence of a somewhat similar kind to prove uniformity in the movements of the air The expanse of the wings of flying organisms certainly does not differ in a direction which indicates any greater violence in the atmospheric conditions. Before the birds had become dominant among the larger flying organisms, their place was taken by the flying reptiles, the pterodactyls, and before the appearance of these we know that, in Palæozoic times, the insects were of immense size, a dragon-fly from the Carboniferous rocks of France being upwards of 2 feet in the expanse of its wings. As one group after another of widely dissimilar organisms gained control of the air, each was in turn enabled to increase to the size which was best suited to such an environment, but we find that the limits which obtain to-day were not widely different in the past. And this is evidence for the uniformity in the strains due to wind and storm no less than to those due to gravity. Furthermore, the condition of the earth's surface at present shows us how extremely sensitive the flying organism is to an increase in the former of these strains, when it occurs in proximity to the sea. Thus it is well known that an unusually large proportion of the Madeiran beetles are wingless, while those which require the power of flight possess it in a stronger degree than on continental areas. This evolution in two directions is readily explained by the destruction by drowning of the winged individuals of the species which can manage to live without the power of flight, and of the less strongly winged individuals of those which need it. Species of the latter kind cannot live at all in the far more stormy Kerguelen Land, and the whole of the insect fauna is wingless

The size and strength of the trunks of fossil trees afford, as Professor George Darwin has pointed out, evidence of uniformity in the strains due to the condition of the atmosphere.

We can trace the prints of raindrops at various geological horizons, and in some cases found in this country it is even said that the eastern side of the depressions is the more deeply pitted, proving that the rain drove from the west, as the great majority of our storms do to-day.

When, therefore, we are accused of uniformitarianism, as if it were an entirely unproved assumption, we can at any rate point to a large body of positive evidence which supports our contention, and the absence of any evidence against it. Furthermore, the data on which we rely are likely to increase largely, as the result of future work.

After this interpolation, chiefly of biological argument in support of the geologist, I cannot do better than bring the geological evidence to a close in the words which conclude Sir Archibald Geikie's address : ' After careful reflection on the subject, I affirm that the geological record furnishes a mass of evidence which no

arguments drawn from other departments of Nature can explain away, and which, it seems to me, cannot be satisfactorily interpreted save with an allowance of time much beyond the narrow limits which recent physical speculation would concede.'

In his letter to Professor Perry,[1] Lord Kelvin says:—

'So far as underground heat alone is concerned, you are quite right that my estimate was 100 million, and please remark[2] that that is all Geikie wants; but I I should be exceedingly frightened to meet him now with only 20 million in my mouth.'

We have seen, however, that Geikie considered the rate of sedimentation to be, on the whole, uniform with that which now obtains, and this would demand a period of nearly 400 million years. He points out furthermore that the time must be greatly increased on account of the breaks and interruptions which occur in the series, so that we shall probably get as near an estimate as is possible from the data which are available by taking 450 million as the time during which the stratified rocks were formed.

Before leaving this part of the subject, I cannot refrain from suggesting a line of enquiry which may very possibly furnish important data for checking the estimates at present formed by geologists, and which, if the mechanical difficulties can be overcome, is certain to lead to results of the greatest interest and importance. Ever since the epoch-making voyage of the 'Challenger,' it has been known that the floor of the deep oceans outside the shallow shelf which fringes the continental areas is covered by a peculiar deposit formed entirely of meteoric and volcanic dust, the waste of floating pumice, and the hard parts of animals living in the ocean. Of these latter only the most resistant can escape the powerful solvent agencies. Many observations prove that the accumulation of this deposit is extremely slow. One indication of this is especially convincing: the teeth of sharks and the most resistant part of the skeleton—the ear-bones—of whales are so thickly spread over the surface that they are continually brought up in the dredge, while sometimes a single haul will yield a large number of them. Imagine the countless generations of sharks and whales which must have succeeded each other in order that these insignificant portions of them should be so thickly spread over that vast area which forms the ocean floor. We have no reason to suppose that sharks and whales die more frequently in the deep ocean than in the shallow fringing seas; in fact, many observations point in the opposite direction, for wounded and dying whales often enter shallow creeks and inlets, and not uncommonly become stranded. And yet these remains of sharks and whales, although well known in the stratified rocks which were laid down in comparatively shallow water and near coasts, are only found in certain beds, and then in far less abundance than in the oceanic deposit. We can only explain this difference by supposing that the latter accumulate with such almost infinite slowness as compared with the continental deposits that these remains form an important and conspicuous constituent of the one, while they are merely found here and there when looked for embedded in the other. The rate of accumulation of all other constituents is so slow as to leave a layer of teeth and ear-bones uncovered, or covered by so thin a deposit that the dredge can collect them freely. Dr. John Murray calculates that only a few inches of this deposit have accumulated since the Tertiary Period. These most interesting facts prove furthermore that the great ocean basins and continental areas have occupied the same relative positions since the formation of the first stratified rocks; for no oceanic deposits are found anywhere in the latter. We know the sources of the oceanic deposit, and it might be possible to form an estimate, within wide limits, of its rate of accumulation. If it were possible to ascertain its thickness by means of a boring, some conclusions as to the time which has elapsed during the lifetime of certain species—perhaps even the lifetime of the oceans themselves—might be arrived at. Lower down the remains of earlier species would probably be found. The depth of this deposit and its character at

[1] *Nature*, Jan 3, 1895.
[2] *P. L and A*, vol. ii., p. 87.

deeper levels are questions of overwhelming interest; and perhaps even more so is the question as to what lies beneath. Long before the 'Challenger' had proved the persistence of oceanic and continental areas, Darwin, with extraordinary foresight, and opposed by all other naturalists and geologists, including his revered teacher, Lyell, had come to the same conclusion. His reasoning on the subject is so convincing that it is remarkable that he made so few converts, and this is all the more surprising since the arguments were published in the 'Origin of Species,' which in other respects produced so profound an effect. In speculating as to the rocks in which the remains of the ancestors of the earliest known fossils may still exist, he suggested that, although the existing relationship between the positions of our present oceans and continental areas is of immense antiquity, there is no reason for the belief that it has persisted for an indefinite period, but that at some time long antecedent to the earliest known fossiliferous rocks 'continents may have existed where oceans are now spread out; and clear and open oceans may have existed where our continents now stand.' Not the least interesting result would be the test of this hypothesis, which would probably be forthcoming as the result of boring into the floor of a deep ocean; for although, as Darwin pointed out, it is likely enough that such rocks would be highly metamorphosed, yet it might still be possible to ascertain whether they had at any time formed part of a continental deposit, and perhaps to discover much more than this. Such an undertaking might be carried out in conjunction with other investigations of the highest interest, such as the attempt to obtain a record of the swing of a pendulum at the bottom of the ocean.

We now come to the strictly biological part of our subject—to the inquiry as to how much of the whole scheme of organic evolution has been worked out in the time during which the fossiliferous rocks were formed, and how far, therefore, the time required by the geologist is sufficient.

It is first necessary to consider Lord Kelvin's attempt to rescue us from the dilemma in which we were placed by the insufficiency of time for evolution—the suggestion that life may have reached the earth on a meteorite. According to this view, the evolution which took place elsewhere may have been merely completed, in a comparatively brief space of time, on our earth.

We know nothing of the origin of life here or elsewhere, and our only attitude towards this or any other hypothesis on the subject is that of the anxious inquirer for some particle of evidence. But a few brief considerations will show that no escape from the demands for time can be gained in this way.

Our argument does not deal with the time required for the origin of life, or for the development of the lowest beings with which we are acquainted from the first formed beings, of which we know nothing. Both these processes may have required an immensity of time; but as we know nothing whatever about them, and have as yet no prospect of acquiring any information, we are compelled to confine ourselves to as much of the process of evolution as we can infer from the structure of living and fossil forms—that is, as regards animals, to the development of the simplest into the most complex Protozoa, the evolution of the Metazoa from the Protozoa, and the branching of the former into its numerous Phyla, with all their Classes, Orders, Families, Genera, and Species. But we shall find that this is quite enough to necessitate a very large increase in the time estimated by the geologist.

The Protozoa, simple and complex, still exist upon the earth in countless species, together with the Metazoan Phyla. Descendants of forms which in their day constituted the beginning of that scheme of evolution which I have defined above, descendants, furthermore, of a large proportion of those forms which, age after age, constituted the shifting phases of its onward progress, still exist, and in a sufficiently unmodified condition to enable us to reconstruct, at any rate in mere outline, the history of the past. Innumerable details and many phases of supreme importance are still hidden from us, some of them perhaps never to be recovered. But this frank admission, and the eager and premature attempts to expound too much, to go further than the evidence permits, must not be allowed

to throw an undeserved suspicion upon conclusions which are sound and well supported, upon the firm conviction of every zoologist that the general trend of evolution has been, as I have stated it, that each of the Metazoan Phyla originated, directly or indirectly, in the Protozoa.

The meteorite theory would, however, require that the process of evolution went backward on a scale as vast as that on which it went forward, that certain descendants of some central type, coming to the earth on a meteorite, gradually lost their Metazoan complexity and developed backward into the Protozoa, throwing off the lower Metazoan Phyla on the way, while certain other descendants evolved all the higher Metazoan groups. Such a process would shorten the period of evolution by half, but it need hardly be said that all available evidence is entirely against it.

The only other assumption by means of which the meteorite hypothesis would serve to shorten the time is even more wild and improbable. Thus it might be supposed that the evolution which we believe to have taken place on this earth, really took place elsewhere—at any rate as regards all its main lines—and that samples of all the various phases, including the earliest and simplest, reached us by a regular meteoric service, which was established at some time after the completion of the scheme of organic evolution. Hence the evidences which we study would point to an evolution which occurred in some unknown world with an age which even Professor Tait has no desire to limit.

If these wild assumptions be rejected, there remains the supposition that, if life was brought by a meteorite, it was life no higher than that of the simplest Protozoon—a supposition which leaves our argument intact. The alternative supposition, that one or more of the Metazoan Phyla were introduced in this way while the others were evolved from the terrestrial Protozoa, is hardly worth consideration. In the first place, some evidence of a part in a common scheme of evolution is to be found in every Phylum. In the second place, the gain would be small; the arbitrary assumption would only affect the evidence of the time required for evolution derived from the particular Phylum or Phyla of supposed meteoric origin.

The meteoric hypothesis, then, can only affect our argument by making the most improbable assumptions, for which, moreover, not a particle of evidence can be brought forward.

We are therefore free to follow the biological evidence fearlessly. It is necessary, in the first place, to expand somewhat the brief outline of the past history of the animal kingdom, which has already been given. Since the appearance of the 'Origin of Species,' the zoologist, in making his classifications, has attempted as far as possible to set forth a genealogical arrangement. Our purpose will be served by an account of the main outlines of a recent classification, which has been framed with a due consideration for all sides of zoological research, new and old, and which has met with general approval. Professor Lankester divides the animal kingdom into two grades, the higher of which, the Enterozoa (Metazoa), were derived from the lower, the Plastidozoa (Protozoa). Each of these grades is again divided into two sub-grades, and each of these is again divided into Phyla, corresponding more or less to the older Sub-Kingdoms. Beginning from below, the most primitive animals in existence are found in the seven Phyla of the lower Protozoan sub-grade, the Gymnomyxa. Of these unfortunately only two, the Reticularia (Foraminifera) and Radiolaria, possess a structure which renders possible their preservation in the rocks. The lowest and simplest of these Gymnomyxa represent the starting-point of that scheme of organic evolution which we are considering to-day. The higher order of Protozoan life, the sub-grade Corticata, contains three Phyla, no one of which is available in the fossil state. They are, however, of great interest and importance to us as showing that the Protozoan type assumes a far higher organisation on its way to evolve the more advanced grade of animal life. The first-formed of these latter are contained in the two Phyla of the sub-grade Cœlentera, the Porifera or Sponges, and the Nematophora or Corals, Sea-Anemones, Hydrozoa and allied groups. Both of these Phyla are plentifully represented in the fossil state. It is considered certain that the latter of these, the Nematophora,

gave rise to the higher sub-grade, the Cœlomata, or animals with a cœlom or body-cavity surrounding the digestive tract. This latter includes all the remaining species of animals in nine Phyla, five of which are found fossil—the Echinoderma, Gephyrea, Mollusca, Appendiculata, and Vertebrata.

Before proceeding further, I wish to lay emphasis on the immense evolutionary history which must have been passed through before the ancestor of one of the higher of these nine Phyla came into being. Let us consider one or two examples, since the establishment of this position is of the utmost importance for our argument. First, consider the past history of the Vertebrata,—of the common ancestor of our Balanoglossus, Tunicates, Amphioxus, Lampreys, Fishes, Dipnoi, Amphibia, Reptiles, Birds, and Mammals. Although zoologists differ very widely in their opinions as to the affinities of this ancestral form, they all agree in maintaining that it did not arise direct from the Nematophora in the lower sub-grade of Metazoa, but that it was the product of a long history within the Cœlomate sub-grade. The question as to which of the other Cœlomate Phyla it was associated with will form the subject of one of our discussions at this meeting, and I will therefore say no more upon this period of its evolution, except to point out that the very question itself, 'the ancestry of Vertebrates,' only means a relatively small part of the evolutionary history of the Vertebrate ancestor within the Cœlomate group. For when we have decided the question of the other Cœlomate Phylum or Phyla to which the ancestral Vertebrate belonged, there remains of course the history of that Phylum or those Phyla earlier than the point at which the Vertebrate diverged, right back to the origin of the Cœlomata; while, beyond and below, the wide gulf between this and the Cœlentera had to be crossed, and then, probably after a long history as a Cœlenterate, the widest and most significant of all the morphological intervals—that between the lowest Metazoon and the highest Protozoon—was traversed. But this was by no means all. There remains the history within the higher Protozoan sub-grade, in the interval from this to the lower, and within the lower sub-grade itself, until we finally retrace our steps to the lowest and simplest forms. It is impossible to suppose that all this history of change can have been otherwise than immensely prolonged, for it will be shown below that all the available evidence warrants the belief that the changes during these earlier phases were at least as slow as those which occurred later.

If we take the history of another of the higher Phyla, the Appendiculata, we find that the evidence points in the same direction. The common ancestor of our Rotifera, earthworms, leeches, Peripatus, centipedes, insects, Crustacea, spiders and scorpions, and forms allied to all these, is generally admitted to have been Chætopod-like, and probably arose in relation to the beginnings of certain other Cœlomata Phyla, such as the Gephyrea and perhaps Mollusca. At the origin of the Cœlomate sub-grade the common ancestor of all Cœlomate Phyla is reached, and its evolution has been already traced in the case of the Vertebrata.

What is likely to be the relation between the time required for the evolution of the ancestor of a Cœlomate Phylum and that required for the evolution, which subsequently occurred, within the Phylum itself? The answer to this question depends mainly upon the rate of evolution in the lower parts of the animal kingdom as compared with that in the higher. Contrary, perhaps, to anticipation, we find that all the evidences of rapid evolution are confined to the most advanced of the smaller groups within the highest Phyla, and especially in the higher Classes of the Vertebrata. Such evidence as we have strongly indicates the most remarkable persistence of the lower animal types. Thus in the Class Imperforata of the Reticularia (Foraminifera) one of our existing genera (*Saccamina*) occurs in the Carboniferous strata, another (*Trochammina*) in the Permian, while a single new genus (*Receptaculites*) occurs in the Silurian and Devonian. The evidence from the Class Perforata is much stronger, the existing genera *Nodosaria, Dentalina, Textularia, Grammostomum, Valvulina*, and *Nummulina* all occurring in the Carboniferous, together with the new genera *Archædiscus* (?) and *Fusulina*.

I omit reference to the much-disputed Eozoon from the Laurentian rocks far below the horizon, which for the purpose of this address I am considering as the

lowest fossiliferous stratum.   We are looking forward to the new light which will
be thrown upon this form in the communication of its veteran defender, Sir
William Dawson, whom we are all glad to welcome.

Passing the Radiolaria, with delicate skeletons less suited for fossilisation, and
largely pelagic and therefore less likely to reach the strata laid down along the
fringes of the continental areas, the next Phylum which is found in a fossil state
is that of the Porifera, including the sponges, and divided into two Classes, the
Calcispongiæ and Silicospongiæ.   Although the fossilisation of sponges is in many
cases very incomplete, distinctly recognisable traces can be made out in a large
number of strata.   From these we know that representatives of all the groups of
both Classes (except the Halisarcidæ, which have no hard parts) occurred in the
Silurian, Devonian, and Carboniferous systems.   The whole Phylum is an example of
long persistence with extremely little change.   And the same is true of the Nema-
tophora : new groups indeed come in, sometimes extremely rich in species, such as
the Palæozoic Rugose corals and Graptolites; but they existed side by side with
representatives of existing groups, and they are not in themselves primitive or
ancestral.   A study of the immensely numerous fossil corals reveals no advance in
organisation, while researches into the structure of existing Alcyonaria and Hydro-
corallina have led to the interpretation of certain Palæozoic forms which were pre-
viously obscure, and the conclusion that they find their place close beside the
living species.

All available evidence points to the extreme slowness of progressive evolu-
tionary changes in the Cœlenterate Phyla, although the Protozoa, if we may judge
by the Reticularia (Foraminifera), are even more conservative.

When we consider later on the five Cœlomate Phyla which occur fossil, we
shall find that the progressive changes were slower and indeed hardly appreciable
in the two lower and less complex Phyla, viz. : the Echinoderma, and Gephyrea,
as compared with the Mollusca, Appendiculata, and Vertebrata.

Within these latter Phyla we have evidence for the evolution of higher groups
presenting a more or less marked advance in organisation.   And not only is the
rate of development more rapid in the highest Phyla of the animal kingdom, but
it appears to be most rapid when dealing with the highest animal tissue, the
central nervous system.   The chief, and doubtless the most significant, difference
between the early Tertiary mammals and those which succeeded them, between the
Secondary and Tertiary reptiles, between man and the mammals most nearly
allied to him, is a difference in the size of the brain.   In all these cases an enormous
increase in this, the dominant tissue of the body, has taken place in a time which,
geologically speaking, is very brief.

When speaking later on upon the evolution which has taken place within the
Phyla, further details upon this subject will be given, although in this as in other
cases the time at our disposal demands that the exposition of evidence must
largely yield to an exposition of the conclusions which follow from its study.
And undoubtedly a study of all the available evidence points very strongly to the
conclusion that in the lower grade, sub-grades, and Phyla of the animal kingdom
evolution has been extremely slow as compared with that in the higher.   We do
not know the reason.   It may be that this remarkable persistence through the
stratified series of deposits is due to an innate fixity of constitution which has
rigidly limited the power of variation; or, more probably perhaps, that the lower
members of the animal kingdom were, as they are now, more closely confined to
particular environments, with particular sets of conditions, with which they had to
cope, and, this being successfully accomplished, natural selection has done little
more than keep up a standard of organisation which was sufficient for their needs;
while the higher and more aggressive forms ranging over many environments, and
always prone to encounter new sets of conditions, were compelled to undergo respon-
sive changes or to succumb.   But whatever be the cause, the fact remains, and is of
the highest importance for our argument.   When the ancestor of one of the higher
Phyla was associated with the lower Phyla of the Cœlomate sub-grade, when
further back it passed through a Cœlenterate, a higher Protozoan, and finally a
lower Protozoan phase, we must believe that its evolution was probably very slow

as compared with the rate which it subsequently attained. But this conclusion is of the utmost importance; for the history contained in the stratified rocks nowhere reveals to us the origin of a Phylum. And this is not mere negative evidence, but positive evidence of the most unmistakable character. All the five Cœlomate Phyla which occur fossil appear low down in the Palæozoic rocks, in the Silurian or Cambrian strata, and they are represented by forms which are very far from being primitive, or, if primitive, are persistent types, such as Chiton, which are now living. Thus Vertebrata are represented by fishes, both sharks and ganoids; the Appendiculata by cockroaches, scorpions, Limulids, Trilobites, and many Crustacea; the Mollusca by Nautilus and numerous allied genera, by Dentalium, Chiton, Pteropods, and many Gastropods and Lamellibranchs: the Gephyrea by very numerous Brachiopods, and many Polyzoa; the Echinoderma by Crinoids, Cystoids, Blastoids, Asteroids, Ophiuroids, and Echinoids. It is just conceivable, although, as I believe, most improbable, that the Vertebrate Phylum originated at the time when the earliest known fossiliferous rocks were laid down. It must be remembered, however, that an enormous morphological interval separates the fishes which appear in the Silurian strata from the lower branches, grades, and classes of the Phylum in which Balanoglossus, the Ascidians, Amphioxus, and the Lampreys are placed. The earliest Vertebrates to appear are, in fact, very advanced members of the Phylum, and, from the point of view of anatomy, much nearer to man than to Amphioxus. If, however, we grant the improbable contention that so highly organised an animal as a shark could be evolved from the ancestral vertebrate in the period which intervened between the earliest Cambrian strata and the Upper Silurian, it is quite impossible to urge the same with regard to the other Phyla. It has been shown above that when these appear in the Cambrian and Silurian, they are flourishing in full force, while their numerous specialised forms are a positive proof of a long antecedent history within the limits of the Phylum.

If, however, we assume for the moment that the Phyla began in the Cambrian, the geologist's estimate must still be increased considerably, and perhaps doubled, in order to account for the evolution of the higher Phyla from forms as low as many which are now known upon the earth; unless, indeed, it is supposed, against the whole weight of all such evidence as is available, that the evolutionary history in these early times was comparatively rapid.

To recapitulate, if we represent the history of animal evolution by the form of a tree, we find that the following growth took place in some age antecedent to the earliest fossil records, before the establishment of the higher Phyla of the Animal Kingdom. The main trunk representing the lower Protozoa divided, originating the higher Protozoa, the latter portion again divided, probably in a threefold manner, originating the two lowest Metazoan Phyla, constituting the Cœlentera. The branch representing the higher of these Phyla, the Nematophora, divided, originating the lower Cœlomate Phyla, which again branched and originated the higher Phyla. And, as has been shown above, the relatively ancestral line, at every stage of this complex history, after originating some higher line, itself continued down to the present day, throughout the whole series of fossiliferous rocks, with but little change in its general characters, and practically nothing in the way of progressive evolution. Evidences of marked advance are to be found alone in the most advanced groups of the latest highest products—the Phyla formed by the last of these divisions.

It may be asked how is it possible for the zoologist to feel so confident as to the past history of the various animal groups. I have already explained that he does not feel this confidence as regards the details of the history, but as to its general lines. The evidence which leads to this conviction is based upon the fact that animal structure and mode of development can be, and have been, handed down from generation to generation from a period far more remote than that which is represented by the earliest fossils; that fundamental facts in structure and development may remain changeless amid endless changes of a more general character; that especially favourable conditions have preserved ancestral forms comparatively unchanged. Working upon this material, com-

parative anatomy and embryology can reconstruct for us the general aspects of a history which took place long before the Cambrian rocks were deposited  This line of reasoning may appear very speculative and unsound, and it may easily become so when pressed too far.  But applied with due caution and reserve, it may be trusted to supply us with an immense amount of valuable information which cannot be obtained in any other way.  Furthermore, it is capable of standing the very true and searching test supplied by the verification of predictions made on its authority.  Many facts taken together lead the zoologist to believe that A was descended from C through B ; but if this be true, B should possess certain characters which are not known to belong to it.  Under the inspiration of hypothesis a more searching investigation is made, and the characters are found.  Again, that relatively small amount of the whole scheme of animal evolution which is contained in the fossiliferous rocks has furnished abundant confirmation of the validity of the zoologist's method  The comparative anatomy of the higher Vertebrate Classes leads the zoologist to believe that the toothless beak and the fused caudal vertebræ of a bird were not ancestral characters, but were at some time derived from a condition more conformable to the general plan of vertebrate construction, and especially to that of reptiles.  Numerous secondary fossils prove to us that the birds of that time possessed teeth and separate caudal vertebræ, culminating in the long lizard-like tail of Archæopteryx.

Prediction and confirmation of this kind, both zoological and palæontological, have been going on ever since the historic point of view was adopted by the naturalist as the outcome of Darwin's teaching, and the zoologist may safely claim that his method, confirmed by palæontology so far as evidence is available, may be extended beyond the period in which such evidence is to be found.

And now our last endeavour must be to obtain some conception of the amount of evolution which has taken place within the higher Phyla of the Animal Kingdom during the period in which the fossiliferous rocks were deposited.  The evidence must necessarily be considered very briefly, and we shall be compelled to omit the Vertebrata altogether.

The Phylum Appendiculata is divided by Lankester into three branches, the first containing the Rotifera, the second the Chætopoda, the third the Arthropoda.  Of these the second is the oldest, and gave rise to the other two, or at any rate to the Arthropoda, with which we are alone concerned, inasmuch as the fossil records of the others are insufficient  The Arthropoda contain seven Classes, divided into two grades, according to the presence or absence of antennæ—the Ceratophora, containing the Peripatoidea, the Myriapoda, and the Hexapoda (or insects) ; the Acerata, containing the Crustacea, Arachnida, and two other classes (the Pantopoda and Tardigrada) which we need not consider.  The first Class of the antenna-bearing group contains the single genus Peripatus—one of the most interesting and ancestral of animals, as proved by its structure and development, and by its immense geographical range.  Ever since the researches of Moseley and Balfour, extended more recently by those of Sedgwick, it has been recognised as one of the most beautiful of the connecting links to be found amongst animals, uniting the antenna-bearing Arthropods, of which it is the oldest member, with the Chætopods.  Peripatus is a magnificent example of the far-reaching conclusions of zoology, and of its superiority to palæontology as a guide in unravelling the tangled history of animal evolution.  Peripatus is alive to-day, and can be studied in all the details of its structure and development ; it is infinitely more ancestral, and tells of a far more remote past than any fossil Arthropod, although such fossils are well known in all the older of the Palæozoic rocks.  And yet Peripatus is not known as a fossil.  Peripatus has come down, with but little change, from a time, on a moderate estimate, at least twice as remote as the earliest known Cambrian fossil.  The agencies which, it is believed, have crushed and heated the Archæan rocks so as to obliterate the traces of life which they contained were powerless to efface this ancient type, for, although the passing generations may have escaped record, the likeness of each was stamped on that which succeeded it, and has continued down to the present day.  It is, of course, a perfectly trite and obvious conclusion,

but not the less one to be wondered at, that the force of heredity should thus far outlast the ebb and flow of terrestrial change throughout the vast period over which the geologist is our guide.

If, however, the older Palæozoic rocks tell us nothing of the origin of the antenna-bearing Arthropods, what do they tell us of the history of the Myriapod and Hexapod Classes?

The Myriapods are well represented in Palæozoic strata, two species being found in the Devonian and no less than thirty-two in the Carboniferous. Although placed in an Order (Archipolypoda) separate from those of living Myriapods, these species are by no means primitive, and do not supply any information as to the steps by which the Class arose. The imperfection of the record is well seen in the traces of this Class; for between the Carboniferous rocks and the Oligocene there are no remains of undoubted Myriapods.

We now come to the consideration of insects, of which an adequate discussion would occupy a great deal too much of your time. An immense number of species are found in the Palæozoic rocks, and these are considered by Scudder, the great authority on fossil insects, to form an Order, the Palæodictyoptera, distinct from any of the existing Orders. The latter, he believes, were evolved from the former in Mesozoic times. These views do not appear to derive support from the wonderful discoveries of M. Brongniart [1] in the Upper Carboniferous of Commentry in the Department of Allier in Central France. Concerning this marvellous assemblage of species, arranged by their discoverer into 46 genera and 101 species, Scudder truly

' Our knowledge of Palæozoic insects will have been increased three or fourfold at a single stroke. . . No former contribution in this field can in any way compare with it, nor even all former contributions taken together.' [2]

When we remember that the group of fossil insects, of which so much can be affirmed by so great an authority as Scudder, lived at one time and in a single locality, we cannot escape the conclusion that the insect fauna of the habitable earth during the whole Palæozoic period was of immense importance and variety. Our knowledge of this single group of species is largely due to the accident that coal-mining in Commentry is carried on in the open air.

Now, these abundant remains of insects, so far from upholding the view that the existing orders had not been developed in Palæozoic times, are all arranged by Brongniart in four out of the nine Orders into which insects are usually divided, viz. the Orthoptera, Neuroptera, Thysanoptera, and Homoptera. The importance of the discovery is well seen in the Neuroptera, the whole known Palæozoic fauna of this Order being divided into 45 genera and 99 species, of which 33 and 72 respectively have been found at Commentry.

Although the Carboniferous insects of Commentry are placed in new families, some of them come wonderfully near those into which existing insects are classified, and obviously form the precursors of these. This is true of the Blattidæ, Phasmidæ, Acridiidæ, and Locustidæ among the Orthoptera, the Perlidæ among the Neuroptera, and the Fulgoridæ among the Homoptera. The differences which separate these existing families from their Carboniferous ancestors are most interesting and instructive. Thus the Carboniferous cockroaches possessed ovipositors, and probably laid their eggs one at a time, while ours are either viviparous or lay their eggs in a capsule. The Protophasmidæ resemble living species in the form of the head, antennæ, legs, and body; but while our species are either wingless or, with the exception of the female Phyllidæ, have the anterior pair reduced to tegmina, useless for flight, those of Palæozoic times possessed four well-developed wings  The forms representing locusts and grasshoppers (Palæacridiidæ) possessed long slender antennæ like the green grasshoppers (Locustidæ), from which the Acridiidæ are now distinguished by their short antennæ  The divergence and specialisation which is thus shown is amazingly small in amount. In

[1] Charles Brongniart.—' Recherches pour servir à l'Histoire des Insectes fossiles des temps primaires, précédées d'une Etude sur la nervation des ailes des Insectes. 1894.

[2] S. H. Scudder, *Am. Journ. Sci.* vol. xlvii , February 1894   Art. viii.

the vast period between the Upper Carboniferous rocks and the present day the cock-roaches have gained a rather different wing venation, and have succeeded in laying their eggs in a manner rather more specialised than that of insects in general ; the stick insects and leaf insects have lost or reduced their wings, the grasshoppers have shortened their antennæ. These, however, are the insects which most closely resemble the existing species , let us turn to the forms which exhibit the greatest differences. Many species have retained in the adult state characters which are now confined to the larval stage of existence, such as the presence of tracheal gills on the sides of the abdomen. In some, the two membranes of the wing were not firmly fixed together, so that the blood could circulate freely between them. On the other hand, they are not very firmly fixed together in existing insects Another important point was the condition of the three thoracic segments, which were quite distinct and separate, instead of being fused as they are now in the imago stage. This external difference probably also extended to the nervous system, so that the thoracic ganglia were separate instead of concentrated. The most interesting distinction, however, was the possession by many species of a pair of prothoracic appendages much resembling miniature wings, and which especially suggest the appearance assumed by the anterior pair (tegmina) in existing Phasmidæ There is some evidence in favour of the view that they were articulated, and they exhibit what appears to be a trace of venation. Brongniart concludes that in still earlier strata, insects with six wings will be discovered, or rather insects with six of the tracheal gills sufficiently developed to serve as parachutes. Of these, the two posterior pair developed into the wings as we know them, while the anterior pair degenerated, some of the Carboniferous insects presenting us with a stage in which degeneration had taken place but was not complete.

One very important character was, as I have already pointed out, the enormous size reached by insects in this distant period. This was true of the whole known fauna as compared with existing species, but it was especially the case with the Protodonata, some of these giant dragon-flies measuring over two feet in the expanse of the wings

As regards the habits of life and metamorphoses, Brongniart concludes that some species of Protoephemeridæ, Protoperlidæ, &c., obtained their food in an aquatic larval stage, and did not require it when mature. He concludes that the Protodonata fed on other animals, like our dragon-flies ; that the Palæacridiidæ were herbivorous like our locusts and grasshoppers. the Protolocustidæ herbivorous and animal feeders like our green grasshoppers, the Palæoblattidæ omnivorous like our cockroaches. The Homoptera, too, had elongated sucking mouth-parts like the existing species. It is known that in Carboniferous times there was a lake with rivers entering it, at Commentry. From their great resemblance to living forms of known habits, it is probable that the majority of these insects lived near the water and their larvæ in it.

When we look at this most important piece of research as a whole, we cannot fail to be struck with the small advance in insect structure which has taken place since Carboniferous times. All the great questions of metamorphosis, and of the structures peculiar to insects, appear to have been very much in the position in which they are to-day. It is indeed probable enough that the Orders which zoologists have always recognised as comparatively modern and specialised, such as the Lepidoptera, Coleoptera, and Hymenoptera, had not come into existence. But as regards the emergence of the Class from a single primitive group, as regards its approximation towards the Myriapods, which lived at the same time, and of both towards their ancestor Peripatus, we learn absolutely nothing. All we can say is that there is evidence for the evolution of the most modern and specialised members of the Class, and some slight evolution in the rest. Such evolution is of importance as giving us some vague conception of the rate at which the process travels in this division of the Arthropoda. If we look upon development as a series of paths which, by successively uniting, at length meet in a common point, then some conception of the position of that distant centre may be gained by measuring the angle of divergence and finding the number of unions which occur in a given length. In this case, the amount of approximation and union shown in

the interval between the Carboniferous Period and the present day is relatively so small that it would require to be multiplied many times before we could expect the lines to meet in the common point, the ancestor of insects, to say nothing of the far more distant past. in which the Tracheate Arthropods met in an ancestor presenting many resemblances to Peripatus. But it must not be forgotten that all this vast undefined period is required for the history of one of the two grades of one of the three branches of the whole Phylum.

Turning now to the brief consideration of the second grade of Arthropods, distinguished from the first grade by the absence of antennæ, the Trilobites are probably the nearest approach to an ancestral form met with in the fossil state. Now that the possession of true antennæ is certain, it is reasonable to suppose that the Trilobites represent an early Class of the Aceratous branch which had not yet become Aceratous They are thus of the deepest interest in helping us to understand the origin of the antennaless branch, not by the ancestral absence, but by the loss of true antennæ which formerly existed in the group. But the Trilobites did not themselves originate the other Classes, at any rate during Palæozoic times. They represent a large and dominant Class, presenting more of the characters of the common ancestor than the other Classes; but the latter had diverged and had become distinct long before the earliest fossiliferous rocks; for we find well-marked representatives of the Crustacea in Cambrian, and of the Arachnida in Silurian strata. The Trilobites, moreover, appear in the Cambrian with many distinct and very different forms, contained in upwards of forty genera, so that we are clearly very far from the origin of the group.

Of the lower group of Crustacea, the Entomostraca, the Cirripedes are represented by two genera in the Silurian, the Ostracodes by four genera in the Cambrian and over twenty in the Silurian; of these latter two genera, Cythere and Bairdia, continue right through the fossiliferous series and exist at the present day. Remains of Phyllopods are more scanty, but can be traced in the Devonian and Carboniferous rocks. The early appearance of the Cirripedes is of especial interest, inasmuch as the fixed condition of these forms in the mature state is certainly not primitive, and yet, nevertheless, appears in the earliest representatives.

The higher group, the Malacostraca, are represented by many genera of Phyllocarida in the Silurian and Devonian, and two in the Cambrian. These also afford a good example of the imperfection of the record, inasmuch as no traces of the group are to be found between the Carboniferous and our existing fauna in which it is represented by the genus Nebalia. The Phyllocarida are recognised as the ancestors of the higher Malacostraca, and yet these latter already existed— in small numbers, it is true—side by side with the Phyllocarida in the Devonian. The evolution of the one into the other must have been much earlier. Here, as in the Arthropoda, we have evidence of progressive evolution among the highest groups of the Class, as we see in the comparatively late development of the Brachyura as compared with the Macrura. We find no trace of the origin of the Class, or of the larger groups into which it is divided, or, indeed, of the older among the small groupings into families and genera.[1]

Of the Arachnida, although some of the most wonderful examples of persistent types are to be found in this class, but little can be said. Merely to state the bare fact that three kinds of scorpion are found in the Silurian, two Pedipalpi, eight scorpions, and two spiders in the Carboniferous, is sufficient to show that the period computed by geologists must be immensely extended to account for the development of this Class alone, inasmuch as it existed in a highly specialised condition almost at the beginning of the fossiliferous series; while, as regards so extraordinarily complex an animal as a scorpion, nothing apparent in the way of progressive development has happened since. Professor Lankester has, however, pointed out to me that the Silurian scorpions possessed heavier limbs than those of existing species, and this is a point in favour of their having been aquatic, like their near relation, Limulus. If so, it is probable that they possessed external

[1] For an account of the evolution of the Crustacea see the Presidential Addresses to the Geological Society in 1895 and 1896 by Dr. Henry Woodward.

gills, not yet inverted to form the lung-book. The Merostomata are of course a Palæozoic group, and reach their highest known development at their first appearance in the Silurian ; since then they have done nothing but disappear gradually, leaving the single genus Limulus, unmodified since its first appearance in the Trias, to represent them. It is impossible to find clearer evidence of the decline rather than the rise of a group. No progressive development, but a gradual or rapid extinction, and consequent reduction in the number of genera and species, is a summary of the record of the fossiliferous rocks as regards this group and many others, such as the Trilobites, the Brachiopods, and the Nautilidæ All these groups begin with many forms in the oldest fossiliferous rocks, and three of them have left genera practically unchanged from their first appearance to the present day What must have been the time required to carry through the vast amount of structural change implied in the origin of these persistent types and the groups to which they belong—a period so extended that the interval between the oldest Palæozoic rocks and the present day supplies no measurable unit?

But I am digressing from the Appendiculate Phylum. We have seen that the fossil record is unusually complete as regards two Classes in each grade of the Arthropod branch, but that these Classes were well developed and flourishing in Palæozoic times. The only evidence of progressive evolution is in the development of the highest orders and families of the Classes. Of the origin of the Classes nothing is told, and we can hardly escape the conclusion that for the development of the Arthropod branches from a common Chætopod-like ancestor, and for the further development of the Classes of each branch, a period many times the length of the fossiliferous series is required, judging from the insignificant amount of development which has taken place during the formation of this series

It is impossible to consider the other Cœlomate Phyla as I have done the Appendiculata. I can only briefly state the conclusions to which we are led

As regards the Molluscan Phylum, the evidence is perhaps even stronger than in the Appendiculata Representatives of the whole of the Classes are, it is believed, found in the Cambrian or Lower Silurian The Pteropods are generally admitted to be a recent modification of the Gastropods, and yet, if the fossils described in the genera Conularia, Hyolithes, Pterotheca, &c. are true Pteropods, as they are supposed to be, they occur in the Cambrian and Silurian strata, while the group of Gastropods from which they almost certainly arose, the Bullidæ, are not known before the Trias. Furthermore, the forms which are clearly the oldest of the Pteropods—Limacina and Spirales—are not known before the beginning of the Tertiary Period Either there is a mistake in the identification of the Palæozoic fossils as Pteropods, or the record is even more incomplete than usual, and the most specialised of all Molluscan groups had been formed before the date of the earliest fossiliferous rocks. If this should hereafter be disproved, there can be no doubt about the early appearance of the Molluscan Classes, and that it is the irony of an incomplete record which places the Cephalopods and Gastropods in the Cambrian and the far more ancestral Chiton no lower than the Silurian. Throughout the fossiliferous series the older families of Gastropods and Lamellibranchs are followed by numerous other families, which were doubtless derived from them , new and higher groups of Cephalopods were developed, and, with the older groups, either persisted until the present time or became extinct. But in all this splitting up of the Classes into groups of not widely different morphological value, there is very little progressive modification, and, taking such changes in such a period as our unit for the determination of the time which was necessary for the origin of the Classes from a form like Chiton, we are led to the same conclusion as that which followed from the consideration of the Appendiculata, viz. that the fossiliferous series would have to be multiplied several times in order to provide it.

Of the Phylum Gephyrea, I will only mention the Brachiopods, which are found in immense profusion in the early Palæozoic rocks and which have occupied the subsequent time in becoming less dominant and important. So far from helping us to clear up the mystery which surrounds the origin of the Class, the earliest forms are quite as specialised as those living now, and, some of them (Lingula

Discina) even generically identical    The demand for time to originate the group is quite as grasping as that of the others we have been considering

All the Classes of Echinoderma, except the Holothurians, which do not possess a structure favourable for fossilisation, are found early in the Palæozoic rocks, and many of them in the Cambrian    Although these early forms are very different from those which succeeded them in the later geological periods, they do not possess a structure which can be recognised as in any way primitive or ancestral.    The Echinoderma are the most distinct and separate of all the Cœlomate Phyla, and they were apparently equally distinct and separate at the beginning of the fossiliferous series.

In concluding this imperfect attempt to deal with a very vast subject in a very short time, I will remind you that we were led to conclude that the evolution of the ancestor of each of the higher animal Phyla, probably occupied a very long period, perhaps as long as that required for the evolution which subsequently occurred within the Phylum    But the consideration of the higher Phyla which occur fossil, except the Vertebrata, leads to the irresistible conclusion that the whole period in which the fossiliferous rocks were laid down must be multiplied several times for this later history alone.    The period thus obtained requires to be again increased, and perhaps doubled, for the earlier history

In the preparation of the latter part of this address I have largely consulted Zittel's great work.    I wish also to express my thanks to my friend Professor Lankester, whom I have consulted on many of the details, as well as the general plan which has been adopted.

# British Association for the Advancement of Science.

## LIVERPOOL, 1896.

# ADDRESS

### TO THE

# GEOGRAPHICAL SECTION,

### BY

### Major DARWIN, Sec. R.G.S.,

#### PRESIDENT OF THE SECTION.

In reviewing the record of geographical work during the past year, all other performances pale in comparison with the feat accomplished by Nansen. It is not merely that he has gone considerably nearer the North Pole than any other explorer, it is not only that he has made one of the most courageous expeditions ever recorded, but he has established the truth of his theory of Polar currents, and has brought back a mass of valuable scientific information. When Nansen comes to England I am certain that we shall give him a reception which will prove how much we admire the heroism of this brave Norwegian.

Besides the news of this most remarkable achievement, the results of a considerable amount of useful exploratory work have been published since the British Association met last at Ipswich. With regard to other Arctic Expeditions, we have had the account of Lieutenant Peary's third season in Northern Greenland, from which place he came back in September last, and to which he has again returned, though without the intention of passing another winter there. In October the 'Windward' brought home more ample information as to the progress of the Jackson-Harmsworth Expedition than that communicated by telegram to the Association at Ipswich, and on her return from her remarkably rapid voyage this summer she brought back the record of another year. As to geographical work in Asia, Mr. and Mrs. Littledale returned safely from their explorations of the little known parts of Tibet; the Pamir Boundary Commission, under Colonel Holdich, has collected a great deal of accurate topographical information in the course of its labours; Dr. Sven Hedin continues his important researches in Turkestan; and the Royal Geographical Society was glad to welcome Prince Henry of Orleans when he came to tell us about his journey near the sources of the Irrawaddy. As to Africa, the most important additions to our knowledge of that continent are due to the French surveyors, who have accurately mapped the recently discovered series of lakes in the neighbourhood of Timbuktu, Lake Faguibine, the largest, being found to be 68 miles in length; Dr. Donaldson Smith has filled up some large blanks in the map of Somaliland; and Mr. and Mrs. Theodore Bent have investigated some interesting remains of ancient gold workings inland of the Red Sea. In other parts of the world less has been done, because there is less to do. Mr. Fitzgerald has proved for the first time the practicable character of a pass across the Southern Alps, thus supplementing the excellent work of Mr. Harper and other pioneers of the New Zealand Alpine Club; and Sir W. M. Conway has commenced a systematic exploration of the interior of Spitzbergen, a region to which the attention of several other geographers is also directed.

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It is impossible in such a brief sketch to enumerate even the leading events of the geographical year, but what I have said is enough to remind us of the great amount of valuable and useful work which is being done in many quarters of the world. It is true that if we compare this record with the record of years gone by, we find a marked difference. Then, there was always some great geographical problem to be attacked; the sources of the Nile had to be discovered; the course of the Niger had to be traced; and the great white patches on our maps stimulated the imagination of explorers with the thought of all sorts of possibilities. Now, though there is much to be learned, yet, with the exception of the Poles, the work will consist in filling in the details of the picture, the general outlines being all drawn for us already. Personally I cannot help feeling a completely unreasoning regret that we have almost passed out of the heroic period of geography. Whatever the future may have in store for us, it can never give us another Columbus, another Magellan, or another Livingstone. The geographical discoverers of the future will win their fame in a more prosaic fashion, though their work may in reality be of even greater service to mankind. There are now few places in the world where the outline of the main topographical features is unknown; but, on the other hand, there are vast districts not yet thoroughly examined. And, in examining these more or less known localities, geographers must take a far wider view than heretofore of their methods of study in order to accommodate themselves to modern conditions.

But even if we confine our attention to the older and more narrow field of geography, it will be seen that there is still an immense amount of work to be done. We have been filling in the map of Africa during recent years with extraordinary rapidity, but yet that map is likely to remain in a very unsatisfactory condition for a long time to come. Englishmen and other Europeans have always shown themselves to be ready to risk their lives in exploring unknown regions, but we have yet to see how readily they will undertake the plodding work of recording topographical details when little renown is to be won by their efforts. It should be one of the objects of geographical societies to educate the public to recognise the importance of this work, and General Chapman deserves great credit for bringing the matter before the International Congress last year in such a prominent manner. He confined himself to four main recommendations. (1) The extension of accurate topographical surveys in regions likely to be settled by Europeans. (2) The encouragement of travellers to sketch areas rather than routes. (3) The study of astronomical observations already taken in the unsurveyed parts of Africa in a systematic manner, and the publication of the results. (4) The accurate determination of the latitude and longitude of many important places in unsurveyed Africa. I am certain that all geographers are in hearty accord with General Chapman in his views, and it is, perhaps, by continually bringing this matter before the public, that we shall best help this movement forward.

Not only do we want a more accurate filling in of the picture, but we have yet to learn to read its lessons aright. The past cannot be understood, and still less can the future be predicted, without a wider conception of geographical facts. Look, for example, at the European Colonies on the West Coast of Africa. Here we find that there have been Portuguese settlements on the Gold Coast since the year 1471, the French possibly having been established there at an even earlier date; whilst we English, who pride ourselves on our go-ahead character, have had trading factories on the Coast since 1667. I have here a map showing the state of our geographical knowledge in 1815. Why was it that Europeans have never, broadly speaking, pushed into the interior from their base on the coast, which they had occupied for so many centuries? That they had not done so, at least to any purpose, is proved by this map. Why had four centuries of contact with Europeans done so little even for geographical knowledge at that time? The answer to this question may be said to be mainly historical; but the history of our African Colonies can never be understood without a study of the distribution of the dense belt of unhealthy forest along the shore; of the distribution of the different types of native inhabitants; and of the courses of the navigable rivers, all strictly geographical considerations.

Geography is the study of distribution, and early in that study we must be struck with the correlation of these different distributions. If we take a map of Africa, and mark on it all the areas within the tropics covered with dense forest or scrub, we shall find we have drawn a map showing accurately the distribution of the worst types of malarial fever; and that we have also indicated with some approach to accuracy—with, however, notable exceptions—the habitat of the lowest types of mankind. These are the facts which give the key to understanding why the progress of European colonisation on the West Coast has been so slow.

Along the coast of the Gulf of Guinea we find settlements of Europeans at more or less distant intervals. All along, or nearly all along this same coast, we find a wide belt of fever-stricken forest, fairly thickly inhabited by uncivilised Negro and Bantu tribes. Inside this belt of forest the country rises in altitude, and becomes more open, whilst at the same time there is a distinct improvement in the type of native; and the more we proceed inland, the more marked does this improvement become. There appear in fact to have been a number of waves of advancing civilisation, each one pressing the one in front of it towards these inhospitable forest belts. Near the coast the lowest type of negro is, generally speaking, to be found; then, as the more open country is reached, higher types of negroes are encountered—for example, the Mandingoes of the Senegal region are distinctly higher than the Jolas inhabiting the mouths of the Gambia; and the Hausas of the Sokoto Empire are vastly superior to the cannibals of the Oil Rivers. In both these cases the higher types are probably not pure negroes, but have Fulah, Berber, or Arab blood in their veins; for we see, in the case of the Fulahs, how they become absorbed into the race they are conquering; near the Senegal River they are comparatively light in colour, but in Adamawa they are hardly to be distinguished by their features from the negroes they despise. Thus the process appears to have been a double one; the higher race driving some of the lower aboriginal tribes before them out of the better lands, and, at the same time, raising other tribes by means of an admixture of better blood. These waves of advancing civilisation seem to have advanced from the north and east, for the more we penetrate in these directions, the higher is the type of inhabitant met with, until at last we reach the pure Berbers and the pure Arabs. Thus there are two civilising influences visible in this part of Africa; one coming from the north and east—a Mahommedan advance—which keeps beating up against this forest belt and occasionally breaking into it; the other, a Christian movement, which, until the middle of this century, was brought to a dead halt by this same obstacle. The map of Africa, showing the state of geographical knowledge in 1815, makes it clear that, except in a few cases where rivers helped travellers through these malarial regions, nothing was known about the interior. No doubt much has been done since those days, but this barrier still remains the great impediment to progress from the West Coast; and those who desire our influence to spread more effectively into the interior must wish to see some means of overcoming this obstacle. On the East Coast of Africa the conditions are somewhat different, as there is comparatively little dense forest there; but the districts near that coast are also usually unhealthy, and how to cross those malarial regions quickly into the healthy or less unhealthy interior is the most important problem connected with the development of tropical Africa.

Other influences have been at work, no doubt, in checking our progress from the West Coast. In old days, the European possessions in these districts were mere depôts for the export of slaves. As the white residents could not hope to compete with the natives in the actual work of catching these unfortunate creatures, and as the lower the type the more easily were they caught, as a rule, there was no reason whatever for attempting to penetrate into the interior, where the higher types are met with. But, though this export trade in human beings is now no longer an impediment to progress, the slave trade in the interior still helps to bar the way. When the forest belt is passed, we now come, generally speaking, to the line of demarcation between the Mahommedan and the Pagan tribes, and here slave catching is generally rife; when it is so, the constant raids of the Mahommedan chiefs keep these border districts in a state of unrest, which in every way tends to

impede progress. Thus a mere advance to the higher inland regions will not by any means solve all our difficulties; but it will greatly lessen them; and it is universally admitted that the more communication with the interior is facilitated, the more easy will it be to suppress this terrible traffic in human beings. By the General Act of the Brussels Anti-Slavery Conference of 1890–91, it was agreed by the assembled delegates that the construction of roads, and, in particular, of railways, connecting the advanced stations with the coast, and permitting easy access to the inland waters, and to the upper courses of rivers, was one of the most effective means of counteracting the slave trade in the interior. Here, then, we have the most formal admission which could be given of the necessity of opening up main trunk lines of communication into the interior.

But not only does geographical knowledge help to demonstrate the necessity of improving the means of communication between the coast and the interior, but it helps us to decide where it is wise to make our first efforts in this direction. In the first place, it is essential to note that if the Continent of Africa is compared with other Continents, its general poverty is clearly seen. Mr. Keltie, in his excellent work on the Partition of Africa, tells us that ' at present (1895) it is estimated that the total exports of the whole of Central Africa by the east and west coast do not amount to more than 20,000,000l. sterling annually.' For the purposes of comparison it may be mentioned that the export trade of India is between sixty and seventy millions sterling annually, and that India is only about one-seventh or one-eighth of the area of the whole of Africa. On the other hand, the trade of India has been increasing by leaps and bounds, largely in consequence of the country being opened out by railways, and there is every reason to hope that somewhat similar results would occur in Africa under similar circumstances, though the lower civilization of the people would prevent the harvest being so quickly reaped. But, however it may be as to the future, the present poverty of Africa is enough to demonstrate the necessity of pushing ahead cautiously and steadily, and of doing so in the most economical manner possible.

M. Decle, in an interesting paper, read before the International Geographical Congress in London last year, strongly advocated the construction of cheap roads for use by the natives, taking precautions to prevent any traffic in slaves along them. His suggestions are well worthy of consideration; but the cost of transport along any road would, I should have thought, soon have eaten up any profits on the import or export trade to or from Africa. What must be done in the first instance is to utilise to the utmost all the natural lines of communication which require little or no expenditure to render them serviceable; in fact, to turn our attention at first to the rivers and to the lakes. I have already pointed out that the early maps of Africa prove that the rivers have almost invariably been the first means of communication with the interior, and until this continent is rich enough to support an extensive railway system, we must rely largely on the waterways as means of transport.

It may be as well here to remark that geographical knowledge is often required in order to control the imagination. I do not know why it is, but almost everyone will admit that if he sees a lake of considerable size depicted on a map, he immediately feels a desire to visit or possess that locality in preference to others. A lake may be of far less commercial value than an equal length of thoroughly navigable river, and yet it will always appear more attractive. Look at the way in which the English, the French, and the Germans are all pressing forward to Lake Chad; and yet Lake Chad is in reality not much more than a huge swamp, and, in all probability, it is excessively unhealthy. Again, it is probable that the Albert Nyanza will prove to be of comparatively small value, because the mountains come down so close to its shores. Of course, the great lakes form an immensely important feature in African geography, but we must judge their commercial value rationally, and without the bias of imagination.

To develop the traffic along the rivers and on the lakes is the first stage in the commercial evolution of a continent like Africa. But it cannot carry us very far. Africa is badly supplied with navigable rivers, chiefly as a natural result of the general formation of the land. The continent consists, broadly speaking,

of a huge plateau, and the rivers flowing off this plateau are obstructed by cataracts in exactly the places where we most want to use them—that is, when approaching the coasts. The second stage in the commercial evolution will therefore be the construction of railways with the view of supplementing this river traffic. Finally, no doubt, a further stage will be reached, when railways will cut out the rivers altogether; for few of the navigable rivers are really well suited to serve as lines of communication. This last stage is, however, so far off that we may neglect it for the present; though it must be noted that there are some parts of Africa where there are no navigable rivers, and where, if anything is to be done, it must be entirely by means of railways.

Thus, as far as the immediate future is concerned, the points to which our attention should be mainly directed are (1) the courses of the navigable parts of the rivers, and (2) the routes most suitable for the construction of railways in order to connect the navigable rivers and lakes with the coast. As to the navigable rivers, little more remains to be discovered with regard to them, and we can indicate the state of our geographical knowledge on this point with sufficient accuracy for our purposes by means of a map. Of course the commercial value of a waterway depends greatly on the kind of boats which can be used, and that point cannot well be indicated cartographically.

As to the railways, we must study the physical features of the country through which the proposed lines of communication would pass. All the obstacles on rival routes should be most carefully surveyed when considering the construction of railways in an economical manner. Great mountain chains are seldom met with in Africa, and from that point of view the continent is as a whole remarkably free from difficulties. But drifting sand is often a serious trouble, and that is met with commonly enough in many parts. Wide tracks of rocky country also form serious impediments, both because of the cost of construction, and also because the supply of water for the engines becomes a problem not to be neglected. Such arid and sandy districts are of course thinly inhabited, and we may therefore generally conclude that where the population is scanty, there railway engineers will have special difficulties to face. On the other hand, dense forests are also very unsuitable. We have not much experience to guide us, but it would appear probable that the initial expense of clearing the forest, and the cost of maintenance, in perpetually battling against the tropical vegetable growth. will be very heavy; for it will not do to allow the line to be in constant danger of being blocked. The dampness of the forest, which will cause all woodwork and wooden sleepers to rot, will be no small source of trouble, and the virulent malarial fevers, always met with where the vegetation is very rank, will add immensely to the difficulty both of construction and of maintenance. The health of the European *employés* will be a most serious question in considering the construction of railways in all parts of tropical Africa, for the turning up of the soil is the most certain of all methods of causing an outbreak of malarial fever; and the evil results would be most severely felt in constructing ordinary railways in dense forests. In making the short Senegal railway, where the climate is healthier than in many of the districts further south, the mortality was very great. Perhaps we shall have to modify our usual methods of construction so as to mitigate this danger, and, in connection with this subject, I may perhaps mention that the Lartigue system seems to be specially worthy of consideration—a system by which the train is carried on a single elevated rail. This is perhaps travelling rather wide of the mark of ordinary geographical studies, but it illustrates the necessity of a thorough examination of the environment before we try to transplant our own methods to other climes.

We may, however, safely conclude that we must as far as possible avoid both dense forests and sandy and rocky wastes in the construction of our first railways.

Then, as to the lines of communication, considered as a whole, rail and river combined, we must obviously, if any capital is to be expended, make them in the directions most likely to secure a profitable traffic. In considering this part of the question, it will be seen that there are several different problems to be discussed:

(1) trade with the existing population in their present condition ; (2) trade with the native inhabitants when their countries have been further developed with the aid of European supervision; and (3) trade with actual colonies of European settlers. To many minds the last of these problems will appear to be the most important, and in the end it may prove to be so. But the time at my disposal compels me to limit myself to the consideration of trade with the existing native races within the tropics, with only an occasional reference to the influence of white residents. We must, no doubt, carefully consider which are the localities most likely to attract those Europeans who go to Africa with the view of establishing commercial intercourse and commercial methods in the interior ; and there can be no doubt that considerations of health will play a prominent part in deciding this point. Moreover, as the lowest types of natives have few wants, the more primitive the inhabitants of the districts opened up, the less will be the probability of a profitable trade being established. For both these reasons the coast districts are not likely in the end to be as good a field for commercial enterprise as the higher lands in the interior; for the more we recede from the coast, the less unhealthy the country becomes, and the more often do we find traces of native civilisation. To put it simply, we must consider both the density of the population and the class of inhabitant in the districts proposed to be opened up. Of course, the exact nature of the products likely to be exported, and the probability of demands for European goods arising amongst the natives of different districts, are vitally important considerations in estimating the profits of any proposed line of railway ; but to discuss such problems in commercial geography at length would open up too wide a field on an occasion like this.

If the importance of considering the density of the population in the different districts in such a preliminary survey is admitted, we may then simplify our inquiry by declining to discuss any lines of communication intended to open up regions where the population falls below some fixed minimum—whatever we may like to decide on. Of course, the question of the greater or less probability of a locality attracting white temporary residents is very important, but unless there is a native population ready to work on, there will be little done for many years to come. Politically it may or may not be right to open up new districts by railways for the sake of finding outlets for our home or our Indian population ; but here I am considering the best lines for the development of commerce, taking things as they are. What then shall be this minimum of population ? The population of Bengal is 470 per square mile ; of India, as a whole, about 180; and of the United States, about 21 or 22. If it is remembered that the inhabitants of the United States are, per head, vastly more trade-producing than the natives of Africa, it will be admitted that we may for the present exclude from our survey all districts in which the population does not reach a minimum of 8 per square mile ; it might be right to put the minimum much higher than this. On the map now before you, the uncoloured parts show where the density of population does not come up to this minimum, and we can see at a glance how enormously this reduces the area to be considered. The light pink indicates a population of from 8 to 32 per square mile, and the darker pink a denser population than that Of course, such a map, in the very imperfect state of our knowledge, must be very inaccurate, as I am sure the compiler would be the first to admit. On the same map are marked the navigable parts of rivers. I should like to have shown the dense forests also, but the difficulty of giving them with any approach to correctness is at present insuperable.

Here, then, is the kind of map we want in order to consider the broad outline of the questions connected with the advisability of attempting to push lines of communication into the interior. The problem is how to connect the inland parts of Africa, which are coloured pink on this map, with the coast, by practicable lines of communications, at the least cost, with the least amount of dense forest to be traversed, and, in the case of railways, whilst avoiding as far as possible all thinly populated districts.

It is of course quite impossible here to discuss all the great routes into the interior, and I should like to devote the remaining time at my disposal to the

consideration of this problem as far as a few of the most important districts are concerned, confining myself, as I have said, to trade with existing native races within the tropics. Taking the East Coast first, and beginning at the north, the first region sufficiently populous to attract our attention is the Valley of the Nile, and parts of the Central Sudan. Wadai, Darfur, and Kordofan are but scantily inhabited, according to our map, and this is probably the case now that the Khalifa has so devastated these districts; but, without doubt, much of this country could support a teeming population, and is capable of great commercial development. The Bahr-el-Ghazal districts are especially attractive, being fertile and better watered than the somewhat arid regions further north. These remarks remind me how difficult it is at this moment to touch on this subject without trenching on politics. Few will deny that the sooner this region is connected with the civilised world the better, and it is only as to the method of opening it up, and as to who is to undertake the work, that burning political questions will arise. The geographical problems connected with the lines of communication to the interior can be considered whilst leaving these two points quite on one side.

A glance at the map reminds us of the well-known fact that, below Berber, the Nile is interrupted by cataracts for several hundred miles, whilst above that town there is a navigable water-way at high Nile until the Fola rapids are reached, a distance of about 1,400 miles, not to mention the 400 to 600 miles of the Blue Nile and the Bahr-el-Gazal, which are also navigable. The importance of a railway from Suakin to Berber is thus at once evident, and there is perhaps only one other place in Africa where an equal expenditure would open up such a large tract of country to European trade. This route, however, is not free from difficulties. Suakin is hot and unhealthy. Then the railway, about 260 miles in length, passes over uninhabited or thinly inhabited districts the whole way. Though the hills over which it would pass are of no great height, the highest part of the track being under 3,000 feet above the sea, it is often said that the desert to be traversed would add greatly to the difficulty of construction. According to Lieut.-Colonel Watson, R.E., however, these difficulties have been greatly exaggerated, for the water supply would give no great trouble. The sixth cataract, between Metemma and Khartum, would make navigation for commercial purposes impossible when the waters are low; it is probable that this impediment could be overcome by erecting locks, but it is impossible to estimate the cost of such works. Then, again, the Nile above Khartum is much obstructed by floating grass or sudd, making navigation at times almost impossible; but it was Gordon's opinion that a line of steamers on the river, even if running at rare intervals, would keep the course of the stream clear; this, however, remains to be proved.

If the canalisation of the sixth cataract should prove to be too costly an undertaking, then it would be most advisable to carry on the railway beyond that obstacle. This might be done by prolonging the line along the banks of the Nile, or by adopting an entirely different route from Suakin through Kassala. I hope we shall hear something from Sir Charles Wilson as to the relative merits of these proposals during the course of our proceedings. Proposals have also been made for connecting the Nile with other ports on the Red Sea, and all of these suggestions should be carefully examined before a decision is made as to the exact route to be adopted. But in any case, considering the matter merely from a geographical standpoint, and putting politics on one side—a very large omission in the case of the Sudan—it would appear that one or other of these routes should be one of the very first to be constructed in all Africa.

Passing further south, it is obvious from the configuration of the shore, and from the distribution of the population, that the lines of communication next to be considered are those leading to the Victoria Nyanza, and on to the regions lying north and west of the lake.

Two routes for railways from the coast to the Victoria Nyanza have been proposed, one running through the British and the other through the German sphere of influence. Looking at the matter from a strictly geographical point of view, there is perhaps hardly sufficient information to enable us to judge of the relative merits of the two proposals. Both run through an unhealthy coast zone, and

both traverse thinly inhabited districts until the lake is reached. The German route, as originally proposed, would be the shorter of the two; but there is some reason to think that the British line will open up more country east of the lake, which will be suitable for prolonged residence by white men. Sir John Kirk, in discussing the question of the possible colonisation of tropical Africa by Europeans, said: 'These uplands vary from 5,000 to 7,000 feet in height, the climate is cool, and, as far as known, very healthy for Europeans. This district is separated from the coast by the usual unhealthy zone, which, however, is narrower than elsewhere on the African littoral. Between the coast zone and the highlands stretches a barren belt of country, which attains a maximum width of nearly 200 miles. The rise is gradual, and throughout the whole area to be crossed the climate is drier and the malarial diseases are certainly much less frequent and less severe than in the regions further south.' These very advantages, however, may have to be paid for by the greater difficulty of railway construction. Putting aside future prospects, the map shows that the populous region to the west of the lake makes either of these proposed lines well worthy of consideration, though it would perhaps be rash to predict how soon the commerce along them would pay for the interest on the capital expended. What will be the fate of the German project I do not know, but we may prophecy with some confidence that the British line, the construction of which has been commenced, will be completed sooner or later.

The two lines of communication we have discussed—the Suakin and the Victoria Nyanza routes—are intended to supply the wants of widely separated districts; but, looking to a more distant future, they must sooner or later come into competition one with the other, in attracting trade from the Central Sudan. Before this can occur, communication by steamboat and by railway must be opened up between the coast and the navigable Nile by both routes. This will necessitate a railway being constructed, not only to the Victoria Nyanza, but also from that lake, or round it, to the Albert Nyanza; and, as the Nile is rendered unnavigable by cataracts about Dufile, and as the navigation is difficult between Dufile and Lado, here also a railway would be necessary in order to complete the chain of steam communication with the coast. If goods were brought across the Victoria Nyanza by steamer, and taken down the Nile in the same manner from the Albert Nyanza to Dufile, this route would necessitate bulk being broken six times before the merchandise was under way on the Nile; by the Suakin route, on the other hand, bulk would only have to be broken twice, provided the sixth cataract were rendered navigable. Thus, if this latter difficulty can be overcome, and if the sudd on the Nile is not found to impede navigation very much, this Nyanza route will certainly not compete with the Suakin route for any trade on the banks of the navigable Nile until a railway is made from the coast to Lado, a distance of over 800 miles as the crow flies, and certainly over 1,000 miles by rail. It must be remembered also that the Nyanza route passes over mountains 8,700 feet above the sea; that the train will have to mount, in all, nearly 13,000 feet in the course of its journey from the coast; and that a difficult gorge has to be crossed to the eastward of the Victoria Nyanza. From these facts we may conclude that it will be a very long time before the Nyanza route will draw any trade from the Central Sudan.

The line through the British sphere of influence runs to the northern end of Victoria Nyanza, but from Mr. Vandaleur's recent expedition into these regions we learn that a shorter route, striking the eastern shore of the lake, is under consideration. To lessen the expense of construction would be a great boon, but if we look to the more ambitious schemes for the future, something may be said in favour of the original proposal as being better adapted to form part of a line of railway reaching the navigable Nile.

With regard to the comparison between the German and British routes to the Victoria Nyanza, the latest accounts seem to imply that the Germans have practically decided on a line from the coast to Ujiji, with a branch from Tabora to the Victoria Nyanza. This would be a most valuable line of communication; but it seems a pity that capital should be expended in competitive routes when there are so many other directions in which it is desirable to open up the continent. If the Germans wish to launch out on great railway projects in Africa, let them make a

line from the south end of Lake Tanganyika to the northern end of Lake Nyasa, and thence on to the coast; they would thus open up a vast extent of territory, and Baron von Schele tells us that a particularly easy route can be found from Kilva to the lake. Such a line of communication, especially if eventually connected with the Victoria Nyanza to the north, would be more valuable than any other line in Africa in putting an end to the slave trade, as it would make it possible to erect a great barrier, as it were, running north and south across the roads traversed by the slave traders.

A line through German territory connecting Lake Nyasa with the sea would, no doubt, come into competition with the route connecting the southern end of that lake with the Zambesi, and thus with the coast. The mouths of the Zambesi, though they are passable, will always present some impediment to commerce. But after entering the river navigation is not obstructed until the Murchison Rapids on the Shirè River are reached. Here there are at present sixty miles of portage to be traversed, and this transit must be facilitated by the construction of a railway, if this route is to be properly developed; Mr. Scott Elliot tells us that 120 miles of railway, from Chiromo to Matope, would be necessary for this purpose. Beyond this latter point there is a good waterway to Lake Nyasa. Thus a comparatively short line of railway would open up this lake to European commerce, and this route is likely to be developed at a much earlier stage of the commercial evolution of Africa than the one through German territory above suggested. It will be seen that these routes connect fairly populous districts with the coast, and it must also be recollected that the high plateau between Lake Nyasa and the Kafue River is one of the very few regions in tropical Africa likely to attract white men as more or less permanent residents.

Further south we come to the Zambesi River, which should, of course, be utilised as far as possible. But this line of communication to the interior has many faults. The difficulties to be met with at the mouths of the Zambesi have already been alluded to. Then the whole valley is unhealthy, and white travellers would prefer any route which would bring them on to high land more quickly. Moreover the Kebrabasa rapids cause a serious break in the waterway, and, as the river above that point is only navigable for canoes, it is doubtful if it would ever be worth making a railway for the sole purpose of connecting these two portions of the river.

As the population of the upper Zambesi valley is considerable, and as the country further from its banks is said to be likely to be attractive to white men, there can be no doubt of the advisability of connecting it with the coast This naturally leads us to consider the Beira route, as a possible competitor with the Zambesi. A sixty centimetre railway is now open from Fontesvilla to Chimoio (190 kilometres), and it is probable that during the course of the next two years the construction of the railway will be completed from the port of Beira itself as far as the territory of the Chartered Company. This will form the first step in the construction of a much better line of communication to the Upper Zambesi regions than that afforded by the river itself. It is true that the gauge is very narrow, and that the first part of the line passes through very unhealthy districts; but this line will nevertheless be a most valuable addition to the existing means of penetrating into the interior of the continent. It is needless to say that the object of this railway is to open up communications with Mashonaland, not for the purposes now suggested.

South of the Zambesi the map shows us that there are no regions in tropical Africa where the density of the native population reaches the minimum of eight per square mile. Here, however, we come to the gold fields, where there is attractive force enough to draw white men in great numbers within the tropics, and where, no doubt, some of the most important problems connected with railway communications will have to be solved in the immediate future. But, for reasons of time and space, I have limited myself to the discussion of districts within the tropics where trade with the existing native races is the object in view. The Beira railway does not in reality come within the limits I have imposed on myself,

except as to its future development. Had time permitted, I should like to have discussed the route leading directly from the Cape to Mashonaland, its relative merits in comparison with the Beira railway, and as to where the two will come into competition one with the other. But I must pass on at once to consider the main trunk routes from the West Coast leading into the interior of Africa.

Passing over those regions on the West Coast where railways would only be commenced because of the probable settlement, temporary or permanent, of white men—passing over, that is, the whole of the German sphere of influence—we first come to more dense native populations near the coast towns of Benguela and St. Paul de Loanda. The latter locality is the more hopeful of the two, according to our map, and here we find that the Portuguese have already constructed a railway leading inland for 191 miles to close to Ambaca. The intention of connecting this railway with Delagoa Bay was originally announced, and I am not aware to what extent this vast project has now been cut down, so as to bring it within the region of practical proposals. A further length of 35 miles is, at all events, being constructed, and 87 more miles have been surveyed. The Portuguese appear to be very active at present in this district, as there are several other rail-ways already under consideration; one from Benguela to Bihe, of which 16 miles is in operation, another from Mossamedes to the Huilla Plateau, and a third from the Congo to the Zambesi. It is difficult to foretell what will be the outcome of these schemes, but our population map is not very encouraging.

Next we come to the Congo, and here there is a grand opportunity of opening up the interior of the continent. In going up this great stream from the coast we first traverse about 150 miles of navigable waterway, and afterwards we come to some 200 miles of cataracts, through which steamers cannot pass. Round this impediment a railway is now being pushed, 189 kilometres of rails (117 miles) being already laid. Then we enter Stanley Pool, and from this point we have open before us—if Belgian estimates are to be accepted—7,000 miles of navigable waterway. If this fact is correct, and if the population is accurately marked on our map, then there is no place in all Africa where 200 miles of railway may be expected to produce such marked results. The districts traversed are unhealthy, and the natives are, generally speaking, of a low type; but in spite of these drawbacks, which no doubt will delay progress considerably, we may confidently predict a grand future for this great natural route into the interior.

To the north of the Congo, the next great navigable waterway met with is the Niger. Again, granting the correctness of the population map, it can be seen at a glance that there is no area of equal size in all Africa so densely inhabited, and no district where trade with the existing native population appears to offer greater inducement to open up a commercial route into the interior. Luckily little has to be done in this respect, for the Niger is navigable for light-draught steamers in the full season as far as Rabba, about 550 miles from the sea; here the navigation soon becomes obstructed by rocks, and at Wuru, about 70 miles further up the river, the rapids are so unnavigable that even the light native canoes have to be emptied before attempting a passage, and there are frequent upsets. From Wuru the rapids extend to Wara, after which a stretch of clear and slow-running river is met with. Above this, again, the Altona Rapids extend for a distance of 15 miles; then 15 miles of navigable waterway, and then 20 miles more of rapids are encountered. Yelo, the capital of Yauri, is situated on these latter cataracts, above which the Middle Niger is navigable for a considerable length. The Binue is also navigable in the floods for many miles, the limits being at present unknown; part of the year, however, it is quite impassable except for canoes. The trade with the Western Sudan, which has been made possible by the opening up of this river, is still only in its infancy, and to get the full benefit of this waterway a line of railway ought to be carried on from Lokoja to Kano, the great commercial centre of Hausal and Mr. Robinson's recent journeys over this country, which we hope to hear about at a later period of our proceedings, have served to confirm the impression that no great physical difficulties would be encountered. The political condition of the country may, however, make the construction of this railway quite impossible for the present, for here we are on the borderland between Mahom-

medanism and Paganism, where the slave trade always puts great impediments in the path of progress, but where the same circumstances make it so eminently desirable to introduce a higher condition of civilisation. The only drawback to the Niger as a line of communication to the Western Sudan is the terribly unhealthy nature of the coast districts which have to be traversed. Any man, who finds a means of combating the deadly diseases here met with, will be the greatest benefactor that Africa has ever had; but of such a discovery there are but few signs at present.

It is perhaps too soon to speculate as to the best means of opening a trade route to Wadai and the more central parts of the Western Sudan; for we may be sure that little will be done in this direction for years to come. Several competing routes are possible. From the British sphere, we may try to extend our communications eastward from the navigable parts of the Binue. The French, on the other hand, may push northwards from the Ubangi; whilst, in a later stage of commercial evolution, the best route of all may be found through German territory, by pushing a railway from the shore in a direct line towards Bagirmi and Wadai. To compare the relative merits of these trunk lines is perhaps looking too far into the future, and traversing too much unknown country, to make the discussion at all profitable.

Proceeding northwards, or rather westwards, along the coast we find ourselves skirting the belt of dense forest already described as being the great obstacle to advance in this part of Africa. It is to be hoped that this barrier will be pierced in several places before long. Naturally we turn our attention to the different spheres of British influence, and here we are glad to learn that there are several railways being constructed or being considered, with a view to opening up the interior.

At Lagos a careful survey of a railway running in the direction of Rabba has been made, and the first section is to be commenced at once. To connect the Niger with the coast in this way would require 240 miles of railway, but the immediate objectives are the towns of Abeokuta and Ibadan, which are said to contain more than a third of a million inhabitants between them. No doubt the populous coast region makes such a line most desirable; but whether it would be wise to push on at all quickly to the Niger, and thus to come into competition with the steamboat traffic on that river, is a very different question

Surveys have also been made for a railway to connect either Kormantain or Apan on the Gold Coast with Insuaim, a town situated on a branch of the Prah. It is believed that the local traffic will be sufficiently remunerative to justify the construction of this line. But, looking to the further prolongation of this railway into the interior, it appears possible that those who selected this route were too much influenced by the desire to reach Kumasi, which is a political rather than a commercial centre. According to the views I have been advocating to-day, the main object of a railway in this quarter should be the crossing of the forest belt, and if, as there is some reason to believe, that belt is exceptionally wide and dense in this locality, the choice of Kumasi as a main point on the route will have been an unfortunate selection. A little further south, nearer the banks of the Volta, it is probable that more open land would be met with, and moreover that river itself, which is navigable for steam launches from Ada to Akusi, would be of use as a preliminary means of transport. It is to be hoped that the merits of a line from Accra through Odumase will be considered before it is too late.

I am now approaching the end of my brief survey of tropical Africa, for the best method of opening communication between the Upper Niger and the coast is the last subject I shall touch on. With this object in view, the French have constructed a railway from Kayes, the head of steam navigation during high water, on the Senegal to Bafulabé, with the intention of ultimately continuing the line to Bamaku on the Niger Unexpected difficulties have been met with in the construction of this railway, and, as the Senegal River between Kayes and St Louis is only navigable for about a quarter of the year, it would hardly appear as if the selection of this route had been based on sound geographical information. No doubt the French will find some other practicable way of connecting the Upper

# British Association for the Advancement of Science.

## LIVERPOOL, 1896.

# ADDRESS

TO THE

# ECONOMIC SCIENCE AND STATISTICS SECTION

BY

## The Right Hon LEONARD COURTNEY, M.A., M.P.,

PRESIDENT OF THE SECTION.

WHEN the British Association revisits a town or city, it is the laudable custom of the President of a Section to refer to what was said by his predecessor in the same chair on the former occasion. I should in any case be disposed to follow this practice, but I could not choose to do otherwise when I find it was my honoured friend Professor Jevons who occupied this place in Liverpool in 1870. He was one of a group which passed away in quick succession, to the great loss of the study of Economics in this country, since each had much promise of further usefulness, and left us with labours unfulfilled. Bagehot, Cairnes, Cliffe Leslie, Fawcett, Jevons, occupied a large space in the field of economic study, and no one among them excelled Professor Jevons in the vigour and clearness of his analysis or in the sincerity and range of his speculations. His first work which arrested public attention was perhaps not so much understood as misunderstood. This busy, bustling, hurrying world cannot afford time to pause and examine the consecutive stages of a drawn-out argument, and too many caught up and repeated to one another the notion that Jevons predicted a speedy exhaustion of our coalfields, and they and their successors have since been congratulating themselves on their cleverness in disbelieving the prophecy No such prophecy was in truth ever uttered. The grave warning that was given was of the impossibility of continuing the rate of development of coal production to which we had been accustomed, of slackening, and even arrested growth, and of the increasing difficulty of maintaining a prosperity based on the relative advantages we possessed in the low cost of production of coal ; and this warning has been amply verified in the years that have since passed, as will be at once admitted by all who are competent to read and understand the significance of our subsequent experience. But I must not dwell on this branch of Jevons's work nor on the many other contributions he made to the study of our economic life I am concerned with what he said here twenty-six years since.

At first sight the address of my predecessor may seem loose and discursive ; but viewed in due perspective, it appears a serious inquiry into the apparent failure of economic teaching to change the course and elevate the standard of our social life, and an earnest endeavour to impress these principles more strongly on the public mind so that the future might better the history he reviewed He referred to the repeal of the Corn Laws, and owned with regret that the condition of the people was little changed, that pauperism had scarcely abated, that little forethought was shown by the industrial classes in preparing for the chances of the future ; and he dwelt on the mischievous influence of the unthinking benevolence of the wealthy in undermining providence by its constant and increasing activity in mitigating the

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evils of improvidₙnce. Jevons was not content to condemn the doles of past testators ; he wanted the reorganisation of the Hospital service of our towns, so that as far, at least, as the ordinary and inevitable casualties of sickness and accident are concerned, they might be met by the co-operation of workers inspired by motives of self-reliance instead of by ever open gratuitous service making forethought unnecessary and even foolish. In this connection it may be noticed that while giving a hearty welcome to Mr. Forster's Education Act, passed in the same year that he spoke, he noted with satisfaction that primary education had not been made gratuitous so as to take away another support of prudence. It is strange, too, in the light of our recent experience, to find him regretting that the task of remodelling local taxation had not been undertaken, so that local wants might be met by a just apportionment of their charge and the principles of association of the members of local communities placed on a firmer basis.

It will be seen that what really occupied the mind of my predecessor was the apparent slow success of Economic thinkers in influencing political action, and we, looking back over the intervening twenty-six years, have certainly no more cause of congratulation than he felt ; we are forced to ask ourselves the same question what is the reason of our apparent failure ; we are driven to examine anew whether our principles are faulty and incomplete or whether the difficulties in their acceptance, they being sound, lie in the prejudices of popular feeling which politicians are more ready to gratify than to correct.

I do not pause to meet the charges of inhumanity or immorality which have in other times been brought against Economists. Jevons pleaded for the benevolence of Malthus, who might indeed be presumed, as an English clergyman, to be not altogether inhuman or immoral. In truth everyone who has ever had any thought about social or fiscal legislation—and we have had such laws among ourselves for five centuries—everyone who has ever tried to influence the currents of foreign trade—and such attempts date from an equally remote past—has been moved by some train of economic reasoning, and must strictly be classified as an Economist , and the only difference between such men and those who are more usually recognised by the name is that the latter have attempted to carry their thoughts a little further, and have been more busy to examine the links of their own reasoning and the soundness of their conclusions. The men who attempted to fix wages, to limit the numbers in special trades, to prohibit or to compel certain specific exports, all had some notion that they were engaged in doing something to strengthen if not to improve the better organisation of communities. Even the aims which appear to us most selfish were disguised as embodying social necessities. But by the beginning of the present reign it may be said that the study of Political Economy in this country had worked itself free from earlier errors, and it had come to be believed that the secret of social regeneration lay in the utmost allowance of freedom of action to every individual of the community, so far at least as that action affected himself, coupled with the most complete development of the principle of self-reliance, so as to bring home to every member, freed from legal restraint on his liberty of action, the moral responsibility of self-support and of discharging the duties, present and to come, of his special position. With this education of the individual in self-reliance, and with this liberation of the same individual in the conduct of life, it was held that by certain, if slow, stages the condition of the community would be improved, and a wholesome reorganisation naturally effected.

Whatever view we may now hold of this belief, whether we must discard it as incomplete or even erroneous, or whether we remain strong in the conviction of its intrinsic soundness and in the possibility of realising the hopes it offered, it must still be evident that those who professed it were imbued with the deepest interest in the well-being of their fellow creatures, and that the aim of all their speculations was the purification of social life, and its healthy and abundant development

Such was the theory more or less openly expressed by Economic thinkers when the British Association was founded, and the same theory, as I conceive, lay at the base of Jevons's address in 1870 Can we hold it now, or must it be recast ?

Since 1870 Primary Education has practically been made gratuitous. The

Legislature had an opportunity for abolishing the mischief of do- es, but showed no inclination to make use of it, and there were even traces of a feeling of favour for the maintenance of these bequests of the past    The indiscriminate multiplication of so-called charitable institutions has in no way been reformed, and there is as great activity as ever in the zeal of those who would mitigate or relieve the effects of improvidence without touching improvidence itself.    As far as the course of legislation is concerned, it may be feared that it has been directed to diminish rather than to increase the spirit of self-reliance.    Codes of regulations have been framed for the supervision of the conduct of special industries, and their sphere has been extended so as to embrace at no distant period, if not now, the whole industrial community.    The reformed Poor Law, which was regarded as a great step in the education of the workman, especially of the agricultural labourer, in independence, stands again upon its trial, and proposals are at least in the air for assuring to the aged poor a minimum measure of support without any regard to the circumstances of their past lives, or to the inevitableness of their condition. The suggestions made by responsible statesmen have indeed been more limited and cautious, but it will be acknowledged of those, as of the German system, from which they may be said to be in some measure borrowed, that they involve a great departure from that ideal of individual development to which I have referred.    Add to this that there is a movement, which has become practical in many large cities and towns, for the community itself to engross some forms of industrial activity, and to undertake in respect of them to meet the wants of their inhabitants.    All these developments and more may be summed up as illustrations of Collectivity—an ideal which has its advocates and professors, and which looks in the future for regulated civic and national monopolies instead of unrestricted freedom of individual activity, and for the supervision and control of those industries which may remain unabsorbed by state or town.    In pursuit of this last conception there have been put forward not only requirements as to hours and conditions of labour, but a demand also for a Living Wage or a minimum, below which no workman shall be paid , and this principle has been already adopted by some municipalities in respect of their monopolised industries    The State itself indeed has, through the popular branch of the legislature, declared more or less clearly in favour of the same principle in respect of the industries which are conducted in its service

We have not only to acknowledge the continued slowness of politicians to adopt and enforce the teaching of Economists such as Jevons contemplated, but also the rise of another school of Economic thought which competes for, and in some measure successfully obtains, the attention of the makers of laws.    The question which has already been suggested thus becomes inevitable.    We must inquire whether the failure of former teaching has not been due to errors in itself rather than to the indocility of those who have neglected it.

The greatest difficulty which the teachers of the past have to overcome when put upon their self-defence lies in the suspicion, or more than suspicion, of an occupied multitude that their promises have failed.    It is thought of them, if it is not openly said, that they had the ear of legislators for a generation, that the course and conduct of successive administrations were governed by their principles, and yet society, as we know it, presents much the same features, and the lifting up of the poor out of the mire is as much as ever a promise of the future    Some quicker method of introducing a new order is called for, and any scheme offering an assurance of it is welcomed.    A ready answer can be given to much of the suspicion of failure that is entertained.    That freedom of industrial action, which is the first postulate of the Economists, has never been secured.    We are so much accustomed to the conditions of our own life that this declaration may seem strange to many, who will say that at least in England labour and trade are free ; but it must be admitted, on reflection, that in one great sphere of action the liberty so postulated has, for good or bad reasons, never been conceded    The limitations and restrictions necessarily consequent upon the system of land laws established among us are not commonly understood, but although much has been done to liberate agriculture from their fetters, its perfect freedom has not been attained.    There

may be free trade in the United Kingdom and free land in the United States, but the country is yet to be found in which both are realised, and even if both these requisites were attained the sores of social life would not be removed unless the spirit of self-reliance were fully developed: and how little has been done to secure this essential condition of progress! nay, how much has been done by law, and still more by usage, to weaken and destroy its power! The Economist of whom I have been speaking may boldly claim that so far as he has had a free hand, his promises have been realised, there has been a larger population with increased means of subsistence and diminished necessity of toil, a people better housed, better fed, better clothed, with fewer relative failures of self-support; and if the teaching which has been partially adopted has brought about so much, everything it promised would have been secured had it been fully followed. If the teaching had been fully followed? This raises the question whether there are inherent difficulties in the nature of man preventing such a consummation, and many will be ready with the answer that such difficulties exist, are permanent and cannot be surmounted. As long as human nature is what it is —so runs the current phrase—men will not see misery without relieving it, they will not wait to inquire into its cause and whether it could have been prevented, and it is claimed that this instinct is one of the best attributes of humanity, which we should not attempt to eradicate. This kind of reply easily catches the popular ear. It seems generous, sympathetic, humane. But it is based on a view of human nature being incapable of education which has been and will long be the excuse for acquiescence in all imperfections and even iniquity, nor can that be said to be truly generous, sympathetic, or humane which refuses to inquire into the possibility of curing disease, and prefers the selfishness of self-relief to the patient endeavour to probe and remove the causes of the sufferings of others. The Economist of the past generation would, I think, be justified in repudiating with warmth the feeble temper which recoils from the strenuousness of endeavouring to deal with social evils at their origin, and in reprobating the acceptance as inevitable of vices we take no pains to prevent. This, however, does not conclude the whole matter    Even if we did attain the ideal of bringing home to all the members of the community the fatal consequences of improvidence and vice, should we find improvidence and vice ever narrowing into smaller and smaller circles, or should we be confronted with their existence as before, with this difference, that past attempts to alleviate their miserable consequences would be discredited and abandoned? I fear I must here confess to a somewhat faltering faith. That a vigorous enforcement of the penalties of improvidence would diminish it, is a conclusion justified by experience as well as suggested by theory; but that it and its consequences would not still remain gross and palpable facts is a conclusion I have not the courage to gainsay. At all events, I cannot refuse to consider the question whether something more than the complete freedom of the individual is not necessary for the reformation of society, and to examine with an open mind any supplementary or alternative proposals that may be made to reach this end    Yet one thing must be said, and said with emphasis, of the theory of the Economist. It was a working theory. No theory can be accepted even for examination which does not show a working organisation of society, and the theory we have had under review has this necessary characteristic, even if it does not open up a certain way to a perfect reconstruction of our social system.

   It will be conceded by the most fearless and thorough-going advocates of the liberty of individual development, that it must be supported by large measures of co-operative action. No individual can by any amount of forethought protect himself by himself against the chances and accidents of the future. No one can tell beforehand what is in store for himself in respect of sickness, or accident, or those changes of circumstances which may arise from the default of others; and mutual aid is necessary to meet such contingencies. The freedom and activity of association thus indicated are in no way inconsistent with the fullest theory of individual responsibility    Nor is there any departure from it in the voluntary combination among themselves of persons, individually weak, to supervise and safeguard the economic conditions into which they may enter with others relatively stronger. A single workman may be

powerless to induce his employer to modify in any particula/ the terms of his employment, but when workmen band together they may meet employers as equal powers. Such liberty of combination is a development and not a limitation of individual liberty. Another step is taken when the parties to such an arrangement as has been suggested seek to make its provisions compulsory on others, be they workmen or employers, who may enter into similar relations; and the principles of former Economists would generally prompt them to condemn such attempts at compulsion. The Factory Acts were opposed in this way, although they rested upon different grounds, for, though in their consequences they affected the labour of adults, they were propounded for the defence of young persons and children unable to protect themselves or to be the parties to free contracts. Legislation has, however, been extended to control directly the employment of fully responsible persons, and this has been defended by three lines of argument. It is urged that when the unchecked liberty of individuals destroys in fact the liberty of action of larger multitudes, it is in defence of liberty of action that those individuals are controlled. If a sea wall is necessary to prevent a large tract from being periodically inundated, it cannot be permitted to the owner of a small patch along the coast to leave the wall unbuilt along his border, and thus threaten the lands of his neighbours with inundation. Again, it is urged that when the overwhelming majority of persons engaged in a particular industry, employers and employed, are agreed upon the necessity of certain rules to govern the industry, it is not merely a convenience, but is a fulfilment of their liberty, to clothe with the sanction of law the regulations upon which they are agreed. Lastly, it is submitted that there are individuals in whom the sense of responsibility is so weak and whose development of forethought is so hopeless, that it is necessary the law should regulate their conduct as it may regulate the conduct of children  I do not propose to examine in detail these real or apparent limitations of individual liberty  The first plea appears to me to be sound in principle, though it may often have been applied to cases not properly coming within it. As to the second, the convenience of giving to an all but universal custom the force of law is incontestable, but it is at least doubtful whether this is sufficient to deprive individuals who deliberately wish to put themselves outside it of the liberty of doing so. Unless their action could be brought within the first line of argument, sufficient reason for restraint does not appear. As for the hopeless class whose existence is made a plea for restrictive legislation, the Economist may forcibly argue that they have never been left to learn the full force of the lessons of experience, and it is the impatient interference of thoughtless men and thoughtless laws which allows this class to be perpetually recruited

The limitations of individual liberty, to which I have referred, are familiar to us, and have obtained a firm hold in our legislation; but we enter upon comparatively new ground when we turn to the proposals that an increasing number of industries should be undertaken and directed by State or Municipality, and that a minimum and not inadequate subsistence should be assured to all those engaged in such industries, if indeed the principle be not presently extended outside the monopolies so established. The ideas which are clothed in the phrases 'The socialisation of the instruments of industry,' and 'The guarantee of a minimum wage to all workmen,' appear to involve a complete reorganisation of society, and an absolute abandonment of the theories of the past. This is not enough to justify their immediate rejection or their immediate acceptance. The past has not been so good that we can refuse to look at any proposals, however strange in appearance, offering a better promise for the future. It has not been so bad that we must abandon its methods in despair, as if no change could be for the worse, if not for the better. A patient inquirer, feeling his way along the movement of his time, may even be constrained to accept a patchwork covering of life instead of the ideal garment woven without seam throughout; or he may be led to see that the harmony of society, like the harmony of the physical universe, must be the result of divers forces, out of which is developed a perfect curve

No one could now be found to deny the possibility, and few to question the utility, of the socialisation of some services. The post office is in all civilised

countries organised as a national institution, and the complaints that are some-
times heard as to defects in its administration never extend to a demand for its
abolition.   Jevons, in a careful paper, showed that the same financial success
which marks our present postal system, must not be expected from the nationalisa-
tion of the telegraph service, and be dismissed even suggestions for the nationalisa-
tion of railways.   His predictions have been amply verified with respect to the
telegraph account; but telegraphs are a national service amongst ourselves, and
railways are largely nationalised in many continental countries, and in some of our
own colonies and dependencies   Some of our largest municipalities have under-
taken the supply of water and of gas, or even of electric light, to the inhabitants,
and a movement has begun, which seems likely to be extended, of undertaking the
service of tramways.   Demands have also been made for the municipalisation or
nationalisation of the telephone service.

It may be said of all the industries thus described as taken over, or likely to be
taken over, by the nation and local communities, that when they are not so taken
over they require for their exercise special powers and privileges conceded by the
State or community, and the conditions of such concessions are settled by agree-
ment between the community and the body or bodies exercising such industries.
These conditions may involve the payment of a fixed sum, or of a rent for the
concession, or the terms upon which the services are to be rendered may be
prescribed in a stipulated tariff of charges, or the amount of profit to be realised
by the concessionaires may be limited with provisions for reduction of charge
when such limit is reached, or it may be required that in working such industries
certain limits of wages shall be observed as the minima to be paid to the work-
men employed upon them.   Speaking very broadly, it may be said that the
community delegates or leases the right of practising the industry, and there is no
impassable gulf between prescribing the terms on which a lease shall be worked
and assuming the conduct of the industry leased.   There may be difficulties in
the management by a community of a cumbrous and unwieldy undertaking, but
there is no difficulty affecting the organisation of society when the undertaking
must be created and shaped by the community in the first place.   The arguments
against the assumption of such monopolies by State or Local Authorities are those
of expediency, founded on a comparison of gain and loss.   It may be urged that
there are more forcible motives of economy on the part of a concessionaire than on
the part of a community working the undertaking itself; that improvements of
method and reductions of cost will be more carefully sought, and although such
improvements and reductions might in theory be realised by the workmen and
agents of a community, which would thus secure all the savings effected by them,
yet private interest is quicker in discovery and more fertile in suggestion, and
it is more profitable in the end for the community to allow a concessionaire to
secure such profits, subject to a stipulation that some part of them should return to
the community in the way either of increased money payment, or of reduced rates
of charge for the services performed.   It may be urged that when a community
works an industry itself, it may do so at a loss, thus benefiting those who specially
require its services at the cost of the whole body, but this objection is not peculiar
to undertakings so directly worked. It is a matter of common experience for State
or Municipality to grant important subventions to persons willing to undertake
such works on stipulated terms of service, and such subventions involve a levy
from the whole community for the benefit of those availing themselves of the
services.

New considerations of great difficulty arise when we pass to the suggestion of
the undertaking by local authorities of productive industries not in the nature of
monopolies.   In monopolies direct competition, often competition in any shape,
is practically impossible, in other industries competition is a general rule; and it is
by virtue of such competition that the members of the community do in the long
run obtain their wants supplied in the most economical manner.   When com-
modities are easily carried without serious deterioration, the constantly changing
conditions of production and of transport induce a constant variation in the sources
of cheapest supply—that is of supply under conditions of least toil and effort—

and any arrest of this mobility involves a corresponding set-ba 'k in the advancement of the economic condition of mankind. It is a necessary consequence of this process that the local production of special commodities should be subject to diminution and extinction, and that the labours hitherto engaged in such local production should become gradually worthless. Quite as much labour as before might be expended in achieving the result, but it would be misapplied; it ought not to command the same return; it should cease. It is at least difficult to foresee how far the production of commodities exposed to free competition could be maintained by communities themselves in face of the movement we have described. There would be a danger of pressure to do away with invasive competition—action which, in my judgment, would be destructive of the most powerful cause of improvement in the condition of the people There would be an allied danger of a refusal to recognise the possibility of a diminished worth of work which remains as toilsome as ever, and of an increasing congestion of labour when the great movement of the world demands its dispersion It may be that those evils are not inevitable, but they would require to be faced if any serious attempt were made to increase the range of national or municipal industries, and I have not yet seen any attempt at their serious investigation.

The position thus taken may be illustrated by an experience to which I have elsewhere referred, but so pregnant with suggestion that I need not apologise for recalling it My native county, Cornwall, was in my boyhood the scene of widespread activity in copper and tin mining There had not been wanting warnings that the competition of richer deposits in far countries would put an end to these industries in the county, but the warnings had not been realised and remained unheeded. In the years that have since passed they have been gradually and almost completely fulfilled. There are no copper mines now in Cornwall, and the tin mines, which were scattered far and wide throughout the county, are reduced to two or three within one limited area. It is not the case that the ores have been exhausted; they could still be raised, but at a cost of production making the process unprofitable The mines were abandoned one by one, and the population of the county has steadily diminished in every recent census. What would the experience have been had the mines been a county or national property worked by county or nation? I do not stop to comment on the difficulty of expropriating present owners, which, however, must not be forgotten. If the collective owner had leased the mines to companies of adventurers (to use the local phrase), the lessees would have gradually relinquished their concessions, as they have done when taking them from private owners. Nor would the case have been materially different even if the collective owner had introduced the novel stipulation into his leases that the working miners should be paid according to prescribed rates of wages. The process of relinquishment might have been precipitated and accelerated by insisting on such a condition, but otherwise the experience would have been the same The shrinkage of industry would go on without a check, and it is to be hoped that the workmen who found their work failing would, with the fine courage and enterprise they have in fact shown, have betaken themselves to the fields of mining industry displacing their own in all parts of the world. Can one think that the same process would have been maintained had the collective owner worked the mines directly, and the working men looked to county or nation for the continuance of work and wages? The attachment which all men have for the homes of themselves and their fathers would have stimulated a demand for a recurrence to the other resources of the collective owner for the maintenance of an industry that was dying Some demand might even be made for a repression or prohibition of that competition which was the undoing of the local industry. These possibilities may be regarded as fanciful, and it is true that forces might be kept under control that operated within an area and affected a population relatively so limited. But what if the warnings of Jevons respecting coal in England proved like the warnings of the men who foresaw the cessation of tin mining in Cornwall, and the community had to deal with the problem of the dwindling coal industry in face of nationalised coal mines and armies of workmen employed by the nation? The initial difficulties of the nationalisation of that which for centuries has been

the subject of private property are formidable, but they could doubtless be overcome by the short and simple process of confiscation    This transformation is theoretically conceivable.  It is in the subsequent development of the scheme of nationalised and municipalised industries that we are confronted with tasks not so easy of solution. How is its working to be reconciled with that opening up of more and more productive fields which is one of the prime factors of social progress ?    How is the allotment of men to be directed so that they may be shifted about as new centres open and old centres close ?    What checks or commands can be invoked to restrain the growth of population in a district when it should be dwindling ?    These are questions that can scarcely be put aside, and it may even be acknowledged that they gain fresh force when viewed in the light of another experience.    Agricultural industry has recently been subjected to severe trials through a great breadth of this country.    This has been due to cheaper importations from other lands, and though the competition has in my judgment been aggravated by causes into which I will not now digress (which aggravation however might and should be dealt with), the importation of food at less cost is a result no Economist will regard as otherwise than beneficial to the community as a whole.    It is well that bread and flesh and the sustenance of life should be procured with as little toil as possible, however severe the trial for those who have been engaged hitherto in the production of those necessaries.    We know that it has been so severe that demands for relief and assistance have been loudly made, and their power has been such as to have been in some measure successful ; but had land been nationalised and farms held from the State or from county, town, or parish, they would have assumed a different shape, have been urged with greater purpose, and have received larger treatment    The difficulties of such a nationalised industry, passing into what may be described as a water-logged condition, would test beyond the straining point such statesmanship as our experience warrants us to believe possible.

However much we may contemplate the reconstruction of an industrial system, it must, if it is to be a living social organism, be constantly responsive to the ever-changing conditions of growth ; some parts must wax whilst others wane, extending here and contracting there, and manifesting at every moment those phenomena of vigour and decline which characterise life.    In the development of industry new and easier ways are constantly being invented of doing old things ; places are being discovered better suited for old industries than those to which resort had been made ; there is a continuous supersession of the worth of known processes and of the utility of old forms of work involving a supersession, or at least a transfer, of the labour hitherto devoted to them.    All these things compel a perpetual shifting of seats of industry and of the settlements of man, and no organisation can be entertained as practicable which does not lend itself to those necessities.    They are the pre-requisites of a diminution of the toil of humanity    As I have said before, the theory of individual liberty, however guarded, afforded a working plan ; society could and did march under it.    The scheme of collective action gives no such promise of practicability , it seems to lack the provision of the forces which should bring about that movement upon which growth depends    The Economist of the past generation still holds his ground, and our best hope lies in the fuller acceptance of his ideas.    Such, at least, appears to me to be the result of a dispassionate inquiry ; but what may be wanting is something more than a dispassionate temper— a certain fervour of faith    The Economist must feel, if he is to animate multitudes and inspire legislatures, that he, too, has a religion.    Beneath the calmness of his analysis must be felt the throb of humanity.  Slow in any case must be the secular progress of any branch of the human family , but if we take our stand upon facts, if our eyes are open to distinguish illusions from truth, if we are animated by the single purpose of subordinating our investigations and our actions to the lifting up of the standard of living, we may possess our souls in patience, waiting upon the promise of the future.

# British Association for the Advancement of Science.

## LIVERPOOL, 1896.

# ADDRESS

### TO THE

# MECHANICAL SCIENCE SECTION

### BY

Sir DOUGLAS FOX, Vice-President Institution of Civil Engineers,

#### PRESIDENT OF THE SECTION.

It is rather over a quarter of a century since the British Association last held its meeting in the hospitable city of Liverpool. The intervening period has been one of unparalleled progress, both generally and locally, in the many branches of knowledge and of practical application covered by Civil and Mechanical Engineering, and therefore rightly coming within the limits for discussion in the important Section of the Association in which we are specially interested.

During these twenty-five years the railway system of the British Isles, which saw one of its earliest developments in this neighbourhood, has extended from 15,376 miles, at a capital cost of 552,680,000*l.*, to 21,174 miles, at a capital cost of 1,001,000,000*l.* The railway system of the United States has more than trebled in the same period, and now represents a total mileage of 181,082, with a capital cost of $11,565,000,000.

The Forth and Brooklyn, amongst bridges, the Severn and St. Gothard, amongst tunnels, the gigantic works for the water-supply of towns, are some of the larger triumphs of the civil engineer; the substitution of steel for iron for so many purposes, the perfecting of the locomotive, of the marine engine, of hydraulic machinery, of gas and electric plant, those of the mechanical branch of the profession.

The city of Liverpool and its sister town of Birkenhead have witnessed wonderful changes during the period under review. Great and successful efforts have been made to improve the watergate to the noble estuary, which forms the key to the city's greatness and prosperity; constant additions have been made to the docks, which are by far the finest and most extensive in the world. The docks on the two sides of the river have been amalgamated into one great trust. In order properly to serve the vast and growing passenger and goods traffic of the port, the great railway companies have expended vast sums on the connections with the dock lines and on the provision of station accommodation, and there have been introduced, in order to facilitate intercommunication, the Mersey Railway, crossing under the river, and carrying annually nearly 10 millions of passengers, and the Liverpool Overhead Railway, traversing for six miles the whole line of docks, and already showing a traffic of $7\frac{1}{2}$ millions of passengers per annum A very complete waterside station connected with the landing-stage has been lately opened by the Dock Board in connection with the London and North-Western Railway. In addition to this, the water-supply from Rivington and Vyrnwy has now been made one of the finest in the world.

The following comparative figures, kindly supplied by Mr. K. Miles Burton may be of interest :—

|  | 1871 | 1895 |
|---|---|---|
| Population of Liverpool . . | 493,405 . | 641,000 (Estimated) |
| „ Birkenhead . | 65,971 . | 109,000 „ |
| Area of docks, Liverpool, about | 236 acres | 362½ acres |
| „ „ Birkenhead, about | 147 „ | 160 „ |
|  | 383 | 522½ |
| Number of steamers using the port . | 7,448 . | 18,429 |
| Average tonnage of six largest vessels entering the port . . | 2,890 | 6,822 |

The following figures show the importance of the local railway traffic :—

| Number of passenger stations within the boroughs . . . . . | — . . | 58 |
|---|---|---|
| Number of goods stations . . . | — . | 50 |
| Number of passengers crossing the Mersey in the twelve months (Woodside Ferry). . . . . . . | — . . | 7,143,088 |
| Number of passengers crossing the Mersey in the twelve months (Mersey Railway) . . . . . . | — . . | 6,976,299 |

To the hydraulic engineer there are few rivers of more interest, and presenting more complicated problems, than the Mersey and its neighbours, the Dee and the Ribble. They all possess vast areas of sand covered at high water, but laid dry as the tide falls, and in each case the maintenance of equilibrium between the silting and scouring forces is of the greatest importance to the welfare of the trading communities upon their banks. The enclosure of portions of the areas of the respective estuaries for the purposes of the reclamation of land, or for railway or canal embankments, may thus have far-reaching effects, diminishing the volume of the tidal flow and reducing the height of tide in the upper reaches of the rivers. Some idea of the magnitude of these considerations may be derived from the fact that a spring tide in the Mersey brings in through the narrows between Birkenhead and Liverpool 710 millions of cubic yards of water to form a scouring force upon the ebb. The tidal water is heavily laden with silt, which is deposited in the docks, and, at slack water, upon the sandbanks. The former is removed by dredging, and amounts to some 1,100,000 cubic yards per annum ; the latter is gradually fretted down into the channels and carried out to sea before the ebb. Whilst a considerable portion of the narrows is kept scoured, in some places right down to the sandstone rock, there is a tendency, on the Liverpool side, near the landing-stage, to silt up, a difficulty counteracted, to some extent, by the extensive sluicing arrangements introduced by Mr. George Fosbery Lyster, the engineer of the Mersey Docks and Harbour Board.

Very extensive and interesting operations have been carried on by the Board in connection with the bar at the mouth of the river. Dredgers specially designed for the purpose have been employed for some six years, with the result that 15,142,600 tons of sand and other dredged matter have been removed, and the available depth of water at low-water increased from 11 to 24 feet in a channel 1,500 feet in width.

Those who have made the transatlantic passage in former years can more readily appreciate the very great advantage accruing from this great improvement.

Formerly vessels arriving off the port on a low tide had to wait for some hours for the water-level to rise sufficiently to enable them to cross the bar ; the result of a large vessel lying outside, rolling in the trough of the sea with her engines stopped, was that not infrequently this proved to be the worst part of the voyage between New York and Liverpool, and passengers who had escaped the malady of sea-

sickness throughout the voyage were driven to their cabins and berths within three or four hours of landing.

Owing to the very successful dredging operations, ships of largest size can now enter or depart from the Mersey at any state of the tide, and they are also able to run alongside the landing-stage without the intervention of a tender.

Such vessels as the 'Teutonic' or 'Majestic,' of nearly 10,000 registered tonnage, 566 feet in length, 57 feet wide, and 37 feet deep, or the still larger vessels, the 'Campania' or 'Lucania,' of nearly 13,000 tons register, 601 feet in length, 65 feet in width, and 38 feet in depth, can be seen, on mail days, lying alongside.

Whilst the estuary of the Mersey presents a narrow entrance with a wide internal estuary, the Dee, owing to extensive reclamation of land in the upper reaches, has a wide external estuary leading to an embanked river of very limited width, up which the tide rushes with great velocity laden with silt, rising in some two hours, then, during a short time of slack water, depositing the silt, which is not removed by the ebb-tide, spread over some ten hours, and therefore having comparatively little velocity. In this case, also, the outer estuary shows a great tendency to silt up beyond the reach of any but the highest spring tides.

. The reclamation of the Ribble has not yet proceeded so far as to so seriously affect the general conditions of the estuary; but here, also, there is a constant tendency in the channels to shift, and the erosion which takes place when a high tide and wind combine is very remarkable.

A most important improvement was introduced in 1886, by Mr. G. F. Lyster, when it was decided to raise the water-level in certain of the docks by pumping, the wharves being heightened in proportion, and half-tide basins, or locks, made use of to compensate for the difference of level.

The area of the docks so treated in Liverpool is 78 acres, whilst at Birkenhead the whole area of the docks on that side of the river, amounting to 160 acres, is so raised.

The hydraulic power used in the docks is very large, the indicated horse-power of the engines amounting to 1,673 in the case of Liverpool, and 874 in that of Birkenhead; whilst the Hydraulic Power Company are supplying some 1,000 h.p. to railways and private firms.

The direct-acting hydraulic lifts of the Mersey Railway have now been at work for ten years, and through these, at St. James's Station, no less than 75,000,000 to 80,000,000 of passengers have passed with regularity and safety.

It is remarkable that, whilst Great Britain led the van in the introduction of steam locomotion, she has lagged in the rear as regards electric and other mechanical traction. This arose in the first instance from mistaken legislation, which strangled electrical enterprise, which is still much hampered by the reluctance of public authorities to permit the introduction of the necessary poles and wires into towns.

At the date of the latest published returns there were at work in the United States no less than 12,133 miles of electric, in addition to 599 miles of cable, tramway. Hardly a large village but has its installation, and vast have been the advantages derived from these facilities. In Brooklyn one company alone owns and works 260 miles of overhead trolley lines. With the exception of some small tramways at Portrush, Brighton, Blackpool, South Staffordshire, Hartlepool, &c., the only examples in this country of serious attempts to apply electro-motive force to the carriage of passengers are the City and South London Railway and the Liverpool Overhead Railway, the latter being the latest constructed, and having, therefore, benefited by the experience gained upon the London line.

This railway is over six miles long, a double line of the normal, or 4 ft. 8½ in. gauge, running on an iron viaduct for the whole length of the docks; the installation is placed for convenience of coal supply about one-third of the distance from the northern end. Particulars of this interesting work will be placed before the Section, but suffice it to say that a train service of three minutes each way is readily maintained, with trains carrying 112 passengers each, at an average speed of twelve miles per hour, including stoppages at fourteen intermediate stations.

During the last year, as before stated, 7½ million passengers were carried, the cost of traction per train mile being 3·4*d*.

The Hartlepool Tramway is proving successful, overhead trollies and electric traction having taken the place of a horse tramroad, which was a failure from a traffic point of view.

Careful researches are being prosecuted, and experiments made, with the intention of reducing the excessive weight of storage batteries. If this can be effected, they should prove very efficient auxiliaries, especially where, in passing through towns, underground conductors are dangerous, and overhead wires objectionable.

In connection with electric traction, it is very important to reduce, if possible, the initial force required for starting from rest. Whether this will be best attained by the improvement of bearings and their better lubrication, or by the storage, for starting purposes, of a portion at least of the force absorbed by the brakes, remains to be seen, but it is a fruitful field for research and experiment.

In the United States there is a very general and rapid displacement of the cable tramways by the overhead wire electric system. The latter has many opponents, owing, probably, to causes which are preventible.

Many accidents were caused by the adoption of very high tension currents, which, on the breakage of a wire, were uncontrollable, producing lamentable results.

The overhead wires were placed in the middle of the street, causing interference with the passage of fire-escapes.

The speed of the cars was excessive, resulting in many persons being run over.

The cable system, therefore, found many advocates, but the result of experience is in favour of electrical traction under proper safeguards.

The cable system can only compete with the electric system when a three-minute or quicker service is possible, or, say, when the receipts average 20*l*. per mile per day; it is impossible to make up lost time in running, and the cars cannot be 'backed.' If anything goes wrong with the cable the whole of the traffic is disorganised. The cost of installation is much greater than in the case of electricity, and extensions are difficult.

On the other hand, electricity lends itself to the demands of a growing district, and extensions are easily effected; it satisfies more easily the growing demands on the part of the public for luxury in service and car appointment. It is less expensive in installation, and works with greater economy. By placing the wire at the side of the street, and using a current of low voltage, the objections are greatly minimised, and the cars are much more easily controlled and manipulated. In cases of breakdown these are limited to the half-mile section, and do not completely disorganise the service. Electric cars have been worked successfully on gradients of 1 in 7.

The conduit slot system can be adopted with good results, provided care is taken in the design of the conduit, and allowance made for ample depth and clearance; a width of ¾-inch is now proved to be sufficient. Where, however, there are frequent turnouts, junctions, and intersecting lines, the difficulties are great, and the cost excessive.

The following figures represent the cost of a tramway, on this system, in America:—

|  | £ |  |
|---|---|---|
| Cost of track and conduit | 5,600 | (per mile of single track) |
| Insulator, boxer, and double conductor . | 480 |  |
| Asphalte paving on 6 inches of concrete to 2 feet outside double track | 1,500 |  |
|  | £7,580 |  |

Complete cost of operating 4 miles of double track for 24 hours per day with 2½ minute service, 4·55*d*. per train mile (exclusive of interest, taxes, &c).

One train consists of one motor car and one trailer.

The trains make a round trip of eight miles in one hour, with three minutes lay-off at each end.

The cost of keeping the slot clean comes to about 40*l*. per quarter, and the repairs to each plough conductor about 50*s*. per quarter.

Attempts have been made to obviate the necessity of the slot by what is known as the closed conduit : but at present the results are not encouraging.

The following figures will help to convey to the mind the great development which is taking place in America, as regards the earnings upon lines electrically equipped. They are derived from the Report of the State Board of Railroad Commissioners for Massachusetts.

|  | 1888 | 1894 | Increase |
|---|---|---|---|
| Net earnings per passenger carried . | ·48 | 78 | 62 5 per cent |
| Net earning per car mile . | 2·78 | 4 83 | 73 56 , |
| Net earning per mile of road . | £484 | £762 | 57 .. |

In addition to the application of electricity for illuminating purposes, and for the driving of tram cars and railways, it has also been applied successfully to the driving of machinery, cranes, lifts, tools, pumps, &c., in large factories and works. This has proved of the greatest convenience, abolishing as it does the shafting of factories, and applying to each machine the necessary power by its own separate motor; the economy resulting from this can hardly be over-estimated.

It is also successfully employed in the refining of copper, and in the manufacture of phosphorus, aluminium, and other metals, which, before its application, were beyond the reach of commercial application.

The extent of its development for chemical purposes in the future no one can foresee.

It is hardly necessary to call attention to the successful manner in which the Falls of Niagara, and the large Falls of Switzerland, and elsewhere, are being harnessed and controlled for the use of man, and in which horse-power by thousands is being obtained.

At Niagara, single units of electrical plant are installed equal to about 5,000 horse-power output   These units are destined to be utilised for any of the purposes previously suggested, and it is computed that one horse-power can be obtained from the river, and sold for the entire year day and night continuously, for the sum of 3*l*. 2*s*. 6*d*. per annum

Electric head lights are being adopted for locomotives in the United States

The use of compressed air and compressed gas for tractive purposes is at present in an experimental stage in this country.  The latter is claimed to be the cheapest for tramway purposes, the figures given being—

|  | *d.* |
|---|---|
| Single horse cars  .    .  .    .   .   . | $5\frac{3}{4}$ |
| Electrical cars, with overhead wires . | $4\frac{4}{4}$ |
| Gas cars  .     .   .   .    . | $3\frac{1}{4}$ |

Combination steam and electric locomotives, gazoline, compressed air, and hotwater motors are all being tried in the United States, but definitive results are not yet published.

The first electric locomotive practically applied to hauling heavy trains was put into service on the Baltimore and Ohio Railway in 1895 to conduct the traffic through the Belt Line Tunnel.

It is stated that, not only was the guaranteed speed of 30 miles per hour attained, but, with the locomotive running light, it reached double that speed.

On the gradient of 8 per cent. a composite train of forty-four cars, loaded with coal and lumber, and three ordinary locomotives—weighing altogether over 1,800 tons—was started easily and gradually to a speed of 12 miles an hour without slipping a wheel.  The voltage was 625.  The current recorded was, at starting, about 2,200 ampères, and, when the train was up to speed, it settled down to about 1,800 ampères.  The drawbar pull was about 63,000 lbs.

The actual working expense of this locomotive is stated to be about the same as for an ordinary goods locomotive—viz. 23 cents per engine mile.

The rapid extension of tunnel construction for railway purposes, both in towns and elsewhere, is one of the remarkable features of the period under review, and has been greatly assisted by the use of shields, with and without compressed air. This brings into considerable importance the question of mechanical ventilation. Amongst English tunnels, ventilation by fan has been applied to those under the Severn and the Mersey. The machinery for the latter is, probably, the most complete and most scientific application up to the present time.

There are five ventilating fans, two of which are 40 feet in diameter, and 12 feet wide on the blades; two of 30 feet, and 10 feet wide; and one quick-running fan of 16 feet in diameter, all of which were ably installed by Messrs. Walker Brothers of Wigan. They are arranged, when in full work, to throw 800,000 cubic feet of air per minute, and to empty the tunnel between Woodside and St. James's Street in eight minutes; but, unfortunately, it is found necessary, for financial reasons, not to work the machinery to its full capacity.

The intended extension of electrical underground railways will render it necessary for those still employing steam traction either to ventilate by machinery or to substitute electro-motive force.

Great improvements have been lately made in the details of mechanical ventilators, especially by the introduction of anti-vibration shutters, and the driving by belts or ropes instead of direct from the engine. The duties now usually required for mining purposes are about 300,000 cubic feet of air per minute with a water-gauge of about 4 inches; but one installation is in hand for 500,000 cubic feet of air per minute, with a water-gauge of 6 inches. Water-gauge up to 10 inches can now be obtained with fans of 15 feet diameter only.

An interesting installation has been made at the Pracchia Tunnel on the Florence and Bologna Railway.

The length of the tunnel is 1,900 metres, or about 2,060 yards; it is for a single line, and is on a gradient of 1 in 40. When the wind was blowing in at the lower end, the steam and smoke of an ascending train travelled concurrently with the train, thus producing a state of affairs almost unimaginable except to those engaged in working the traffic.

Owing to the height of the Apennines above the tunnel, ventilating shafts are impracticable; but it occurred to Signor Saccardo that, by blowing air by means of a fan into the mouth of the tunnel, through the annular space which exists between the inside of the tunnel arch and the outside of the traffic gauge, a sufficient current might be produced to greatly ameliorate the state of things.

The results have been most satisfactory, the tunnel, which was formerly almost dangerous, under certain conditions of weather, being now kept cool and fresh, with but a small expenditure of power.

In an age when, fortunately, more attention is paid than formerly to the well-being of the men, the precautions necessary to be observed in driving long tunnels, and especially in the use of compressed air, are receiving the consideration of engineers. In the case of the intended Simplon Tunnel, which will pierce the Alps at a point requiring a length of no less than 12½ miles, a foreign commission of engineers was entrusted by the Federal Government of Switzerland with an investigation of this amongst other questions.

During the construction of the St. Gothard Tunnel, which is about 10 miles in length, the difficulties encountered were, of necessity, very great; the question of ventilation was not fully understood, nor was sanitary science sufficiently advanced to induce those engaged in the work to give it much attention. The results were lamentable, upwards of 600 men having lost their lives, chiefly from an insidious internal malady not then understood. But the great financial success of this international tunnel has been so marked, as to justify the proposed construction of a still longer tunnel under the Simplon

The arrangements which are to be adopted for securing the health of the *employés* are admirable, and will surely not only result in reducing the death rate to a minimum, but also tend to shorten the time necessary for the execution of the undertaking to one-half.

The quantity of air to be forced into the workings will be twenty times greater than

in previous works. Special arrangements are devised for reducing the temperature of the air by many degrees, suitable houses are to be provided for the men, with excellent arrangements for enabling them to change their mining clothes, wet with the water of the tunnel, before coming in contact with the Alpine cold; every man will have a bath on leaving; his wet clothes will be taken care of by a custodian, and dried ready for his return to work; suitable meals of wholesome food will be provided, and he will be compelled to rest for half-an-hour on emerging from the tunnel, in pleasant rooms furnished with books and papers. This may appear to some as excessive care; but kind and humane treatment of men results, not only in benefit to them, but also in substantial gain to those employing them, and the endeavour of our own authorities, and of Parliament, to secure for our own workpeople the necessary protection for their lives and limbs in carrying out hazardous trades and employments, is worthy of admiration.

The great improvements in sub-aqueous tunnelling can be clearly recognised from the fact that the Thames Tunnel cost 1,150*l.* per lineal yard, whilst the Blackwall Tunnel, consisting of iron lined with concrete, and of 25 feet internal diameter, has, by means of Greathead's shield and grouting machine, been driven from shaft to shaft a distance of 754 yards for 375*l.* per yard.

Tunnels have now been successfully constructed through the most difficult strata, such as waterbearing silt, sand, and gravel, and, by the use of grouting under pressure, subsidence can almost entirely be avoided, thus rendering the piercing of the substrata of towns, underneath property without damaging it, a simple operation; and opening up to practical consideration many most important lines of communication hitherto considered out of the question.

On the other hand, very little improvement has taken place in the mode of constructing tunnels in ordinary ground, since the early days of railways  The engineers and contractors of those days adopted systems of timbering and construction which have not been surpassed.  The modern engineer is, however, greatly assisted by the possibility of using Brindle bricks of great strength to resist pressure, combined with quick-setting Portland cement, by the great improvement which have taken place in pumping machinery, and by the use of the electric light during construction.

A question which is forcing itself upon the somewhat unwilling attention of our great railway companies, in consequence of the continual great increase of the population of our cities, is the pressing necessity for a substantial increase in the size of the terminal stations in the great centres of population.

Many of our large terminal stations are not of sufficient capacity to be worked properly, either with regard to the welfare of the staff, or to the convenience of the travelling public.

Speak to station-masters and inspectors on duty, when the holiday season is on, and they will tell you of the great physical strain that is produced upon them and their subordinates, in endeavouring to cope with the difficulty.

This, if nothing else, is a justification for the enterprise of the Manchester, Sheffield and Lincolnshire Railway Company in providing an entirely new terminus for London.

It is thirty years since the last, that of St. Pancras, was added, and during that period the population of London has increased by no less than two millions.

The discussion, both in and out of Parliament, of the proposals for light railways has developed a considerable amount of interest in the question.  Experience only can prove whether they will fulfil the popular expectations.  If the intended branch lines are to be of the standard gauge, with such gradients and curves as will render them suitable for the ordinary rolling-stock, they will, in many cases, not be constructed at such low mileage costs as to be likely to be remunerative at rates that would attract agricultural traffic.  The public roads of this country (very different from the wide and level military roads of Northern Italy and other parts of the Continent) do not usually present facilities for their utilisation, and, once admitted, the necessity for expropriating private property, the time-honoured questions of frontage severances and interference with amenities will force their way to the front, fencing will be necessary, and,

even if level crossings be allowed at public roads, special precautions will have to
be taken.

Much must then depend upon the regulations insisted upon by the Board of
Trade. If, in consideration of a reduction in speed, relaxation of existing safe-
guards are permitted, much may, no doubt, be effected by way of feeders to existing
main lines.

If, on the other hand, the branches are of narrower gauge, separate equipment
will be necessary, and transhipment at junctions will involve both expense and
delay. It is very doubtful whether the British farmer would benefit much from
short railways of other than standard gauge   He must keep horses for other pur-
poses, and he will probably still prefer to utilise them for carting his produce to the
nearest railway station of the main line, or to the market town

The powers granted by the Light Railways Act, in the hands of the able
Commissioners appointed under the Act, cannot, however, fail to be a public boon.

Special Acts of Parliament will be unnecessary, facilities will be granted,
procedure simplified, some Government aid rendered, and probably the heavy
burden of a Parliamentary deposit will be removed.

It would seem quite probable, that motor cars may offer one practical solution of
the problem how best to place the farms of the country in commercial touch with
the trunk railways, seaports, and market towns.   They could use existing roads,
could run to the farmyard or field, and receive or deliver produce at first hand.

Such means of locomotion were frequently proposed towards the end of the
last century, and in the early part of the present one, and it was not until the year
1840, that the victory of the railway over steam upon common roads was assured,
the tractive force required being then shown to be relatively as 1 to 7

The passing of the Act of 1896, superseding those of 1861 and 1865, will
undoubtedly mark the commencement of a new era in mechanical road traction
The cars, at present constructed chiefly by German and French engineers, are
certainly of crude design, and leave much to be desired.   They are ugly in appear-
ance, noisy, difficult to steer, and vibrate very much with the revolutions of their
engines, rising as they do to 400 per minute ; those driven by oil give out offensive
odours, and cannot be readily started, so that the engine runs on during short
stops     There would seem to be arising here an even more important opening for
the skill of our mechanical engineers than in the case of bicycles, in which
wonderful industry the early steps appear also to have been foreign.

It is claimed for a motor car that it costs no more than carriage, horse, and
harness, that the repairs are about the same, and that, whilst a horse, travelling
20 miles per day, represents for fodder a cost of $2d.$ per mile, a motor car of
$2\frac{1}{2}$ horse-power will run the same distance at $\frac{1}{2}$d per mile.

The highway authorities should certainly welcome the new comer, for it is
estimated that two-thirds of the present wear and tear of roads is caused by
horses, and one-third only by wheels.

Perhaps no invention has had so widespreading an influence on the construction
of railways as the adoption of the Bessemer process for the manufacture of steel rails.
This has substituted a homogeneous crystalline structure, of great strength and
uniformity, for the iron rails of former years, built up by bundles of bars, and
therefore liable to lamination and defective welds   The price has been reduced
from the $13l.$ per ton, which iron rails once reached, to $3l.$ $15s.$ as a minimum for
steel.   There are, however, not infrequently occurring, in the experience of rail-
way companies, the cracking, and even fracture of steel rails, and the Government
has lately appointed a Board of Trade Committee for the investigation, inciden-
tally of this subject, but specially of the important question of the effect of fatigue
upon the crystallisation, structure, and strength, of the rail.   Experience proves, at
any rate, that it is of great importance to remove an ample length of crop end, as
fractures more frequently take place near the ends, aided by the weakening caused
by bolt holes.   Frequent examination by tapping, as in the case of tyres, seems, at
present, the most effective safeguard.

It is open to serious question, whether the great rigidity of the permanent way
of the leading railways of this country is an advantage.   Certainly the noise is

very great, more so than in other countries, and this points to severe shocks, heavy wear and tear of rails and tyres, and—especially when two heavy locomotives are run with the same train—liability to fracture. Whilst the tendency in this country, and in the United States, has been to gradually increase the weight of rails from 40 lbs up to 100 lbs. per lineal yard, there are engineers who think that to decrease the rigidity of rail and fishplate, and weight of chair, and to increase the sleepers, so as to arrive as nearly as possible at a continuous bearing, would result in softness and smoothness of running.

The average and maximum speeds now attained by express trains would appear to have reached the limit of safety, at any rate under the existing conditions of junctions, cross-over roads, and other interferences with the continuity of the rail. If higher speeds are to be sought, it would seem to be necessary to have isolated trunk lines, specially arranged in all their details, free from sharp curve and severe gradient, and probably worked electrically, although a speed of 100 miles per hour is claimed to have been reached by a steam locomotive in the United States.

The grain trade of the port of Liverpool has assumed very large proportions, and the system of storage in large silos has been adopted, with great advantage, both as regards capital, outlay, and the cost of working, per ton of grain.

The Liverpool Grain Storage Warehouses at Bootle will be open to Members of the Association, and there can be seen the latest development of the mechanical unloading, storing and distribution of grain in bulk; the capacity is large, being:—

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Warehouse No  1, 56,000 tons ⎤
     „      „   2, 30,000  „  ⎬ or 4,240,000 bushels
Quay Stores      20,000  „  ⎦
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thus constituting this granary as one of the largest, if not the largest, in the world.

The question of the pressure of grain is a very difficult one, and, in constructing the brick silos, which are 12 feet across at the top, by nearly 80 feet in depth, large allowance has been made both for ordinary pressure, and for possible swelling of the grain.

The grain is unloaded by elevators, and then transported on bands, the result being its cooling and cleansing, as well as its storage and distribution.

The question of the early adoption in England of the metric system is of importance not only to the engineering profession, but also to the country at large. The recommendation of the recent Royal Commission, appointed for the consideration of the subject, was, that it should be taught at once in all schools, and that, in two years' time, its adoption should be compulsory; but it is much to be regretted that, up to the present time, nothing has been done.

The slight and temporary inconvenience of having to learn the system is of no moment compared to the great assistance it would prove to the commercial and trading world; the simplification of calculations and of accounts would be hailed with delight by all so soon as they realised the advantages. England is suffering greatly in her trade with the Continent for want of it.

Our foreign customers, who have now used it for many years, will not tolerate the inconvenience of the endless variety of weights and measures in use in England, and they consequently purchase their goods, to a great extent, from Germany, rather than use our antiquated English system. It is no exaggeration to say that, with their knowledge of the metric system, they regard ours as completely obsolete and unworkable, just in the same way as we should were we to buy our corn, our wine, our steel and iron, by the hin, the ephah, or the homer, or to compute our measurements by cubit, stadium, or parasang.

It behoves all who desire to see England regain her trade to use all their influence in favour of the adoption of this system, as its absence is, doubtless, one of the contributory causes for the loss that has taken, and is taking, place.

An important argument in favour of the metric system of weights and measures is that it is adopted all over the civilised world by physicists and chemists; and it may be stated with confidence, that the present international character of these sciences is largely due to this.

It is interesting also to notice, that the metric system is being gradually introduced into other branches of science. Anthropometric measurements made by the Committees of the British Association in this country and in Canada are invariably given in metres, and a comparison with measurements made in other countries can be at once made.

The period of twenty-five years under review has indeed witnessed great advances, both in scientific knowledge and practical application. This progress has led to powerful yet peaceful competition between the leading nations. Both from among our cousins of the United States, and from our nearer neighbours of Europe, have we, at this Meeting, the pleasure of welcoming most respected representatives. But their presence, and the knowledge of the great discoveries made, and colossal works carried out, by them and their brother scientists and engineers, must make us of Great Britain face with increased earnestness the problem of maintaining our national position, at any rate, in the forefront of all that tends towards the 'utilisation of the great sources of power in Nature for the use and convenience of man.' Those English engineers who have been brought in contact with engineering thought and action in America and abroad have been impressed with the thoroughness of much of the work, the great power of organisation, and the careful reliance upon scientific principles constantly kept in view, and upon chemical and mechanical experiments, carried out often upon a much more elaborate scale than in this country. This is not the place from which to discuss the questions of bounties and tariffs, which have rendered possible powerful competition for the supply of machinery and railway plant from the Continent to our own Colonies; but there is certainly need for advance all along the line of mechanical science and practice, if we are to hold our own—need especially to study the mechanical requirements of the world, ever widening and advancing, and to be ready to meet them, by inventive faculty first, but also by rigid adherence to sound principles of construction, to the use of materials and workmanship of the highest class, to simplicity of design and detail, and to careful adaptation of our productions to the special circumstances of the various markets.

It is impossible to forecast in what direction the great advances since 1871 will be equalled and exceeded in the coming quarter of a century. Progress there will and must be, probably in increased ratio; and some, at the end of that period, may be able to look back upon our gathering here in Liverpool in 1896 as dealing with subjects then long since left behind in the race towards perfection.

The mechanical engineer may fairly hope for still greater results in the perfection of machinery, the reduction of friction, the economical use of fuel, the substitution of oil for coal as fuel in many cases, and the mechanical treatment of many processes still dependent upon the human hand.

The electrical engineer (hampered as he has been in this country by unwise and retrograde legislation) may surely look forward to a wonderful expansion in the use of that mysterious force, which he has already learned so wonderfully to control, especially in the direction of traction.

The civil engineer has still great channels to bridge or tunnel, vast communities to supply with water and illuminating power, and (most probably with the assistance of the electrician) far higher speeds of locomotion to attain. He has before him vast and ever-increasing problems for the sanitary benefit of the world, and it will be for him to deal from time to time with the amazing internal traffic of great cities. China lies before him, Japan welcomes all advance, and Africa is great with opportunities for the coming engineers.

Let us see to it, then, that our rising engineers are carefully educated and prepared for these responsibilities of the future, and that our scientific brethren may be ever ready to open up for them by their researches fresh vistas of possibilities, fresh discoveries of those wonderful powers and facts of Nature which man to all time will never exhaust.

The Mechanical Section of the British Association has done good work in this direction in the past, and we may look forward with confidence to our younger brethren to maintain these traditions in the future.

# British Association for the Advancement of Science.

## LIVERPOOL, 1896.

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# ADDRESS

### TO THE

# ANTHROPOLOGICAL SECTION.

#### BY

### ARTHUR J. EVANS, M.A., F.S.A.,

PRESIDENT OF THE SECTION.

*' The Eastern Question ' in Anthropology.*

TRAVELLERS have ceased to seek for the ' Terrestrial Paradise,' but, in a broader sense, the area in which lay the cradle of civilised mankind is becoming generally recognised. The plateaux of Central Asia have receded from our view. Anthropological researches may be said to have established the fact that the White Race, in the widest acceptation of the term, including, that is, the darker-complexioned section of the South and West, is the true product of the region in which the earliest historic records find it concentrated Its ' Area of Characterisation is conterminous, in fact, with certain vast physical barriers due to the distribution of sea and land in the latest geological period. The continent in which it rose, shut in between the Atlantic and the Indian Oceans, between the Libyan Desert, and what is now Sahara, and an icier Baltic stretching its vast arms to the Ponto-Caspian basin, embraced, together with a part of anterior Asia, the greater part of Europe, and the whole of Northern Africa The Mediterranean itself—divided into smaller separate basins, with land bridges at the Straits of Gibraltar, and from Sicily and Malta to Tunis—did not seriously break the continuity of the whole. The English Channel, as we know, did not exist, and the old sea-coast of what are now the British Islands, stretching far to the west, is, as Professor Boyd Dawkins has shown, approximately represented by the hundred-fathom line. To this great continent Dr. Brinton, who has so ably illustrated the predominant part played by it in isolating the white from the African black and the yellow races of mankind, has proposed to give the useful and appropriate name of ' Eurafrica.' In ' Eurafrica,' in its widest sense, we find the birthplace of the highest civilisations that the world has yet produced, and the mother country of its dominant peoples

It is true that later geological changes have made this continental division no longer applicable. The vast land area has been opened to the east, as if to invite the Mongolian nomads of the Steppes and Tundras to mingle with the European population ; the Mediterranean bridges, on the other hand, have been swept away. Asia has advanced, Africa has receded Yet the old underlying connexion of the peoples to the north and south of the Mediterranean basin seems never to have been entirely broken. Their inter-relations affect many of the most interesting phenomena of archæology and ancient history, and the old geographical unity of ' Eurafrica ' was throughout a great extent of its area revived in the great political system which still forms the basis of civilised society, the Roman Empire. The Mediterranean was a Roman lake A single fact brings home to us the extent to

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which the earlier continuity of Europe and North Africa asserted itself in the imperial economy. At one time, what is now Morocco and what is now Northumberland, with all that lay between them on both sides of the Pyrenees, found their administrative centre on the Mosel.

It is not for me to dwell on the many important questions affecting the physiological sides of ethnography that are bound up with these old geographical relations. I will, however, at least call attention to the interesting, and in many ways original, theory put forward by Professor Sergi in his recent work on the 'Mediterranean Race.'

Professor Sergi is not content with the ordinary use of the term 'White Race.' He distinguishes a distinct 'brown' or 'brunette' branch, whose swarthier complexion, however, and dark hair bear no negroid affinities, and are not due to any intermixture on that side. This race, with dolichocephalic skulls, amongst which certain clearly defined types constantly repeat themselves, he traces throughout the Mediterranean basin, from Egypt, Syria, and Asia Minor, through a large part of Southern Europe, including Greece, Italy, and the Iberic peninsula, to the British islands. It is distributed along the whole of North Africa, and, according to the theory propounded, finds its original centre of diffusion somewhere in the parts of Somaliland.

It may be said at once that this grouping together into a consistent system of ethnic factors spread over this vast yet inter-related area—the heart of 'Eurafrica'—presents many attractive aspects. The ancient Greek might not have accepted kinship even with 'the blameless Ethiopian,' but those of us who may happen to combine a British origin with a Mediterranean complexion may derive a certain ancestral pride from remote consanguinity with Pharaoh. They may even be willing to admit that 'the Ethiopian' in the course of his migrations has done much to 'change his skin.'

In part, at least, the new theory is little more than a re-statement of an ethnographic grouping that commands a general consensus of opinion. From Thurnam's time onwards we have been accustomed to regard the dolichocephalic type found in the early Long Barrows, and what seem to have been the later survivals of the same stock in our islands, as fitting on to the Iberian element in South-western Europe. The extensive new materials accumulated by Dr. Garson have only served to corroborate these views, while further researches have shown that the characteristic features of the skeletons found in the Ligurian caves, at Cro Magnon and elsewhere in France, are common to those of a large part of Italy, Sicily, and Sardinia, and extend not only to the Iberic group, but to the Guanche interments of the Canary Islands.

The newly correlated data unquestionably extend the field of comparison; but the theories as to the original home of this 'Mediterranean race' and the course of its diffusion may be thought to be still somewhat lacking in documentary evidence. They remind us rather too closely of the old 'Aryan' hypothesis, in which we were almost instructed as to the halting places of the different detachments as they passed on their way from their Central Asian cradle to rearrange themselves with military precision, and exactly in the order of their relationship, in their distant European homes. The existing geological conditions are made the basis of this migratory expansion from Ethiopia to Ireland; parallel streams move through North Africa and from Anatolia to Southern Europe. One cardinal fact has certainly not received attention, and that is, that the existing evidence of this Mediterranean type dates much further back on European soil than even in ancient Egypt

Professor Sergi himself has recognised the extraordinary continuity of the cranial type of the Ligurian caves among the modern population of that coast.

But this continuity involves an extreme antiquity for the settlement of the 'Mediterranean Race' in North-western Italy and Southern France. The cave interments, such as those of the Finalese, carry back the type well into Neolithic times. But the skeletons of the Baoussé Roussé caves, between Mentone and Ventimiglia, which reproduce the same characteristic forms, take us back far behind any stage of culture to which the name of Neolithic can be properly applied.

The importance of this series of interments is so unique, and the fulness of the evidence so far surpasses any other records immediately associated with the earliest remains of man, that even in this brief survey they seem to demand more than a

So much, at least, must be admitted on all hands : an earlier stage of culture is exhibited in these deposits than that which has hitherto been regarded as the minimum equipment of the men of the later Stone Age  The complete absence of pottery, of polished implements, of domesticated animals—all the more striking from the absolute contrast presented by the rich Neolithic cave burials a little further up the same coast —how is it to be explained ?  The long flint knives, the bone and shell ornaments, might, indeed, find partial parallels among Neolithic remains ; but does not, after all, the balance of comparison incline to that more ancient group belonging to the ' Reindeer Period ' in the South of France, as illustrated by the caves of La Madeleine, Les Eyzies and Solutré ?

It is true that, in an account of the interments found in 1892 in the Barma Grande Cave, given by me to the Anthropological Institute, I was myself so prepossessed by the still dominant doctrine that the usage of burial was unknown to Palæolithic man, and so overpowered by the vision of the yawning hiatus between him and his Neolithic successor, that I failed to realise the full import of the evidence.  On that occasion I took refuge in the suggestion that we had here to deal with an earlier Neolithic stratum than any hitherto recorded.  'Neolithic,' that is, without the Neolithic.

But the accumulation of fresh data, and especially the critical observations of M. d'Acy and Professor Issel, have convinced me that this intermediate position is untenable.  From the great depth below the original surface, of what in all cases seem to have been homogeneous quaternary deposits, at which the human remains were found, it is necessary to suppose, if the interments took place at a later period, that pits in many cases from 30 to 40 feet deep must have been excavated in the cave earth.  But nothing of the kind has been detected, nor any intrusion of extraneous materials.  On the other hand, the gnawed or defective condition of the extremities in several cases points clearly to superficial and imperfect interment of the body ; and in one case parts of the same core from which flints found with the skeleton had been chipped were found some metres distant on the same floor level. Are we, then, to imagine that another pit was expressly dug to bury these ?

In the case of a more recently discovered and as yet unpublished interment, at the excavation of which I was so fortunate as to assist, the superficial character of the deposit struck the eye.  The skeleton, with flint knife and ochre near, decked out with the usual shell and deer's tooth ornaments, lay as if in the attitude of sleep, somewhat on the left side.  The middle of the body was covered with a large flat stone, with two smaller ones lying by it, while another large stone was laid over the feet.  The left arm was bent under the head as if to pillow it, but the extremities of the right arm and the toes were suggestively deficient : the surface covering of big stones had not sufficiently protected them.  The stones themselves seem in turn to have served as a kind of hearth, for a stratum of charred and burned bones about 45 cm. thick lay about them.

Is it reasonable to suppose that a deposit of this kind took place at the bottom of a pit over 20 feet deep, left open an indefinite time for the convenience of roasting venison at the bottom ?

A rational survey of the evidence in this as in the other cases leads to the conclusion that we have to deal with surface burial, or, if that word seems too strong, with simple ' seposition '—the imperfect covering with handy stones of the dead bodies as they lay in the attitude of sleep on the then floor of the cavern.  In other words, they are in situ in a late quaternary deposit, for which Professor Issel has proposed the name of ' Meiolithic.'

But if this conclusion is to hold good, we have here on the northern coast of the Mediterranean evidence of the existence of a late Palæolithic race, the essential features of which, in the opinion of most competent osteological inquirers, reappear in the Neolithic skeletons of the same Ligurian coast, and still remain characteristic of the historical Ligurian type.  In other words, the ' Mediterranean Race ' finds

its first record in the West; and its diffusion, so far from having necessarily followed the lines of later geographical divisions, may well have begun at a time when the land bridges of ' Eurafrica' were still unbroken.

There is nothing, indeed, in all this to exclude the hypothesis that the original expansion took place from the East African side. That the earliest homes of primæval man lay in a warm region can hardly be doubted, and the abundant discovery by Mr. Seton Karr in Somaliland of Palæolithic implements reproducing many of the most characteristic forms of those of the grottoes of the Dordogne affords a new link of connexion between the Red Sea and the Atlantic littoral.

When we recall the spontaneous artistic qualities of the ancient race which has left its records in the carvings on bone and ivory in the caves of the 'Reindeer Period,' this evidence of at least partial continuity on the northern shores of the Mediterranean suggests speculations of the deepest interest. Overlaid with new elements, swamped in the dull, though materially higher, Neolithic civilisation, may not the old æsthetic faculties which made Europe the earliest-known home of anything that can be called human art, as opposed to mere tools and mechanical contrivances, have finally emancipated themselves once more in the Southern regions, where the old stock most survived? In the extraordinary manifestations of artistic genius to which, at widely remote periods, and under the most diverse political conditions, the later populations of Greece and Italy have given birth, may we not be allowed to trace the re-emergence, as it were, after long underground meanderings, of streams whose upper waters had seen the daylight of that earlier world?

But the vast gulf of time beyond which it is necessary to carry back our gaze in order to establish such connexions will hardly permit us to arrive at more than vague probabilities. The practical problems that concern the later culture of Europe from Neolithic times onwards connect themselves rather with its relation to that of the older civilisations on the southern and eastern Mediterranean shores.

Anthropology, too, has its ' Eternal Eastern Question.' Till within quite recent years, the glamour of the Orient pervaded all inquiries as to the genesis of European civilisation. The Biblical training of the northern nations prepared the ground. The imperfect realisation of the antiquity of European arts; on the other hand, the imposing chronology of Egypt and Babylonia; the abiding force of classical tradition, which found in the Phœnician a *deus ex machinâ* for exotic importations; finally, the 'Aryan Hypothesis,' which brought in the dominant European races as immigrant wanderers from Central Asia, with a ready-made stock of culture in their wallets—these and other causes combined to create an exaggerated estimate of the part played by the East as the illuminator of the benighted West.

More recent investigations have resulted in a natural reaction. The primitive ' Aryan' can be no longer invoked as a kind of patriarchal missionary of Central Asian culture. From d'Halloy and Latham onwards to Penka and Schrader an array of eminent names has assigned to him an European origin. The means by which a kindred tongue diffused itself among the most heterogeneous ethnic factors still remain obscure, but the stricter application of phonetic laws and the increased detection of loan-words has cut down the original ' Aryan' stock of culture to very narrow limits, and entirely stripped the members of this linguistic family of any trace of a common Pantheon

Whatever the character of the original · Aryan' stage, we may be very sure that it lies far back in the mists of the European Stone Age. The supposed common names for metals prove to be either a vanishing quantity or strikingly irrelevant. It may be interesting to learn on unimpeachable authority that the Celtic words for ' gold' are due to comparatively recent borrowing from the Latin; but nothing is more certain than that gold was one of the earliest metals known to the Celtic races, its knowledge going back to the limits of the pure Stone Age We are told that the Latin ' ensis,' ' a sword,' is identical with the Sanskrit ' asi ' and Iranian ' ahi,' but the gradual evolution of the sword from the dagger, only completed at a late period of the Bronze Age, is a commonplace of prehistoric archæology. If ' ensis,' then, in historical times an iron sword, originally meant a

bronze dagger, may not the bronze dagger in its turn resolve itself into a flint knife?

The truth is that the attempts to father on a common Aryan stock the beginnings of metallurgy argue an astonishing inability to realise the vast antiquity of languages and their groups. Yet we know that, as far back as we have any written records, the leading branches of the Aryan family of speech stood almost as far apart as they do to-day, and the example of the Egyptian and Semitic groups, which Maspero and others consider to have been originally connected, leads to still more striking results. From the earliest Egyptian stela to the latest Coptic liturgy we find the main outlines of what is substantially the same language preserved for a period of some six thousand years. The Semitic languages in their characteristic shape show a continuous history almost as extensive. For the date of the diverging point of the two groups we must have recourse to a chronology more familiar to the geologist than the antiquary.

As importer of exotic arts into primitive Europe the Phœnician has met the fate of the immigrants from the Central Asian 'Arya.' The days are gone past when it could be seriously maintained that the Phœnician merchant landed on the coast of Cornwall, or built the dolmens of the North and West. A truer view of primitive trade as passing on by inter-tribal barter has superseded the idea of a direct commerce between remote localities. The science of prehistoric archæology, following the lead of the Scandinavian School, has established the existence in every province of local centres of early metallurgy, and it is no longer believed that the implements and utensils of the European Bronze Age were imported wholesale by Semites or 'Etruscans.'

It is, however, the less necessary for me to trace in detail the course of this reaction against the exaggerated claims of Eastern influence that the case for the independent position of primitive Europe has been recently summed up with fresh arguments, and in his usual brilliant and incisive style, by M. Salomon Reinach, in his 'Mirage Orientale.' For many ancient prejudices as to the early relations of East and West it is the trumpet sound before the walls of Jericho. It may, indeed, be doubted whether, in the impetuousness of his attack, M. Reinach, though he has rapidly brought up his reserves in his more recent work on primitive European sculpture, has not been tempted to occupy outlying positions in the enemy's country which will hardly be found tenable in the long run. I cannot myself, for instance, be brought to believe that the rude marble 'idols' of the primitive Ægean population were copied on Chaldæan cylinders. I may have occasion to point out that the oriental elements in the typical higher cultures of primitive Europe, such as those of Mycenæ, of Hallstatt, and La Tène, are more deeply rooted than M. Reinach will admit. But the very considerable extent to which the early European civilisation was of independent evolution has been nowhere so skilfully focussed into light as in these comprehensive essays of M. Reinach It is always a great gain to have the extreme European claims so clearly formulated, but we must still remember that the 'Sick Man' is not dead.

The proofs of a highly developed metallurgic industry of home growth accumulated by prehistoric students *pari passu* over the greater part of Europe, and the considerable cultural equipment of its early population—illustrated, for example, in the Swiss Lake settlements—had already prepared the way for the more startling revelations as to the prehistoric civilisation of the Ægean world which have resulted from Dr. Schliemann's diggings at Troy, Tiryns, and Mycenæ, so admirably followed up by Dr. Tsountas.

This later civilisation, to which the general name of 'Ægean' has been given, shows several stages, marked in succession by typical groups of finds, such as those from the Second City of Troy, from the cist-graves of Amorgos, from beneath the volcanic stratum of Thera, from the shaft-graves of Mycenæ, and again from the tombs of the lower town. Roughly, it falls into two divisions, for the earlier of which the culture illustrated by the remains of Amorgos may be taken as the culminating point, while the later is inseparably connected with the name of Mycenæ.

The early 'Ægean' culture rises in the midst of a vast province extending from

Switzerland and Northern Italy through the Danubian basin and the Balkan peninsula, and continued through a large part of Anatolia, till it finally reaches Cyprus. It should never be left out of sight that, so far as the earliest historical tradition and geographical nomenclature reach back, a great tract of Asia Minor is found in the occupation of men of European race, of whom the Phrygians and their kin—closely allied to the Thracians on the other side of the Bosphorus—stand forth as the leading representatives. On the other hand, the great antiquity of the Armenoid type in Lycia and other easterly parts of Asia Minor, and its priority to the Semites in these regions, has been demonstrated by the craniological researches of Dr. von Luschan. This ethnographic connexion with the European stock, the antiquity of which is carried back by Egyptian records to the second millennium before our era, is fully borne out by the archæological evidence. Very similar examples of ceramic manufactures recur over the whole of this vast region. The resemblances extend even to minutiæ of ornament, as is well shown by the examples compared by Dr. Much from the Mondsee, in Upper Austria, from the earliest stratum of Hissarlik, and from Cyprus. It is in the same Anatolo-Danubian area—as M. Reinach has well pointed out—that we find the original centre of diffusion of the ' Svastika ' motive in the Old World Copper implements, and weapons too, of primitive types, some reproducing Neolithic forms, are also a common characteristic, though it must always be remembered that the mere fact that an implement is of copper does not of itself necessitate its belonging to the earliest metal age, and that the freedom from alloy was often simply due to a temporary deficiency of tin. Cyprus, the land of copper, played, no doubt, a leading part in the dissemination of this early metallurgy, and certain typical pins and other objects found in the Alpine and Danubian regions have been traced back by Dr. Naue and others to Cypriote prototypes. The same parallelism throughout this vast area comes out again in the appearance of a class of primitive ' idols ' of clay, marble, and other materials, extending from Cyprus to the Troad and the Ægean islands, and thence to the pile settlements of the Alps and the Danubian basin, while kindred forms can be traced beyond the Carpathians to a vast northern Neolithic province that stretches to the shores of Lake Ladoga.

It is from the centre of this old Anatolo-Danubian area of primitive culture, in which Asia Minor appears as a part of Europe, that the new Ægean civilisation rises from the sea. ' Life was stirring in the waters.' The notion that the maritime enterprise of the Eastern Mediterranean began on the exposed and comparatively harbourless coast of Syria and Palestine can no longer be maintained. The island world of the Ægean was the natural home of primitive navigation. The early sea-trade of the inhabitants gave them a start over their neighbours, and produced a higher form of culture, which was destined to react on that of a vast European zone—nay, even upon that of the older civilisations of Egypt and Asia.

The earlier stage of this Ægean culture culminates in what may conveniently be called the Period of Amorgos from the abundant tombs explored by Dr. Dummler and others in that island. Here we already see the proofs of a widespread commerce. The ivory ornaments point to the South ; the abundance of silver may even suggest an intercourse along the Libyan coast with the rich silver-producing region of South-eastern Spain, the very ancient exploitation of which has been so splendidly illustrated by the researches of the brothers Siret. Additional weight is lent to this presumption by the recurrence in these Spanish deposits of pots with rude indications of eyes and eyebrows, recalling Schliemann's owl-faced urns ; of stone ' idols,' practically identical with those of Troy and the Ægean islands, here too associated with marble cups of the same simple forms ; of triangular daggers of copper and bronze, and of bronze swords which seem to stand in a filial relation to an ' Amorgan ' type of dagger. In a former communication to this Section I ventured to see in the so-called ' Cabiri ' of Malta—very far removed from any Phœnician sculpture—an intermediate link between the Iberian group and that of the Ægean, and to trace on the fern-like ornaments of the altar-stone a comparison with the naturalistic motives of proto-Mycenæan art, as seen, for instance, on the early vases of Thera and Therasia.

A Chaldæan influence cannot certainly be excluded from this early Ægean art. It reveals itself, for instance, in indigenous imitations of Babylonian cylinders. My own conclusion that the small marble figures of the Ægean deposits, though of indigenous European lineage, were in their more developed types influenced by Istar models from the East, has since been independently arrived at by the Danish archæologist, Dr. Blinkenburg, in his study on præ-Mycenæan art.

More especially the returning-spiral decoration, which in the 'Amorgan Period' appears upon seals, rings, bowls, and caskets of steatite, leads us to a very interesting field of comparison. This motive, destined to play such an important part in the history of European ornament, is absent from the earlier products of the great Anatolo-Danubian province. As a European design it is first found on these insular fabrics, and it is important to observe that it first shows itself in the form of reliefs on stone. The generally accepted idea, put forward by Dr. Milchhöter, that it originated here from applied spirals on metal work is thus seen to be bereft of historical justification. At a somewhat later date we find this spiraliform motive communicating itself to the ceramic products of the Danubian region, though from the bold relief in which it sometimes appears, a reminiscence of the earlier steatite reliefs seems still traceable. In the late Neolithic pile-station of Butmir, in Bosnia, this spiral decoration appears in great perfection on the pottery, and is here associated with clay images of very advanced fabric. At Lengyel, in Hungary, and elsewhere, we see it applied to primitive painted pottery. Finally, in the later Hungarian Bronze Age it is transferred to metal work.

But this connexion—every link of which can be made out—of the lower Danubian Bronze Age decoration with the Ægean spiral system—itself much earlier in origin—has a very important bearing on the history of ornament in the North and West. The close relation of the Bronze Age culture of Scandinavia and North-western Germany with that of Hungary is clearly established, and of the many valuable contributions made by Dr. Montelius to prehistoric archæology, none is more brilliant than his demonstration that this parallelism of culture between the North-west and South-east owes its origin to the most ancient course of the amber trade from the North Sea shores of Jutland by the valley of the Elbe and Moldau to the Danubian Basin. As Dr. Montelius has also shown, there was, besides, a western extension of this trade to our own islands. If Scandinavia and its borderlands were the source of amber, Ireland was the land of gold. The wealth of the precious metal there is illustrated by the fact that, even as late as 1796, the gold washings of County Wicklow amounted to 10,000l. A variety of evidence shows a direct connexion between Great Britain and Scandinavia from the end of the Stone Age onwards. Gold diadems of unquestionably British—probably Irish—fabric have been found in Seeland and Funen, and from the analysis of early gold ornaments it clearly results that it was from Ireland rather than the Ural that Northern and Central Europe was supplied. Mr. Coffey, who has made an exhaustive study of the early Irish monuments, has recently illustrated this early connexion by other comparisons, notably the appearance of a design which he identifies with the early carvings of boats on the rocks of Scandinavia.

This prolongation of the Bronze Age trade route—already traced from the Middle Danube—from Scandinavia to Ireland, ought it to be regarded as the historic clue to the contemporary appearance of the spiral motive in the British Islands? Is it to this earlier intercourse with the land of the Vikings that we must ascribe the spiral scrolls on the slabs of the great chambered barrows of the Irish Bronze Age—best seen in the most imposing of them all, before the portal and on the inner chambers of New Grange?

The possibility of such a connexion must be admitted, the probability is great that the contemporary appearance of the spiraliform ornament in Ireland and on the Continent of Europe is due to direct derivation. It is, of course, conceivable that such a simple motive as the returning spiral may have originated independently in various parts of Europe, as it did originate in other parts of the world. But anthropology has ceased to content itself with the mere accumulation of sporadic coincidences. It has become a historic study. It is not sufficient to know how

such and such phenomena *may* have originated, but how, as a matter of fact, they *did*. Hence in the investigation of origins and evolution the special value of the European field where the evidence has been more perfectly correlated and the continuous records go further back. An isolated example of the simple volute design belonging to the 'Reindeer Period' has been found in the grotto of Arudy. But the earliest cultural strata of Europe, from the beginning of the Neolithic period onwards, betray an entire absence of the returning spiral motive. When we find it later propagating itself as a definite ornamental system in a regular chronological succession throughout an otherwise inter-related European zone, we have every right to trace it to a common source.

But it does not therefore follow that the only alternative is to believe that the spiral decoration of the Irish monuments necessarily connects itself with the ancient stream of intercourse flowing from Scandinavia.

We have to remember that the Western lands of gold and tin were the goals of other prehistoric routes. Especially must we bear in mind the early evidence of intercourse between the British Isles and the old Iberic region of the opposite shores of the Continent. The derivation of certain forms of Bronze Age types in Britain and Ireland from this side has already been demonstrated by my father, and British or Irish bronze flat axes with their characteristic ornamentation have in their turn been found in Spain as well as in Denmark. The peculiar technique of certain Irish flint arrowheads of the same period, in which chipping and grinding are combined, is also characteristic of the Iberian province, and seems to lead to very extended comparisons on the Libyan side, recurring as it does in the exquisite handiwork of the non-Egyptian race whose relics Mr. Petrie has brought to light at Nagada. In prehistoric Spanish deposits, again, are found the actual wallet-like baskets with in-curving sides, the prototypes of a class of clay food-vessels which (together with a much wider distribution) are of specially frequent occurrence in the British Isles as well as the old Iberian area.

If the spiral decoration had been also a feature of the Scandinavian rock carvings, the argument for derivation from that side would have been strong. But they are not found in them, and, on the other hand, the sculptures on the dolmens of the Morbihan equally show certain features common to the Irish stone chambers, including the primitive ship figure. The spiral itself does not appear in these, but the more the common elements between the Megalithic piles, not only of the old Iberian tract on the mainland, including Brittany, but in the islands of the West Mediterranean basin, are realised, the more probable it becomes that the impulse came from this side. The prehistoric buildings of Malta, hitherto spoken of as 'Phœnician temples,' which show in their primitive conception a great affinity to the Megalithic chambers of the earliest British barrows, bear witness on this side to the extension of the Ægean spiral system in a somewhat advanced stage, and accompanied, as at New Grange, with intermediate lozenges. In Sardinia, as I hope to show, there is evidence of the former existence of monuments of Mycenæan architecture in which the chevron, the lozenge, and the spiral might have been seen associated as in Ireland. It is on this line, rather than on the Danube and the Elbe, that we find in a continuous zone that Cyclopean tradition of domed chambers which is equally illustrated at Mycenæ and at New Grange.

These are not more than indications, but they gain additional force from the converging evidence to which attention has already been called of an ancient line of intercourse, mainly, we may believe, connected with the tin trade between the East Mediterranean basin and the Iberian West. A further corroboration of the view that an Ægean impulse propagated itself as far as our own islands from that side is perhaps afforded by a very remarkable find in a British barrow.

I refer to the Bronze Age interment excavated by Canon Greenwell on Folkton Wold, in Yorkshire, in which, beside the body of a child, were found three carved chalk objects resembling round boxes with bossed lids. On one of these lids were grouped together, with a lozenge-shaped space between them, two partly spurali-form partly concentric circular ornaments, which exhibit before our eyes the degeneration of two pairs of returning spiral ornaments. Upon the sides of two of these chalk caskets, associated with chevrons, saltires, and lozenges, were rude

indications of faces—eyes and nose of bird-like character—curiously recalling the early Ægean and Trojan types of Dr. Schliemann. These, as M. Reinach has. pointed out, also find an almost exact parallel in the rude indications of the human face seen on the sculptured menhirs of the Marne and the Gard valleys. To this may be added the interesting comparisons supplied by certain clay vessels. of rounded form, somewhat resembling the chalk 'caskets' discovered by MM. Siret in Spanish interments of the early metal age, in which eyes and eyebrows of a primitive style are inserted, as on the British relics, in the inter-spaces of linear ornamentation. The third chalk disc exhibits, in place of the human face, a butterfly with volute antennæ, reminding us of the appearance of butterflies as a decorative motive on the gold roundels from the shaft-graves of Mycenæ, as also on early Mycenæan gems of steatite from Crete; in the latter case with the feelers curving outwards in the same way. The stellate design with central circles on the lid of one of the chalk caskets is itself not impossibly a distant degeneration of the star-flowers on the same Mycenæan plates. Putting all these separate elements of resemblance together—the returning spiral and star, the rude face and butterfly—the suggestion of Ægean reminiscence becomes strong, but the other parallels lead us for the line of its transmission towards the Iberian rather than the Scandinavian route.[1]

So much, at least, results from these various comparisons that, whether we find the spiral motive in the extreme West or North of Europe, everything points to the Ægean world as its first European centre. But have we any right to regard it, even there, as of indigenous evolution?

It had been long my own conviction that the Ægean spiral system must itself be regarded as an offshoot of that of ancient Egypt, which as a decorative motive on scarabs goes back, as Professor Petrie has shown, to the Fourth Dynasty. During the time of the Twelfth Dynasty, which, on general grounds, may be supposed roughly to correspond with the 'Amorgan Period' of Ægean culture, it attained its apogee. The spiral convolutions now often cover the whole field of the scarab, and the motive begins to spread to a class of black bucchero vases the chalk inlaying of whose ornaments suggests widespread European analogies. But the important feature to observe is that here, as in the case of the early Ægean examples, the original material on which the spiral ornament appears is stone, and that, so far from being derived from an advanced type of metal work, it goes back in Egypt to a time when metal was hardly known.

The prevalence of the spiral ornamentation on stone work in the Ægean islands and contemporary Egypt, was it merely to be regarded as a coincidence? To turn one's eyes to the Nile Valley, was it simply another instance of the '*Mirage Orientale*'? For my own part, I ventured to believe that, as in the case of Northern Europe, the spread of this system was connected with many collateral symptoms of commercial inter-connexion, so here, too, the appearance of this early Ægean ornament would be found to lead to the demonstration of a direct intercourse between the Greek islands and Egypt at least a thousand years earlier than any that had hitherto been allowed.

One's thoughts naturally turned to Crete, the central island, with one face on the Libyan Sea—the natural source and seminary of Ægean culture—where fresh light was already being thrown on the Mycenæan civilisation by the researches of Professor Halbherr, but the earlier prehistoric remains of which were still unexplored. Nor were these expectations unfounded. As the result of three expeditions—undertaken in three successive years, from the last of which I returned three months since—it has been my fortune to collect a series of evidences of a very early and intimate contact with Egypt, going back at least to the Twelfth

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[1] A further piece of evidence pointing in this direction is supplied by one of the chalk 'caskets.' On the upper disc of this, in the place corresponding with the double-spirals on the other example, appears a degeneration of the same motive in a more compressed form, resembling two sets of concentric horseshoes united at their bases. This recurs at New Grange, and single sets of concentric horseshoes, or semicircles, are found both there and at Gavrinnis The degeneration of the returning spiral motive extends therefore to Brittany.

Dynasty, and to the earlier half of the third millennium before our era. It is not only that in primitive deposits, like that of Hagios Onuphrios, scarabs, acknowledged by competent archæologists to be of Twelfth Dynasty date, occurred in association with steatite seals presenting the Ægean spiral ornamentation, and with early pottery answering to that of Amorgos and the second city of Troy. This by itself might be regarded by many as convincing. But,—what from the point of view of intercourse and chronology is even more important,—in the same deposit and elsewhere occurred early button-shaped and triangular seals of steatite with undoubted indigenous copies of Egyptian lotos designs characteristic of the same period, while in the case of the three-sided bead-seals it was possible to trace a regular evolution leading down to Mycenæan times. Nor was this all. Throughout the whole of the island there came to light a great variety of primitive stone vases, mostly of steatite, a large proportion of which reproduced the characteristic forms of Egyptian stone vases, in harder materials, going far back into the Ancient Empire. The returning spiral motive is also associated with these, as may be seen from a specimen now in the collection of Dr. Naue, of Munich.

A geological phenomenon which I was able to ascertain in the course of my recent exploration of the eastern part of the island goes far to explain the great importance which these steatite or 'soapstone' fabrics played in the primitive culture of Crete and the Ægean islands. In the valley of the Sarakina stream I came upon vast deposits of this material, the diffusion of which could be further traced along a considerable tract of the southern coast. The abundant presence of this attractive and, at the same time, easily workable stone—then incomparably more valuable, owing to the imperfection of the potter's art—goes far to explain the extent to which these ancient Egyptian forms were imitated, and the consequent spread of the returning spiral motive throughout the Ægean.

In the matter of the spiral motive, Crete may thus be said to be the missing link between prehistoric Ireland and Scandinavia and the Egypt of the Ancient Empire. But the early remains of the island illustrate in many other ways the comparatively high level of culture already reached by the Ægean population in præ-Mycenæan times. Especially are they valuable in supplying the antecedent stages to many characteristic elements of the succeeding Mycenæan civilisation.

This ancestral relationship is nowhere more clearly traceable than in a class of relics which bear out the ancient claim of the islanders that they themselves had invented a system of writing to which the Phœnicians did not do more than add the finishing touches. Already, at the Oxford meeting of the Association, I was able to call attention to the evidence of the existence of a prehistoric Cretan script evolved by gradual simplification and selection from an earlier picture writing. This earlier stage is, roughly speaking, illustrated by a series of primitive seals belonging to the 'Period of Amorgos.' In the succeeding Mycenæan age the script is more conventionalised, often linear, and though developments of the earlier forms of seals are frequently found, they are usually of harder materials, and the system is applied to other objects. As the result of my most recent investigations, I am now able to announce the discovery of an inscribed prehistoric relic, which surpasses in interest and importance all hitherto known objects of this class. It consists of a fragment of what may be described as a steatite 'Table of Offerings,' bearing part of what appears to be a dedication of nine letters of probably syllabic values, answering to the same early Cretan script that is seen on the seals, and with two punctuations. It was obtained from the lowest level of a Mycenæan stratum, containing numerous votive objects, in the great cave of Mount Dikta, which, according to the Greek legend, was the birthplace of Zeus.

This early Cretan script, which precedes by centuries the most ancient records of Phœnician writing, and supplies, at any rate, very close analogies to what may be supposed to have been the pictorial prototypes of several of the Phœnician etters, stands in a direct relation to the syllabic characters used at a later date by the Greeks of Cyprus. The great step in the history of writing implied by the evolution of symbols of phonetic value from primitive pictographs is thus shown to have effected itself on European soil.

In many other ways the culture of Mycenæ—that extraordinar revelation from

the soil of prehistoric Greece—can be shown to be rooted in this earlier Ægean stratum. The spiral system, still seen in much of its pure original form on the gold vessels and ornaments from the earlier shaft-graves of Mycenæ, is simply the translation into metal of the pre-existing steatite decoration.[1]

The Mycenæan repoussé work in its most developed stage as applied to human and animal subjects has probably the same origin in stone work. Cretan examples, indeed, give the actual transition in which an intaglio in dark steatite is coated with a thin gold plate impressed into the design. On the other hand, the noblest of all creations of the Mycenæan goldsmith's art, the Vaphio cups, with their bold reliefs, illustrating the hunting and capture of wild bulls, find their nearest analogy in a fragment of a cup, procured by me from Knôsos, of black Cretan steatite, with naturalistic reliefs, exhibiting a fig-tree in a sacred enclosure, an altar, and men in high action, which in all probability was originally coated, like the intaglio, with thin plates of gold.

In view of some still prevalent theories as to the origin of Mycenæan art, it is important to bear in mind these analogies and connexions, which show how deeply set its roots are in Ægean soil. The Vaphio cups, especially, from their superior art, have been widely regarded as of exotic fabric. That the art of an European population in prehistoric times should have risen above that of contemporary Egypt and Babylonia was something beyond the comprehension of the traditional school These most characteristic products of indigenous skill, with their spirited representations of a sport the traditional home of which in later times was the Thessalian plains, have been, therefore, brought from 'Northern Syria'! Yet a whole series of Mycenæan gems exists executed in the same bold naturalistic style, and of local materials, such as lapis Lacedæmonius, the subjects of which are drawn from the same artistic cycle as those of the cups, and not one of these has as yet been found on the Eastern Mediterranean shores. Like the other kindred intaglios, they all come from the Peloponnese, from Crete, from the shores and islands of the Ægean, from the area, that is, where their materials were procured. Their lentoid and almond-shaped forms are altogether foreign to Semitic usage, which clung to the cylinder and cone. The finer products of the Mycenæan glyptic art on harder materials were, in fact, the outcome of long apprentice studies of the earlier Ægean population. of which we have now the record in the primitive Cretan seals, and the explanation in the vast beds of such an easily worked material as steatite.

But the importation of the most characteristic Mycenæan products from 'Northern Syria' has become quite a moderate proposition beside that which we have now before us. In a recent communication to the French Academy of Inscriptions, Dr. Helbig has re-introduced to us a more familiar figure. Driven from his prehistoric haunts on the Atlantic coasts, torn from the Cassiterides, dislodged even from his Thucididean plantations in pre-Hellenic Sicily, the Phœnician has returned, tricked out as the true 'Mycenæan.'

A great part of Dr. Helbig's argument has been answered by anticipation. Regardless of the existence of a regular succession of intermediate glyptic types, such as the 'Melian' gems and the engraved seals of the geometrical deposits of the Greek mainland, like those of Olympia and of the Heræon at Argos, which link the Mycenæan with the classical series, Dr. Helbig takes a verse of Homer to hang from it a theory that seals and engraved stones were unknown to the early Greeks. On this imaginary fact he builds the astounding statement that the engraved gems and seals found with Mycenæan remains must be of foreign and, as he believes, Phœnician importation. The stray diffusion of one or two examples of Mycenæan pots to the coast of Palestine, the partial resemblance of some Hittite bronze figures, executed in a more barbarous Syrian style, to specimens of quite different fabric found at Tiryns, Mycenæ, and, it may be added, in a Cretan cave near Sybrita, the wholly unwarranted attribution to Phœnicia of a bronze vase-handle found in Cyprus, exhibiting the typical lion-headed demons of the Mycenæans—these are only a few salient examples of the

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[1] See *Hellenic Journal*, xii. (1892, p. 221.

reasoning by which the whole prehistoric civilisation of the Greek world, so instinct with naturalism and individuality, is handed over to the least original member of the Semitic race. The absence in historic Greece of such arts as that of *intarsia* in metal work, of glass-making (if true) and of porcelain-making, is used as a conclusive argument against their practice by an Ægean population, of uncertain stock, a thousand years earlier, as if in the intervening dark ages between the primitive civilisation of the Greek lands and the Classical Renaissance no arts could have been lost!

Finally, the merchants of Keftô depicted on the Egyptian monuments are once more claimed as Phœnicians, and with them—though this is by no means a necessary conclusion, even from the premise—the precious gifts they bear, including vases of characteristic Mycenæan form and ornament. All this is diametrically opposed to the conclusions of the most careful inquirer into the origins of this mysterious people, Dr W. Max Muller (to be distinguished from the eminent Professor), who shows that the list of countries in which Keftô occurs places them beyond the limit of Phœnicia or of any Semitic country, and connects them rather with Cilicia and with Cyprus, the scene, as we now know, of important Mycenæan plantations. It is certain that not only do the Keftiu traders bear articles of Mycenæan fabric, but their costume, which is wholly un-Semitic, their leggings and sandals, and the long double locks of hair streaming down below their armpits, identify them with the men of the frescoes of Mycenæ, and of the Vaphio and Knôsian cups.

The truth is that these Syrian and Phœnician theories are largely to be traced to the inability to understand the extent to which the primitive inhabitants of the Ægean shores had been able to assimilate exotic arts without losing their own individuality. The precocious offspring of our Continent, first come to man's estate in the Ægean island world, had acquired cosmopolitan tastes, and already stretched forth his hands to pluck the fruit of knowledge from Oriental boughs. He had adopted foreign fashions of dress and ornament. His artists revelled in lion-hunts and palm-trees. His very worship was infected by the creations of foreign religions.

The great extent to which the Mycenæans had assimilated exotic arts and ideas can only be understood when it is realised that this adaptive process had begun at least a thousand years before, in the earlier period of Ægean culture. New impulses from Egypt and Chaldæa now succeed the old. The connexion with Eighteenth and Nineteenth Dynasty Egypt was of so intimate a kind that it can only be explained by actual settlement from the Ægean side. The abundant relics of Ægean ceramic manufactures found by Professor Petrie on Egyptian sites fully bear out this presumption. The early marks on potsherds discovered by that explorer seem to carry the connexion back to the earlier Ægean period, but the painted pottery belongs to what may broadly be described as Mycenæan times. The earliest relics of this kind found in the rubbish heaps of Kahun, though it can hardly be admitted that they go quite so far back as the Twelfth Dynasty date assigned to them by Mr Petrie (c. 2500 B.C.), yet correspond with the earliest Mycenæan classes found at Thera and Tiryns, and seem to find their nearest parallels in pottery of the same character from the cave of Kamâres on the northern steep of the Cretan Ida, recently described by Mr. J. L. Myres and by Dr. Lucio Mariani. Vases of the more typical Mycenæan class have been found by Mr. Petrie in a series of deposits dated, from the associated Egyptian relics, from the reign of Thothmes III. onwards (1450 B C.). There is nothing Phœnician about these— with their seaweeds and marine creatures they are the true products of the island world of Greece. The counterpart to these Mycenæan imports in Egypt is seen in the purely Egyptian designs which now invade the northern shores of the Ægean, such as the ceiling of the sepulchral chamber at Orchomenos, or the wall-paintings of the palace at Tiryns—almost exact copies of the ceilings of the Theban tombs—designs distinguished by the later Egyptian combination of the spiral and plant ornament which at this period supersedes the pure returning spiral of the earlier dynasties. The same contemporary evidence of date is seen in the scarabs and porcelain fragments with the cartouches of Queen Tyi and Amenhotep III.,

found in the Mycenæan deposits. But more than a mere commercial connexion between the Ægean seat of Mycenæan culture and Egypt seems to be indicated by some of the inlaid daggers from the Acropolis tombs. The subject of that representing the ichneumons hunting ducks amidst the lotos thickets beside a stream that can only be the Nile, as much as the intarsia technique, is so purely of Egypt that it can only have been executed by a Mycenæan artificer resident within its borders. The whole cycle of Egyptian Nile-pieces thoroughly penetrated Mycenæan art,—the duck-catcher in his Nile-boat, the water-fowl and butterflies among the river plants, the spotted cows and calves, supplied fertile motives for the Mycenæan goldsmiths and ceramic artists. The griffins of Mycenæ reproduce an elegant creation of the New Empire, in which an influence from the Asiatic side is also traceable.

The assimilation of Babylonian elements was equally extensive. It, too, as we have seen, had begun in the earlier Ægean period, and the religious influence from the Semitic side, of which traces are already seen in the assimilation of the more primitive ' idols ' to Eastern models, now forms a singular blend with the Egyptian, as regards, at least, the externals of cult. We see priests, in long folding robes of Asiatic cut, leading griffins, offering doves, holding axes of a type of Egyptian derivation which seems to have been common to the Syrian coast, the Hittite regions of Anatolia, and Mycenæan Greece. Female votaries in flounced Babylonian dresses stand before seated Goddesses, rays suggesting those of Shamas shoot from a Sun-God's shoulders, conjoined figures of moon and star recall the symbols of Sin and Istar, and the worship of a divine pair of male and female divinities is widely traceable, reproducing the relations of a Semitic Bel and Beltis. The cylinder subjects of Chaldæan art continually assert themselves: A Mycenæan hero steps into the place of Gilgames or Eabani, and renews their struggles with wild beasts and demons in the same conventional attitudes, of which Christian art has preserved a reminiscence in its early type of Daniel in the lions' den. The peculiar schemes resulting from, or, at least, brought into continual prominence by the special conditions of cylinder engraving, with the constant tendency to which it is liable of the two ends of the design to overlap, deeply influenced the glyptic style of Mycenæ. Here, too, we see the same animals with crossed bodies, with two bodies and a single head, or simply confronted These latter affiliations to Babylonian prototypes have a very important bearing on many later offshoots of European culture. The tradition of these heraldic groups preserved by the later Mycenæan art, and communicated by it to the so-called ' Oriental ' style of Greece, finds in another direction its unbroken continuity in ornamental products of the Hallstatt province, and that of the late Celtic metal workers.

' But this,' exclaims a friendly critic, ' is the old heresy—the " *Mirage Orientale* " over again. Such heraldic combinations have originated independently elsewhere :—why may they not be of indigenous origin in primitive Europe ? '

They certainly may be. Confronted figures occur already in the Dordogne caves. But, in a variety of instances, the historic and geographical connexion of these types with the Mycenæan, and those in turn with the Oriental, is clearly made out. That system which leaves the least call on human efforts at inventiveness seems in anthropology to be the safest.

Let us then fully acknowledge the indebtedness of early Ægean culture to the older civilisations of the East. But this indebtedness must not be allowed to obscure the fact that what was borrowed was also assimilated. On the easternmost coast of the Mediterranean, as in Egypt, it is not in a pauper's guise that the Mycenæan element makes its appearance. It is rather the invasion of a conquering and superior culture. It has already outstripped its instructors. In Cyprus, which had lagged behind the Ægean peoples in the race of progress, the Mycenæan relics make their appearance as imported objects of far superior fabric, side by side with the rude insular products. The final engrafting on Cypriote soil of what may be called a colonial plantation of Mycenæ later reacts on Assyrian art, and justifies the bold theory of Professor Brunn that the sculptures of Nineveh betray Greek handiwork. The concordant Hebrew tradition that the Philistines were immigrants from the Islands of the Sea, the name ' Cherethim,' or Cretans, actually

applied to them, and the religious ties which attached 'Minoan' Gaza to the cult of the Cretan Zeus, are so many indications that the Ægean settlements, which in all probability existed in the Delta, extended to the neighbouring coast of Canaan, and that amongst other towns the great staple of the Red Sea trade had passed into the hands of these prehistoric Vikings. The influence of the Mycenæans on the later Phœnicians is abundantly illustrated in their eclectic art. The Cretan evidence tends to show that even the origins of their alphabet receive illustration from the earlier Ægean pictography. It is not the Mycenæans who are Phœnicians. It is the Phœnicians who, in many respects, acted as the depositaries of decadent Mycenæan art.

If there is one thing more characteristic than another of Phœnician art, it is its borrowed nature, and its incongruous collocation of foreign elements. Dr. Helbig himself admits that if Mycenæan art is to be regarded as the older Phœnician, the Phœnician historically known to us must have changed his nature. What the Mycenæans took they made their own. They borrowed from the designs of Babylonian cylinders, but they adapted them to gems and seals of their own fashion, and rejected the cylinders themselves. The influence of Oriental religious types is traceable on their signet rings, but the liveliness of treatment and the dramatic action introduced into the groups separate them, *toto cœlo*, from the conventional schematism of Babylonian cult-scenes. The older element, the sacred trees and pillars which appear as the background of these scenes—on this I hope to say more later on in this Section—there is no reason to regard here as Semitic. It belongs to a religious stage widely represented on primitive European soil, and nowhere more persistent than in the West.

Mycenæan culture was permeated by Oriental elements, but never subdued by them. This independent quality would alone be sufficient to fix its original birthplace in an area removed from immediate contiguity with that of the older civilisations of Egypt and Babylonia. The Ægean island world answers admirably to the conditions of the case. It is near, yet sufficiently removed, combining maritime access with insular security. We see the difference if we compare the civilisation of the Hittites of Anatolia and Northern Syria, in some respects so closely parallel with that of Mycenæ. The native elements were there cramped and trammelled from the beginning by the Oriental contact. No real life and freedom of expression was ever reached; the art is stiff, conventional, becoming more and more Asiatic, till finally crushed out by Assyrian conquest. It is the same with the Phœnicians. But in prehistoric Greece the indigenous element was able to hold its own, and to recast what it took from others in an original mould. Throughout its handiwork there breathes the European spirit of individuality and freedom. Professor Petrie's discoveries at Tell-el-Amarna show the contact of this Ægean element for a moment infusing naturalism and life into the time-honoured conventionalities of Egypt itself.

A variety of evidence, moreover, tends to show that during the Mycenæan period the earlier Ægean stock was reinforced by new race elements coming from north and west. The appearance of the primitive fiddle-bow-shaped *fibula* or safety-pin brings Mycenæan Greece into a suggestive relation with the Danube Valley and the Terremare of Northern Italy. Certain ceramic forms show the same affinities; and it may be noted that the peculiar 'two-storied' structure of the 'Villanova' type of urn which characterises the earliest Iron Age deposits of Italy finds already a close counterpart in a vessel from an Akropolis grave at Mycenæ—a parallelism which may point to a common Illyrian source. The painted pottery of the Mycenæans itself, with its polychrome designs, betrays Northern and Western affinities of a very early character, though the glaze and exquisite technique were doubtless elaborated in the Ægean shores. Examples of spiraliform painted designs on pottery going back to the borders of the Neolithic period have been found in Hungary and Bosnia. In the early rock-tombs of Sicily of the period anterior to that marked by imported products of the fully developed Mycenæan culture are found unglazed painted wares of considerable brilliancy, and allied classes recur in the heel of Italy and in the cave deposits of Liguria of the period transitional between the use of stone and metal. The 'household gods,'

if so we may call them, of the Mycenæans also break away from the tradition of the marble Ægean forms. We recognise the coming to the fore again of primitive European clay types in a more advanced technique. Here, too, the range of comparison takes us to the same Northern and Western area. Here, too, in Sicily and Liguria, we see the primitive art of ceramic painting already applied to these at the close of the Stone Age. A rude female clay figure found in the Arene Candide cave near Finalmarina, the upper part of the body of which, armless and rounded, is painted with brown stripes on a pale rose ground, seems to me to stand in a closer relation to the prototype of a well-known Mycenæan class than any known example. A small painted image, with punctuated cross-bands over the breast, from a sepulchral grotto at Villafrati, near Palermo, belongs to the same early family as the *bucchero* types of Butmir, in Bosnia. Unquestionable parallels to the Mycenæan class have been found in early graves in Servia, of which an example copied by me some years since in the museum at Belgrade was found near the site of that later emporium of the Balkan trade, Viminacium, together with a cup attesting the survival of the primitive Ægean spirals. These extensive Italian and Illyrian comparisons, which find, perhaps, their converging point in the North-Western corner of the Balkan peninsula, show, at least approximately, the direction from which this new European impulse reached the Ægean shores.

It is an alluring supposition that this North-Western infusion may connect itself with the spread of the Greek race in the Ægean islands and the Southern part of the Balkan peninsula. There seems, at least, to be a reasonable presumption in favour of this view. The Mycenæan tradition, which underlies so much of the classical Greek art, is alone sufficient to show that a Greek element was at least included in the Mycenæan area of culture. Recent criticism has found in the Mycenæan remains the best parallel to much of the early arts and industries recorded by the Homeric poems. The *megaron* of the palaces at Tiryns and Mycenæ is the hall of Odysseus; the inlaid metal work of the shield of Achilles recalls the Egypto-Mycenæan intarsia of the dagger blades; the cup of Nestor with the feeding doves, the subjects of the ornamental design—the siege-piece, the lion-hunt, the hound with its quivering quarry—all find their parallels in the works of the Mycenæan goldsmiths. The brilliant researches of Dr. Reichel may be said to have resulted in the definite identification of the Homeric body-shield with the most typical Mycenæan form, and have found in the same source the true explanation of the greaves and other arms and accoutrements of the epic heroes.

That a Greek population shared in the civilisation of Mycenæ cannot reasonably be denied, but that is far from saying that this was necessarily the only element, or even the dominant element. Archæological comparisons, the evidence of geographical names and consistent tradition, tend to show that a kindred race, represented later by the Phrygians on the Anatolian side, the race of Pelops and Tantalos, the special votaries of Kybelê, played a leading part. In Crete a non-Hellenic element, the Eteocretes, or 'true Cretans,' the race of Minôs, whose name is bound up with the earliest sea-empire of the Ægean and perhaps identical with that of the Minyans of continental Greece, preserved their own language and nationality to the borders of the classical period. The Labyrinth itself, the double-headed axe as a symbol of the divinity called Zeus by the Greek settlers, the common forms in the characters of the indigenous script, local names and historical traditions, further connect these Mycenæan aborigines of Crete with the primitive population, it, too, of European extraction, in Caria and Pisidia, and with the older elements in Lycia.

It is difficult to exaggerate the part played in this widely ramifying Mycenæan culture on later European arts from prehistoric times onwards. Beyond the limits of its original seats, primitive Greece and its islands, and the colonial plantations thrown out by it to the west coast of Asia Minor to Cyprus, and in all probability to Egypt and the Syrian coast, we can trace the direct diffusion of Mycenæan products, notably the ceramic wares, across the Danube to Transylvania and Moldavia. In the early cemeteries of the Caucasus the fibulas and other objects indicate a late Mycenæan source, though they are here blended with allied elements of a more Danubian character. The Mycenæan impress is very strong in Southern

Italy, and, to take a single instance, the prevailing sword-type of that region is of Mycenæan origin. Along the western Adriatic coast the same influence is traceable to a very late date in the sepulchral stelæ of Pesaro and the tympanum relief of Bologna, and bronze knives of the prehistoric Greek type find their way into the later Terremare. At Orvieto and elsewhere have even been discovered Mycenæan lentoid gems. In Sicily the remarkable excavations of Professor Orsi have brought to light a whole series of Mycenæan relics in the beehive rock-tombs of the south-eastern coast, associated with the later class of Sikel fabrics

Sardinia, whose name has with great probability been connected with the Shardanas, who, with the Libyan and Ægean races, appear as the early invaders of Egypt, has already produced a Mycenæan gold ornament. An unregarded fact points further to the probability that it formed an important outpost of Mycenæan culture. In 1853 General Lamarmora first printed a MS. account of Sardinian antiquities, written in the latter years of the fifteenth century by a certain Gilj, and accompanied by drawings made in 1497 by Johan Virde, of Sassari. Amongst these latter (which include, it must be said, some gross falsifications) is a capital and part of a shaft of a Mycenæan column in a style approaching that of the façade of the 'Treasury of Atreus.' It seems to have been found at a place near the Sardinian Olbia, and Virde has attached to it the almost prophetic description, 'columna Pelasgica.' That it is not a fabrication due to some traveller from Greece is shown by a curious detail. Between the chevrons that adorn it are seen rows of eight-rayed stars, a detail unknown to the Mycenæan architectural decoration till it occurred on the painted base of the hearth in the *megaron* of the palace at Mycenæ excavated by the Greek Archæological Society in 1886. In this neglected record, then, we have an indication of the former existence in Sardinia of Mycenæan monuments, perhaps of palaces and royal tombs comparable to those of Mycenæ itself.

More isolated Mycenæan relics have been found still further afield, in Spain, and even the Auvergne, where Dr. Montelius has recognised an evidence of an old trade connexion between the Rhone valley and the Eastern Mediterranean, in the occurrence of two bronze double axes of Ægean form. It is impossible to do more than indicate the influence exercised by the Mycenæan arts on those of the early Iron Age Here it may be enough to cite the late Mycenæan parallels afforded by the Ægina Treasure to the open-work groups of bird-holding figures and the pendant ornaments of a whole series of characteristic ornaments of the Italo-Hallstatt culture.

In this connexion, what may be called a sub-Mycenæan survival in the North-Western corner of the Balkan peninsula has a special interest for the Celtic West. Among the relics obtained by the fruitful excavations conducted by the Austrian archæologists in Bosnia and Herzegovina, and notably in the great prehistoric cemetery of Glasinatz, a whole series of Early Iron Age types betray distinct Mycenæan affinities. The spiral motive and its degeneration—the concentric circles grouped together with or without tangential lines of connexion—appears on bronze torques, on fibulæ of Mycenæan descent, and the typical finger-rings with the besil at right angles to the ring. On the plates of other 'spectacle fibulæ' are seen triquetral scrolls singularly recalling the gold plates of the Akropolis graves of Mycenæ. These, as well as other parallel survivals of the spiral system in the Late Bronze Age of the neighbouring Hungarian region, I have elsewhere [1] ventured to claim as the true source from which the Alpine Celts, together with many Italo-Illyric elements from the old Venetian province at the head of the Adriatic, drew the most salient features of their later style, known on the Continent as that of La Tène. These Mycenæan survivals and Illyrian forms engrafted on the 'Hallstatt' stock were ultimately spread by the conquering Belgic tribes to our own islands, to remain the root element of the Late Celtic style in Britain—where the older spiral system had long since died a natural death—and in Ireland to live on to supply the earliest decorative motives of its Christian art.

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[1] Rhind Lectures, 1895, 'On the Origins of Celtic Art,' summaries of which appeared in the *Scotsman*.

From a Twelfth Dynasty scarab to the book of Durrow or the font of Deerhurst is a far cry. But, as it was said of old, 'Many things may happen in a long time.' We have not to deal with direct transmission *per saltum*, but with gradual propagation through intervening media. This brief survey of 'the Eastern Question in Anthropology' will not have been made in vain if it helps to call attention to the mighty part played by the early Ægean culture as the mediator between primitive Europe and the older civilisations of Egypt and Babylonia. Adequate recognition of the Eastern background of the European origins is not the 'Oriental Mirage.' The independent European element is not affected by its power of assimilation. In the great days of Mycenæ we see it already as the equal, in many ways the superior, of its teachers, victoriously reacting on the older countries from which it had acquired so much. I may perhaps be pardoned if in these remarks, availing myself of personal investigations, I have laid some stress on the part which Crete has played in this first emancipation of the European genius. There far earlier than elsewhere we can trace the vestiges of primæval intercourse with the valley of the Nile. There more clearly than in any other area we can watch the continuous development of the germs which gave birth to the higher Ægean culture. There before the days of Phœnician contact a system of writing had already been worked out which the Semite only carried one step further To Crete the earliest Greek tradition looks back as the home of divinely inspired legislation and the first centre of maritime dominion.

Inhabited since the days of the first Greek settlements by the same race, speaking the same language, and moved by the same independent impulses, Crete stands forth again to-day as the champion of the European spirit against the yoke of Asia.

# 𝔅ritish 𝔄ssociation for the 𝔄dvancement of 𝔖cience.

## LIVERPOOL, 1896.

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# ADDRESS

### TO THE

# PHYSIOLOGICAL SECTION

### BY

## W. H. GASKELL, M.D., LL.D., M.A., F.R.S.

### PRESIDENT OF THE SECTION.

WHEN I received the honour of an invitation to preside at the Physiological Section of the British Association, my thoughts naturally turned to the subject of the Presidential Address, and it seemed to me that the traditions of the British Association, as well as the fact that a Physiological Section was a comparatively new thing, both pointed to the choice of a subject of general biological interest rather than a special physiological topic; and I was the more encouraged to choose such a subject because I look upon the growing separation of physiology from morphology as a serious evil, and detrimental to both scientific subjects. I was further encouraged to do so by the thought that, after all, a large amount of the work done in physiological laboratories is anatomical—either minute anatomy or topographical anatomy, such as the tracing out of the course of nerve-fibre tracts in the central and peripheral nervous system by physiological methods. Such methods require to be supplemented by the morphological method of inquiry. If we can trace up step by step the increasing complexity of the vertebrate central nervous system; if we can unravel its complex nature, and determine the original simpler paths of its conducting fibres, and the original constitution of the special nerve centres, then it is clear that the method of comparative anatomy would be of immense assistance to the study of the physiology of the central nervous system of the higher vertebrates. So also with numbers of other physiological problems, such as, for instance, the question whether all muscular substances are supplied with inhibitory as well as motor nerves; to which is closely allied the question of the nature of the mechanism by which antagonistic muscles work harmoniously together. Such questions receive their explanation in the researches of Biedermann on the nerves of the opening and closing muscles of the claw of the crayfish, as soon as it has been shown that a genetic relationship exists between the nervous system and muscles of the crayfish and those of the vertebrate.

Take another question of great interest in the present day, viz. the function of such ductless glands as the thyroid and the pituitary glands. The explanation of such function must depend upon the original function of these glands, and cannot, therefore, be satisfactory until it has been shown by the study of comparative anatomy how these glands have arisen. The nature of the leucocytes of the blood and lymph spaces, the chemical problems involved in the assigning of cartilage into its proper group of mucin compounds, and a number of other questions of physiological chemistry, will all advance a step nearer solution as soon as we definitely know from what group of invertebrates the vertebrate has arisen.

I have therefore determined to choose as the subject of my address 'The

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Origin of Vertebrates,' feeling sure that the evidence which has appealed to me as a physiologist will be of interest to the Physiological Section; while at the same time, as I have invited also the Sections of Zoology and Anthropology to be present, I request that this address may be considered as opening a discussion on the subject of the origin of vertebrates. I do not desire to speak *ex cathedrâ*, and to suppress discussion, but, on the contrary, I desire to have the matter threshed out to its uttermost limit, so that if I am labouring under a delusion the nature of that delusion may be clearly pointed out to me.

The central pivot on which the whole of my theory turns is the central nervous system, especially the brain region. There is the *ego* of each animal; there is the master-organ, to which all the other parts of the body are subservient. It is to my mind inconceivable to imagine any upward evolution to be associated with a degradation of the brain portion of the nervous system. The striking factor of the ascent within the vertebrate phylum from the lowest fish to man is the steady increase of the size of the central nervous system, especially of the brain region. However much other parts may suffer change or degradation, the brain remains intact, steadily increasing in power and complexity. If we turn to the invertebrate kingdom, we find the same necessary law: when the metamorphosis of an insect takes place, when the larval organs are broken up by a process of histolysis, and new ones formed, the central nervous system remains essentially intact, and the brain of the imago differs from that of the larva only in its increased growth and complexity.

A striking instance of the same necessary law is seen in the case of the transformation of the larval lamprey, or Ammocœtes, into the adult lamprey, or Petromyzon; here also, by a process of histolysis, most of the organs of the head region of the animal undergo dissolution and re-formation, while the brain remains intact, increasing in size by the addition of new elements, without any sign of preliminary dissolution. On the other hand, when, as is the case in the Tunicates, the transformation process is accompanied with a degradation of the central nervous system, we find the adult animal so hopelessly degraded that it is impossible to imagine any upward evolution from such a type.

It is to my mind perfectly clear that, in searching among the Invertebrata for the immediate ancestor of the Vertebrata, the most important condition which such ancestor must fulfil is to possess a central nervous system, the anterior part of which is closely comparable with the brain region of the lowest vertebrate. It is also clear on every principle of evolution that such hypothetical ancestor must resemble the lowest vertebrate much more closely than any of the higher vertebrates, and therefore a complete study of the lowest true vertebrate must give the best chance of discovering the homologous parts of the vertebrate and the invertebrate. For this purpose I have chosen for study the Ammocœtes, or larval form of the lamprey, rather than Amphioxus or the Tunicates, for several reasons.

In the first place, all the different organs and parts of the higher vertebrates can be traced directly into the corresponding parts of Petromyzon, and therefore of Ammocœtes. Thus, every part of the brain and organs of special sense—all the cranial nerves, the cranial skeleton, the muscular system, &c., of the higher vertebrates can all be traced directly into the corresponding parts of the lamprey. So direct a comparison cannot be made in the case of Amphioxus or the Tunicates.

Secondly, Petromyzon, together with its larval form, Ammocœtes, constitutes an ideal animal for the tracing of the vertebrate ancestry, in that in Ammocœtes we have the most favourable condition for such investigations, viz. a prolonged larval stage, followed by a metamorphosis, and the consequent production of the imago or Petromyzon—a transformation which does not, as in the case of the Tunicates, lead to a degenerate condition, but, on the contrary, leads to an animal of a distinctly higher vertebrate type than the Ammocœtes form. As we shall see, the Ammocœtes is so full of invertebrate characteristics that we can compare organ for organ, structure for structure, with the corresponding parts of Limulus and its allies. Then comes that marvellous transformation scene during which, by a process of histolysis, almost all the invertebrate characteristics are destroyed or

changed, and there emerges a higher animal, the Petromyzon, which can now be compared organ for organ, structure for structure, with the larval form of the Amphibian, and so through the medium of these larval forms we can trace upwards without a break the evolution of the vertebrate from the ancient king-crab form. On the other hand, Amphioxus and the Tunicates are distinctly degenerate ; it is easier to look upon either of them as a degenerate Ammocœte than as giving a clue to the ancestor of the Ammocœte. It is to my mind surprising how difficult it appears to be to get rid of preconceived opinions, for one still hears, in the assertion that Petromyzon as well as Amphioxus is degenerate, the echoes of the ancient myth that the Elasmobranchs are the lowest fishes, and the Cyclostomata their degenerated descendants.

The characteristic of the vertebrate central nervous system is its tubular character ; and it is this very fact of its formation as a tube which has led to the disguising of its segmental character, and to the whole difficulty of connecting vertebrates with other groups of animals. The explanation of the tubular character of the central nervous system is the keystone to the whole of my theory of the origin of vertebrates. The explanation which I have given differs from all others, in that I consider the nervous system to be composed of two parts—an internal epithelial tube, surrounded to a greater or less extent by a segmented nervous system ; and I explain the existence of these two parts by the hypothesis that the internal epithelial tube was originally the alimentary canal of an arthropod animal, such as Limulus or Eurypterus, which has become surrounded to a greater or less extent by the nervous system.

Any hypothesis which deals with the origin of one group of animals from another must satisfy three conditions :—

1. It must be in accordance with the phylogenetic history of each group. It must therefore give a consistent explanation of all the organs and tissues of the higher group which can be clearly shown not to have originated within the group itself. At the same time, the variations which have occurred on the hypothesis must be in harmony with the direction of variation in the lower group, if not actually foreshadowed in that group.

This condition may be called the Phylogenetic test.

2. The anatomical relation of parts must be the same in the two groups, not only with respect to coincidence of topographical arrangement, but also with respect to similarity of structure, and, to a large extent, also of function.

This condition may be called the Anatomical test.

3. The peculiarities of the ontogeny or embryological development of the higher group must receive an adequate explanation by means of the hypothesis, while at the same time they must help to illustrate the truth of the hypothesis.

This condition may be called the Ontogenetic test.

I hope to convince you that all these three conditions are satisfied by my hypothesis as far as the head region of the vertebrate is concerned. I speak only of the head region at present, because that is the part which I have especially studied up to the present time, and also because it is natural and convenient to consider the cranial and spinal nerves separately ; and I hope to demonstrate to you that not only the nervous system and alimentary canal of such a group of animals as the Giganostraca—*i.e.* Limulus and its allied forms—is to be found in the head region of Ammocœtes, but also, as must logically follow, that every part of the head region of Ammocœtes has its homologous part in the prosomatic and mesosomatic regions of Limulus and its allies. I hope to convince you that our brain is hollow because it has grown round the old cephalic stomach, that our skeleton arose from the modifications of chitinous ingrowths, that the nerves of the medulla oblongata—*i.e.* the facial, glosso-pharyngeal, and vagus nerves—arose from the mesosomatic nerves to the branchial and opercular appendages of Limulus, while the nerves of the hind brain are derived from the nerves of the prosomatic region of Limulus ; that our cerebral hemispheres are but modifications of the supra-œsophageal ganglia of a scorpion, while our eyes and nose are the direct descendants of its eyes and olfactory organs.

In the first place, I will give you shortly the reasons why the central nervous

FIG. 1.—Comparison of Vertebrate Brain from Mammalia to Ammocœtes.
(Epithelial parts represented by dotted lines.)



MAMMALIA.          REPTILIA.          AMPHIBIA

TELEOST.          AMMOCŒTES.
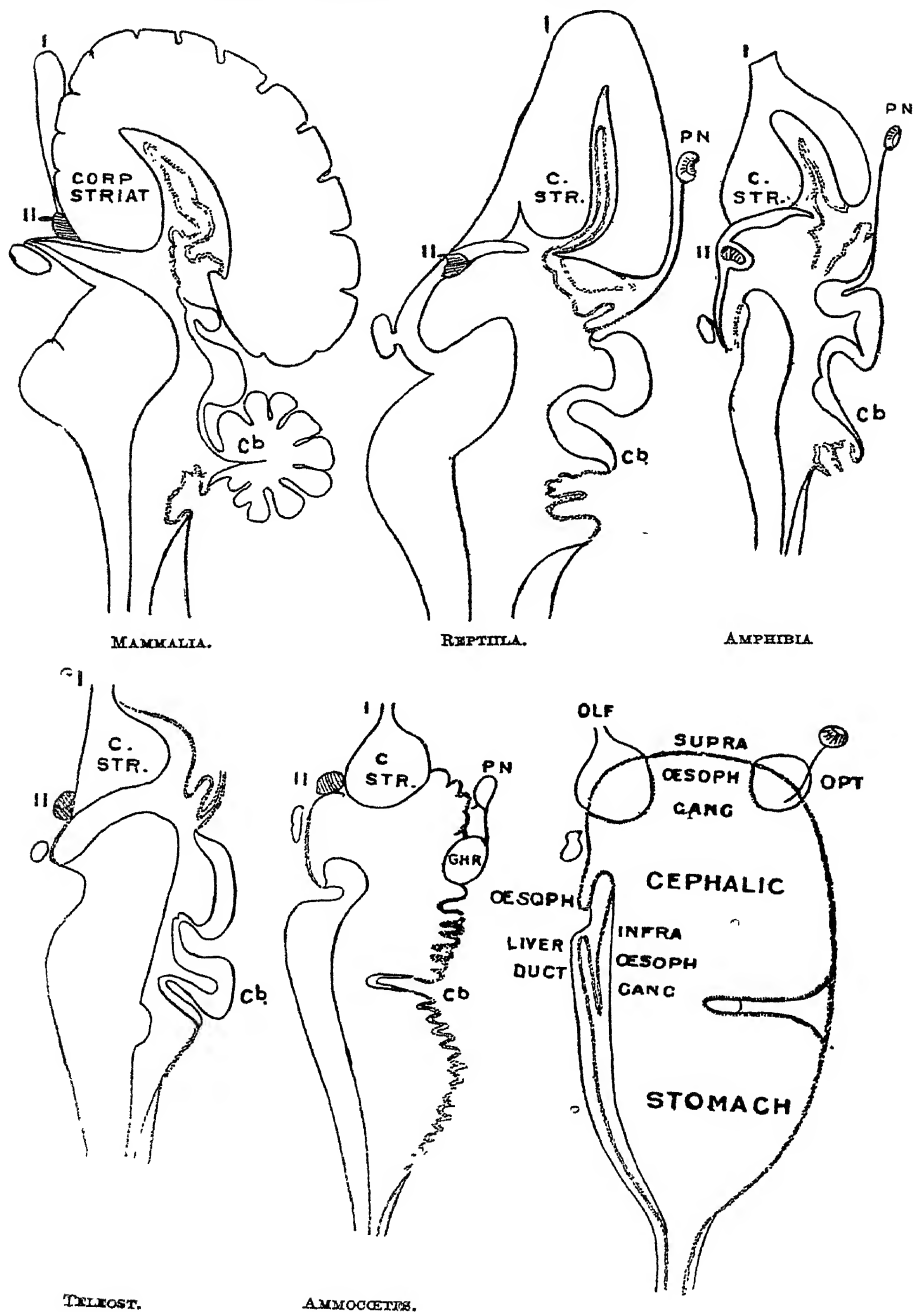
system of the vertebrate must be considered as derived from the conjoined central nervous system and alimentary canal of an arthropod.

*Comparison of the Central Nervous System of Ammocœtes with the Conjoined Central Nervous System and Alimentary Canal of an Arthropod Animal such as Limulus.*

1. *The phylogenetic test* proves that the tube of the central nervous system was originally an epithelial tube, surrounded to a certain extent by nervous material.

The anatomical test then proves that this epithelial tube corresponds in its topographical relations to the nervous material exactly with the alimentary canal of an arthropod in its relations to the central nervous system; and, further, that the topographical relations, structure, and function of the corresponding parts of this nervous material are identical in the Ammocœtes and in the arthropod.

We see from these diagrams, taken from Edinger, how the greater simplicity of the brain region as we descend the vertebrate phylum is attained by the reduction

FIG. 2.—Dorsal and lateral view of the Brain of Ammocœtes.
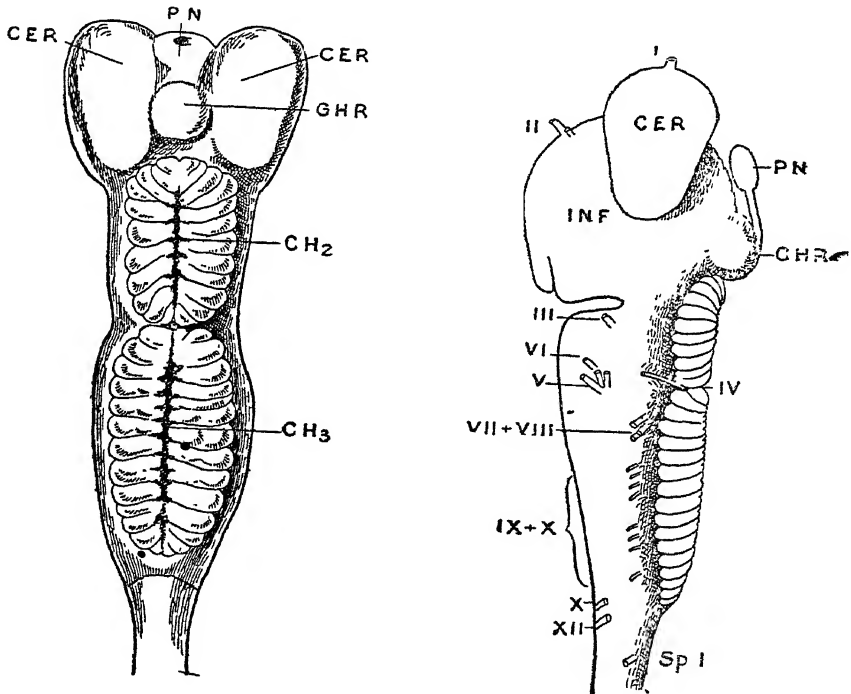
of the nervous material more and more to the ventral side of the central tube, with the result that the dorsal side becomes more and more epithelial, until at last, as is seen in Ammocœtes, the roof of the epichordal portion of the brain consists entirely of fold upon fold of a simple epithelial membrane, interrupted only in one place by the crossing of the IVth nerve and commencement of the cerebellum. In the prechordal part of the brain this simple epithelial portion of the tube is continued on in the middle line as the first choroid plexus of Ahlborn, and the lamina terminalis round to the ventral side; where, again, in the infundibular region, the epithelial saccus vasculosus, which has been becoming more and more

conspicuous in the lower vertebrates, together with the median tube of the infundibulum, testifies to the withdrawal of the nervous material from this part of the brain, as well as from the dorsal region. Further, as already mentioned in my previous papers, the invasion of this epithelial tube by nervous material during the upward development of the vertebrate is beautifully shown by the commencing development of the cerebellar hemispheres in the dogfish; by the dorsal growth of nervous material to form the optic lobes in the Petromyzon; by the occlusion of the ventral part of the tube in the epichordal region to form the raphé, as seen in its commencement in Ammocœtes. Finally, evidence of another kind in favour of the tubular formation being due to an original non-nervous epithelial tube is given by the frequent occurrence of cystic tumours, and also by the formation of the sinus rhomboidalis in birds.

The phylogenetic history of the brain of vertebrates, in fact, is in complete harmony with the theory that the tubular nervous system of the vertebrate originally consisted of two parts—viz. an epithelial tube and a nervous system outside that tube, which has grown over it more and more, and gives not only no support whatever, but is in direct opposition, to the view that the whole tube was originally nervous, and that the epithelial portions, such as the choroid plexuses and roof of the fourth ventricle, are thinned-down portions of that nerve tube. Passing now to

2. *The anatomical test*, we see immediately why this epithelial tube comes out so much more prominently in the lowest vertebrates, for, as can be seen from the diagrams, and is more fully pointed out in my previous papers,[1] every part of the central tube of the vertebrate nervous system corresponds absolutely, both in position and structure, with the corresponding part of the alimentary canal of the arthropod, and the nervous material which is arranged round this epithelial tube is identically the same in topographical position, in structure, and in function as the corresponding parts of the central nervous system of an arthropod.

Especially noteworthy is it to find that the pineal eye (PN), with its large optic ganglion, the ganglion habenulæ (GHR), falls into its right and appropriate place as the right median eye of such an animal as Limulus or Eurypterus. In the following table I will shortly group together the evidence of the anatomical test.

### A. *Coincidence of Topographical Position.*

| LIMULUS AND ITS ALLIES. | AMMOCŒTES AND VERTEBRATES. |
|---|---|
| *Alimentary Canal*:— | |
| 1 Cephalic stomach. | Ventricles of the brain. |
| 2. Straight intestine, ending in anus. | Spinal canal, ending by means of the neurenteric canal in the anus |
| 3. Œsophageal tube. | Median infundibular tube and saccus vasculosus. |
| 1. Supra-œsophageal ganglia. | Brain proper, or cerebral hemispheres. |
| 2. Olfactory ganglia | Olfactory lobe. |
| 3 Optic ganglia of the lateral eyes. | Optic ganglia of the lateral eyes. |
| 4 Optic ganglia of the median eyes. | Ganglia habenulæ. |
| 5. Median eyes. | Pineal eyes. |
| 6 Œsophageal commissures | Crura cerebri. |
| 7 Infra-œsophageal or prosomatic ganglia, giving origin to the prosomatic nerves. | Hind brain, giving origin to the IIIrd, IVth, and Vth cranial nerves. |
| 8. Mesosomatic ganglia, giving origin to the mesosomatic nerves. | Medulla oblongata, giving origin to the VIIth, IXth, and Xth cranial nerves |
| 9. Metasomatic ganglia. | Spinal cord. |

[1] Gaskell, *Journ. of Anat and Physiol.* vol. xxiii. 1888; *Journ. of Physiol.* vol. x. 1889; *Brain*, vol. xii. 1889; *Q. J. of Micr. Sci.* 1890.

## B. *Coincidence of Structure and Physiological Function.*

1. The simple non-glandular epithelium of the nerve tube coincides with the simple non-glandular epithelium of the alimentary canal, ciliated as it is in Daphnia.[1]

2. The structure and function of the cerebral hemispheres, olfactory lobes, and optic ganglia closely resemble the corresponding parts of the supra-œsophageal ganglia.

3. The structure of the right pineal eye, with its nerve end-cells and rhabdites, is of the same nature as that of a median arthropod eye.

4. The structure of the right ganglion habenulæ is the same as that of the optic ganglion of the median eye.

5. The region of the hind brain, like the region of the infra-œsophageal ganglia, is concerned with the co-ordination of movements.

6. The region of the medulla oblongata, like the mesosomatic region of Limulus and its allies, is concerned especially with the movements of respiration.

7. The centres for the segmental cranial nerves resemble closely in their groups of motor cells and plexus substance the centres for the prosomatic and mesosomatic nerves, with their groups of motor cells and reticulated substance (Punkt-Substanz).

3. *The third test is the ontogenetic test.* The theory must be in harmony with, and be illustrated by, the embryonic development of the central nervous system. Such is the case, for we see that the nerve tube arises as a simple straight tube opening by the neurenteric canal into the anus, the anterior part of the tube, *i.e.* the cephalic stomach region, being remarkably dilated; the anterior opening of this tube, or anterior neuropore, is considered by most authors to have been situated in the infundibular region.

Next comes the formation of the cerebral vesicles, indicating embryologically the constricting growth of nervous material outside the cephalic stomach. First, the formation of two cerebral vesicles by the growth of nervous material in the position of the ganglia habenulæ, posterior commissure, and Meynert's bundle; *i.e.* the constricting influence of commissures between the optic part of the supra-œsophageal ganglia and the infra-œsophageal ganglia, then the formation of the third cerebral vesicle by the constricting influence of the IVth nerve and commencing cerebellum. Subsequently the first cerebral vesicle is divided into two parts by another nerve commissure—the anterior commissure, *i.e.* by nerve material joining the supra-œsophageal ganglia. Further, the embryological evidence shows that in the spinal cord region the nerve masses are at first most conspicuous ventrally and laterally to the original tube, such ventral masses being early connected together with the strands of the anterior commissure; ultimately, by the growth of nervous material dorsalwards, the dorsal portion of the tube is compressed to form the posterior fissure and the substantia Rolandi, the original large lumen of the old intestine being thus reduced to the small central canal of the adult nervous system. Finally, this nerve tube is formed at a remarkably early stage, just as ought to be the case if it represented an ancient alimentary canal.

The ontogenetic test appears to fail in two points :—

1. That the nerve tube of vertebrates is an epiblastic tube, whereas if it represented the old invertebrate gut it ought to be largely hypoblastic.

2. The nerve tube of vertebrates is formed from the dorsal surface of the embryo, while the central nervous system of arthropods is formed from the ventral surface.

With respect to the first objection, it might be argued, with a good deal of plausibility, that the term hypoblast is used to denote that surface which is known by its later development to form the alimentary canal, that in fact, as Heymons[2] has pointed out, the theory of the germinal layers is not sufficiently well established to give it any phylogenetic value. It is, however, unnecessary to discuss

Hardy and McDougall, *Proc. Camb. Philos. Soc.* vol viii 1893
Heymons, *Die Embryonalentwickl. v. Dermapteren u. Orthopteren*, Jena, 1895.

this question, seeing that Heymons has shown that the whole alimentary tract in such arthropods as the earwig, cockroach, and mole cricket, is, like the nerve tube . of vertebrates, formed from epiblast.

The second objection appears to me more apparent than real. The nerve layer in the vertebrate, as soon as it can be distinguished, is always found to lie ventrally to the layer of epiblast which forms the central canal. In the middle line of the body, owing to the absence of the mesoblast layer, the cells which form the notochord and those which form the central nervous system form a mass of cells which cannot be separated in the earlier stages. The nerve layer in the arthropod lies between the ventral epiblast and the gut; the nerve layer in the vertebrate lies between the so-called hypoblast (*i.e.* the ventral epiblast of the arthropod) and the neural canal (*i.e.* the old gut of the arthropod). The new ventral surface of the vertebrate in the head region is not formed until the head fold is completed. Before this time, when we watch the vertebrate embryo lying on the yolk, with its nervous system, central canal, and lateral plates of mesoblast, we are watching the embryonic representation of the original Limulus-like animal; then, when the lateral plates of mesoblast have grown round, and met in the middle line to assist in forming the new ventral surface, and the head fold is completed, we are watching the embryonic representation of the transformation of the Limulus-like animal into the scorpion-like ancestor of the vertebrates.

In the Arthropoda, the simple epithelial tube which forms the stomach and intestine is not a glandular organ, and we find that the digestive part of the alimentary tract is found in the large organ, the so-called liver. This organ, together with the generative glands, forms an enormous mass of glandular substance, which, in Limulus, is tightly packed round the whole of the central nervous system and alimentary canal, along the whole length of the animal (represented in fig. 4 by the dark dotted substance). The remains of this glandular mass are seen in Ammocœtes in the peculiar so-called packing tissue around the brain and spinal cord (represented in fig. 6 by the dark dotted substance). It satisfies the three tests to the following extent:—

1. *The phylogenetic test.*—As we descend the vertebrate phylum, we find that the brain fills up the brain-case to a less and less extent, until finally in Ammocœtes a considerable space is left between brain and brain-case, filled up with a peculiar glandular-looking material, interspersed with pigment, which is not fat tissue, and is most marked in the lowest vertebrates. The natural interpretation of this phylogenetic history is that the cranial cavity is too large for the brain in the lowest vertebrates, and is filled up with a peculiar glandular substance because that glandular substance pre-existed as a functional organ or organs, and not because it was necessary to surround the brain with packing material in order to keep it steady, owing to the unfortunate mistake having been made of forming a brain much too small for its case.

2. *The anatomical test* shows that this glandular and pigmented material is in the same position with respect to the central nervous system of Ammocœtes as the generative and liver material with respect to the central nervous system and alimentary canal of Limulus.

3. *The ontogenetic test* remains to be worked out. I do not know the origin of this tissue in Ammocœtes; the evidence has not yet been given by Kuppfer.[1] He has, however, shown that the neural ridge gives origin to a mass of mesoblastic cells, the further fate of which is not worked out. The whole story is very suggestive from the point of view of my theory, but incomprehensible on the view that the neural ridge is altogether nervous.

Finally, we ought to find in the invertebrate group in question indications of the commencement of the enclosure of the alimentary canal by the central nervous system; such is, in fact, the case. In the scorpion group a marked process of cephalisation has gone on, so that the separate ganglia, both of the prosomatic and mesosomatic region, have fused together, and fused

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[1] Kuppfer, *Studien z. vergleich Entwicklungsgesch. d. Kopfes der Kranioten,* 2. Heft, München u. Leipzig, 1894.

also with the large supra-œsophageal mass.  In the middle of this large brain mass a small canal is seen closely surrounded and compressed with nervous matter, as is shown in this specimen of Thelyphonus, this canal is the alimentary canal.  Again, Hardy, in his work on the nervous system of Crustácea, has sections through the brain of Branchipus which demonstrate so close an attachment between the nervous matter of the optic ganglion and the anterior diverticulum of the gut that no line of demarcation is visible between the cells of the gut wall and the cells of the optic ganglion.

For all these reasons I consider that the tubular nature of the vertebrate central nervous system is explained by my hypothesis much more satisfactorily and fully than by any other as yet put forward; it further follows that if this hypothesis enables us to homologise all the other parts of the head region of the vertebrate with similar parts in the arthropod, then it ceases to be an hypothesis, but rises to the dignity of the most probable theory of the origin of vertebrates.

## Origin of Segmental Cranial Nerves.

1. *The phylogenetic test.*—It follows from the close resemblance of the brain region of the central nervous systems in the two groups of animals that the cranial nerves of the vertebrate must be homologous with the foremost nerves of such an animal as Limulus, and must therefore supply homologous organs.  Leaving out of consideration for the present the nerves of special sense, it follows that the segmental cranial nerves must be divisible into two groups corresponding to two sets of segmental muscles, viz. a group supplying structures homologous to the appendages of Limulus and its allies, and a group supplying the somatic or body muscles; in other words, we must find precisely what is the most marked characteristic of the vertebrate cranial nerves, viz. that they are divisible into two sets corresponding to a double segmentation in the head region.  The one set, consisting of the Vth, VIIth, IXth, and Xth nerves, supply the muscles of the branchial or visceral segments; the other set, consisting of the IIIrd, IVth, VIth, and XIIth nerves, the muscles of the somatic segments  Further, we see that the nerves supplying the branchial segments, like the nerves supplying the appendages in Limulus, are mixed motor and sensory, while the nerves supplying the somatic segments are all purely motor, the corresponding sensory nerves running separately as the ascending root of the fifth nerve; so also in Limulus, the nerves supplying the powerful body muscles arise separately from those supplying the appendages, and also are quite separate from the purely sensory or epimeral (Milne Edwards) [1] nerves which supply the surfaces of the carapace in the prosomatic and mesosomatic regions.  Finally, the researches of Hardy [2] have shown that the motor portion of these appendage nerves, just like the nerves of the branchial segmentation in vertebrates, i e. the motor part of the trigeminal, of the facial, of the glosso-pharyngeal, and of the vagus, arise from nerve centres or nuclei quite separate from those which give origin to the motor nerves of the somatic muscles.  The phylogenetic history, then, of the cranial nerves points directly to the conclusion that the Vth, VIIth, IXth, and Xth nerves originally innervated structures of the nature of arthropod appendages

We can, however, go further than this, for we find, as we trace downwards throughout the vertebrate kingdom the structures supplied by these nerves, that they are divisible into two well-marked groups, especially well seen in Ammocœtes, viz. :—

1. A posterior group, viz. the VIIth, IXth, and Xth nerves, which arise from the medulla oblongata and supply all the structures within a branchial chamber.

2. An anterior group, viz. the Vth nerves, which arise from the hind brain and supply all the structures within an oral chamber.
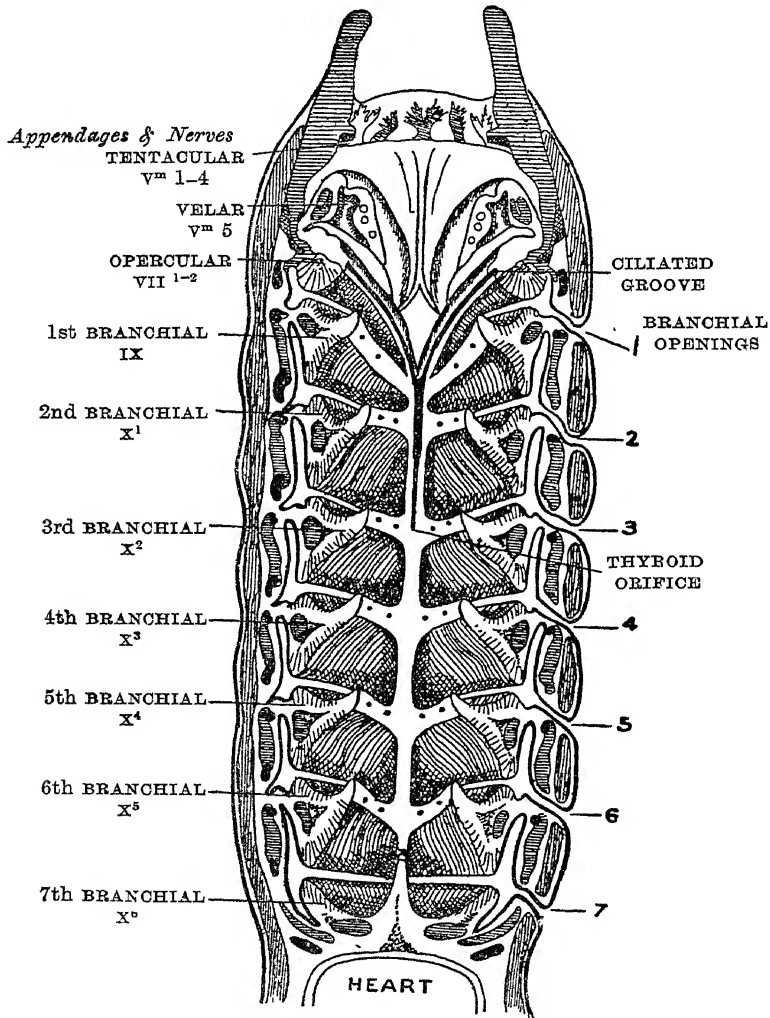
[1] Milne Edwards, ' Recherches sur l'Anatomie des Limulus,' *Ann. des Sc. Nat*, 5th ser

[2] Hardy, *Phil. Trans. Roy. Soc.* 1894,

The reason for this grouping is seen when we turn to Limulus and its allies, for we find that the body is always divided into a prosoma and mesosoma, and that the appendage nerves are divisible into two corresponding well-marked groups, viz :—

1. A posterior or mesosomatic group, which arise from the mesosomatic ganglia and supply the operculum and branchial appendages.

FIG. 3.—Head Region of Ammocœtes, split longitudinally into a ventral and dorsal half. (Ventral Half.)



*Appendages & Nerves*

TENTACULAR
$V^m$ 1-4

VELAR
$V^m$ 5

OPERCULAR
VII $^{1-2}$

1st BRANCHIAL
IX

2nd BRANCHIAL
$X^1$

3rd BRANCHIAL
$X^2$

4th BRANCHIAL
$X^3$

5th BRANCHIAL
$X^4$

6th BRANCHIAL
$X^5$

7th BRANCHIAL
$X^6$

CILIATED GROOVE

BRANCHIAL OPENINGS
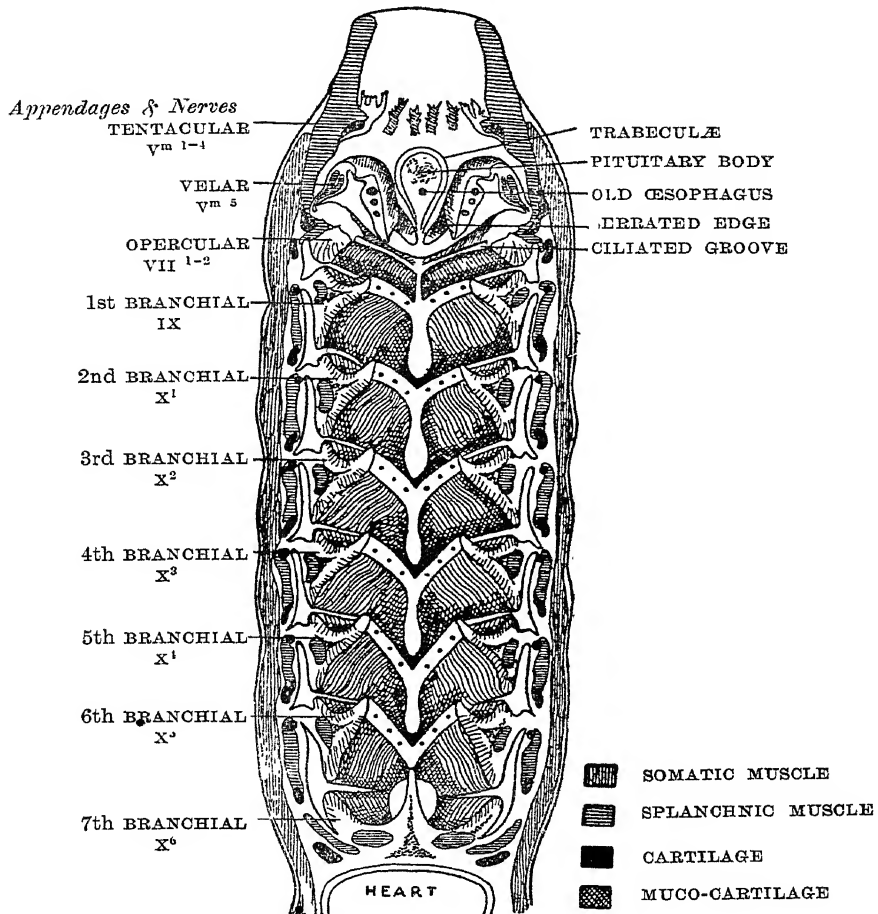
2

3

THYROID ORIFICE

4

5

6

7

HEART

2. An anterior or prosomatic group, which arise from the prosomatic ganglia and supply the oral or locomotor appendages.

*Comparison of the Branchial Appendages of Limulus, Eurypterus, &c., with the Branchial Appendages of Ammocœtes. Meaning of the IXth and Xth Nerves.*

We will first consider the posterior group—the VIIth, IXth, and Xth nerves—and of these I will take the IXth and Xth nerves together, and discuss the VIIth separately. These nerves are always described as supplying in the fishes the

FIG. 3 —Head Region of Ammocœtes, split longitudinally into a ventral and dorsal half. (Dorsal Half )



*Appendages & Nerves*

TENTACULAR
$V^m {}^{1-4}$

VELAR
$V^m {}^5$

OPERCULAR
VII $^{1-2}$

1st BRANCHIAL
IX

2nd BRANCHIAL
$X^1$

3rd BRANCHIAL
$X^2$

4th BRANCHIAL
$X^3$

5th BRANCHIAL
$X^4$

6th BRANCHIAL
$X^5$

7th BRANCHIAL
$X^6$

TRABECULÆ
PITUITARY BODY
OLD ŒSOPHAGUS
SERRATED EDGE
CILIATED GROOVE

SOMATIC MUSCLE
SPLANCHNIC MUSCLE
CARTILAGE
MUCO-CARTILAGE

HEART

muscles and other tissues in the walls of a series of gill-pouches, so that the respiratory chamber is considered to consist of a series of pouches, which open on the one hand into the alimentary canal, and on the other to the exterior. Such a description is possible even as low down as Petromyzon, but when we pass to the Ammocœtes we find the arrangement of the branchial chamber has become so different that it is no longer possible to describe it in terms of gill-pouches. The
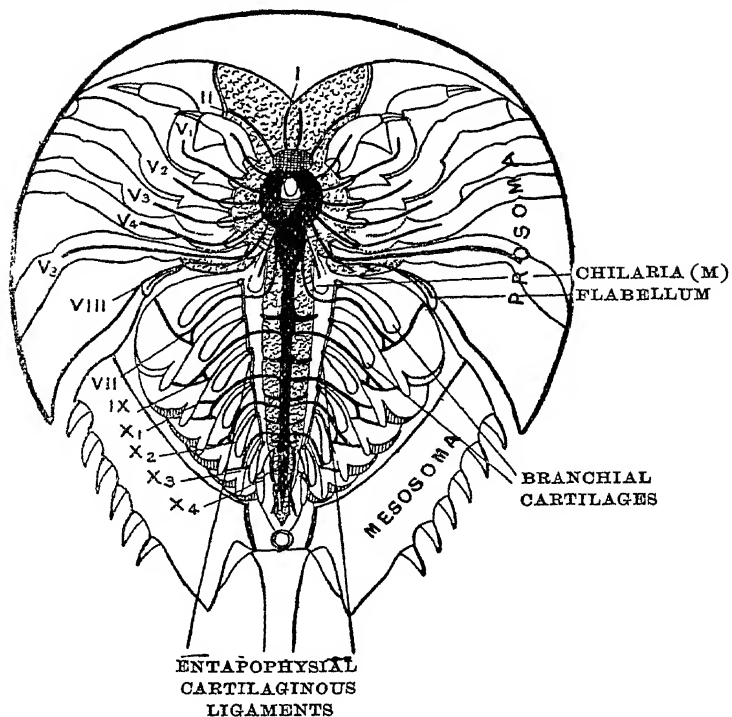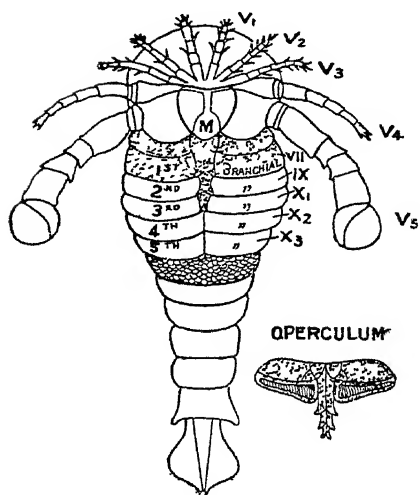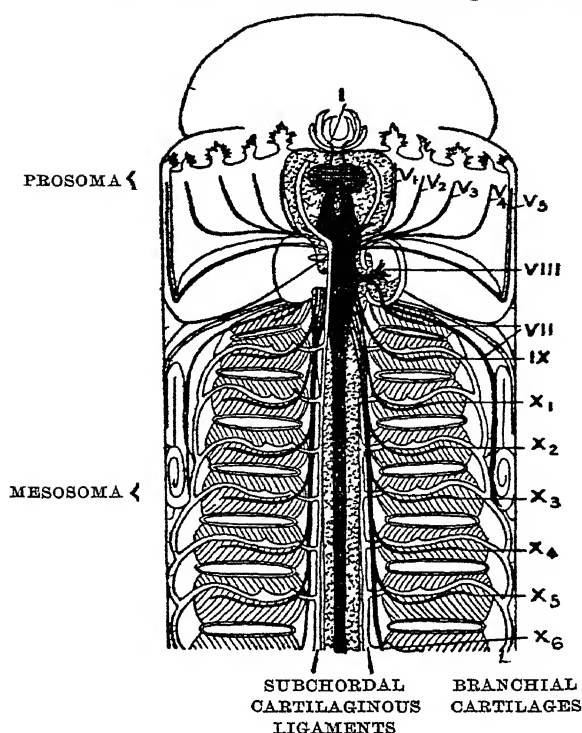
FIG. 4.—Limulus.  Nerves of Appendages and Cartilages.



FIG. 5.—Eurypterus

FIG. 6.—Ammocœtes.  Nerves of visceral segments and cartilages



In all three Figures $v_1$–$v_5$=Prosomatic appendages and nerves, $vii$=1st mesosomatic appendage or opercular appendage and nerves, $ix$, $x$, . . = remaining mesosomatic appendages and nerves ; M = Chilaria in Limulus, metastoma in Eurypterus.

nature of the branchial chamber is seen in fig. 3, which demonstrates clearly that the IXth and Xth nerves supply a series of separate gill-bearing structures or appendages, which hang freely into a common respiratory chamber, each one of these appendages is moved by its own separate group of branchial muscles, and possesses an external branchial bar of cartilage, which, by its union with its fellows, contributes to form the extra-branchial basket-work so characteristic of this primitive respiratory chamber.  The segmental branchial unit is clearly in this case, as Rathke originally pointed out, each one of these suspended gills, or rather gill-bearing appendages, it is absolutely unnatural, as Nestler [1] attempts to do, to take a portion of the space between two consecutive gills and call that a gill-pouch.  It is, to my mind, one of the most extraordinary and confusing conceptions of the current morphology to describe an animal in terms of the spaces between organs, rather than in terms of the organs by which those spaces are formed.  We might as well speak of a net as a number of holes tied together with string.  Another most striking advantage is obtained by considering the segmental unit to be represented by each of these separate branchial appendages—viz. that we can continue the series in the most natural manner (as seen in fig. 3) in front of the limits of the IXth and Xth nerves, and so find a series of appendages in the oral chamber serially homologous with the branchial appendages.  The uppermost of the respiratory appendages is the hyo-branchial, supplied

[1] Nestler, *Archiv f. Naturgeschich.* 56, vol. i.

by the VIIth nerve, then, passing into the oral chamber, we find a series of non-branchial appendages, viz. the velar and tentacular appendages, supplied by branches of the Vth nerve.  In fact, by simply considering the tissue between the so-called gill-pouches as the segmental unit, we no longer get lost in a maze of hypothetical gill-pouches in front of the branchial region, but find that the resemblances between the oral and branchial regions, which have led to the endless search for gill-slits and gill-pouches, really mean that the oral chamber contains appendages just as the branchial chamber, but that the former were not gill-bearing.

The study of Ammocœtes, then, leads directly to the conclusion that the ancestor of the vertebrate possessed an oral or prosomatic chamber, which contained a series of non-branchial, tactile and masticatory appendages, which were innervated from the fused prosomatic ganglia or hind brain, and a branchial or mesosomatic chamber, which contained a series of branchial appendages which were innervated from the fused mesosomatic ganglia or medulla oblongata.  These two chambers did not originally communicate with each other, for the embryological evidence shows that they are separated at first by the septum of the stomatodæum, and also that the oral chamber is formed by the forward growth of the lower lip.

The phylogenetic test on the side of Limulus and its congeners agrees in a remarkable manner with the conclusions derived from the study of Ammocœtes, for we see that the variation which has occurred in the formation of Eurypterus from Limulus is exactly of the kind necessary to form the oral and branchial chambers of the Ammocœtes.  Thus, we find with respect to the mesosomatic appendages that the free, many-jointed appendages of the crustacean become converted into the plate-like appendages of Limulus, in which the separate joints are still visible, but insignificant in comparison with the large branchiæ-bearing lamella ; then comes the in-sinking of these appendages, as described by Macleod,[1] to form the branchial lamellæ, or so-called lung-books of Thelyphonus, and the branchiæ of Eurypterus, in which all semblance of jointed and free appendages disappears and the branchiæ project into a series of chambers or gill-pouches, each pair of which in Thelyphonus open freely into communication.  In this way we see already the commencement of the formation of a branchial chamber similar to that of Ammocœtes

So also with the innervation of these mesosomatic appendages, originally a series of separate mesosomatic ganglia, each of which innervates a separate appendage , then a process of cephalisation takes place, in consequence of which, in the first place, a single ganglion, the opercular ganglion, fuses with the already fused prosomatic ganglia, as is seen in the stage of Limulus, then, as pointed out by Lankester, in the different groups of scorpions more and more of the mesosomatic ganglia fuse together, and so we find the upward variation in this group is distinctly in the direction of the formation of the medulla oblongata coincidently with the formation of a branchial chamber.

In a precisely similar way, we find the variation which has occurred in the prosomatic appendages leads directly to the formation of the oral chamber and oral appendages of Ammocœtes ; for the original chelate and locomotor appendages of Limulus become converted into the tactile non-chelate appendages of Eurypterus (cf. figs. 4 and 5), and the small chilaria (M) of Limulus, according to Lankester, fuse in the middle line and grow forward to form the metastoma of Eurypterus, thus forming an oral chamber, into which the short tactile appendages could be withdrawn, closely similar in its formation to the oral chamber of Ammocœtes. The prosomatic ganglia supplying these oral appendages have already, in Limulus (see fig. 4), been fused together to form the infra-œsophageal ganglia or hind brain.

The phylogenetic test, then, both on the side of the vertebrate and of the invertebrate, points direct to the conclusion that the peculiarities of the trigeminal and vagus groups of nerves are due to their origin from nerves supplying prosomatic and mesosomatic appendages respectively.

2. *The anatomical test* confirms and emphasises this conclusion in a most striking manner, for we find not only coincidence of topographical arrangement, as

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[1] Macleod, *Archiv de Biologie*, vol v 1884.

already mentioned, but also similarity of structure : thus we see that the blood in the gill lamellæ and velar appendages of Ammocœtes does not circulate in distinct capillaries, but, as in the arthropod appendages, in lacunar spaces, which by the subdivision of the surface of the appendage to form gill lamellæ become narrow channels ; that also certain of the branchial muscles and of the muscles of the velar appendages are of the invertebrate type of so-called tubular muscles. These invertebrate muscles are not found in higher vertebrates, but only in Ammocœtes, and moreover disappear entirely at transformation.

## Origin of the Vertebrate Cartilaginous Skeleton

Perhaps, however, the most startling evidence in favour of the homology between the branchial segments of Ammocœtes and the branchial appendages of Limulus is found in the fact that a cartilaginous bar external to the branchiæ exists in each one of the branchial appendages of Limulus, to which some of the branchial muscles are attached in precisely the same way as in Ammocœtes.  The branchial cartilages of Limulus (see fig 4) spring from the entapophyses and form strong cartilaginous bars which are extra-branchial in position, just as in Ammocœtes, in addition to each branchial bar, a cartilaginous ligament passes from one entapophysis to another, so as to form a longitudinal or entapophysial ligament, more or less cartilaginous, which extends on each side along the length of the mesosoma.   In precisely the same way the branchial bars of Ammocœtes are joined together along each side of the notochord by a ligamentous band of more or less continuous cartilaginous tissue, forming a subchordal or parachordal cartilaginous ligament.

Further, we see that this cartilage of Limulus is of a very striking structure, quite different from that of vertebrate cartilage, and that it is formed in a fibro-massive tissue which, like the matrix of the cartilage, gives a deep purple stain with thionin, thus showing the presence of some form of chondro-mucoid.   This fibro-massive tissue is closely connected with the chitinogenous cells of the entapo-

Startling is it to find that the branchial cartilages of Ammocœtes possess identically the same structure as the cartilages of Limulus; that the branchial cartilages are formed in a fibro-massive tissue which, like the matrix of the cartilage, gives a deep purple stain with thionin, and that this fibro-massive tissue, to which Schneider [1] gives the name of muco-cartilage, or Vorknorpel, entirely disappears at transformation.

Further, according to Shipley,[2] the cartilaginous skeleton of the Ammocœtes when first formed consists simply of a series of straight branchial bars, springing from a series of cartilaginous pieces arranged bilaterally along the notochord.

The formation of the trabeculæ, of the auditory capsules, of the crossbars to form the branchial basket-work, all occur subsequently, so that exactly those parts which alone exist in Limulus are those parts which alone exist at an early stage in Ammocœtes.   Another distinction is manifest between these branchial cartilages and those of the trabeculæ and auditory capsules, in that the latter do not stain in the same manner ; whereas the matrix of the branchial cartilages stains red with picro-carmine, that of the trabeculæ and auditory capsules stains deep yellow, so that the junction between the trabeculæ and the first branchial bar is well marked by the transition from the one to the other kind of staining   The difference corresponds to Parker's [3] soft and hard cartilage.

The new cartilages which are formed at transformation, either in places where muco-cartilage exists before or by the invasion of the fibrous tissue of the brain-case by chondroblasts, are all of the hard cartilage variety.

The phylogenetic, anatomical, and ontogenetic history of the formation of the

[1] Schneider, *Beitrage z   Anat   u.  Entwicklungsgesch. der Wirbelthiere*   Berlin, 1879.

[2] Shipley, *Quart  Journ of Micr  Sci*  1887

[3] Parker, *Phil. Trans. Roy. Soc*  1883

vertebrate skeleton all show how the bony skeleton is formed from the cartilaginous, and how the cartilaginous skeleton can be traced back to that found in Petromyzon, and so to the still simpler form found in Ammocœtes; from this, again, we can pass directly to the cartilaginous skeleton of Limulus, and so finally trace back the cranial skeleton of the vertebrate to its commencement in the modified chitinous ingrowths connected with the entapophyses of Limulus. A similar explanation of the origin of cartilage from modifications of the chitinous ingrowths of Limulus was suggested by Gegenbauer [1] so long ago as 1858, in consideration of the near chemical resemblances between the chitin and mucin groups of substances.

### Comparison of the Thyroid and Hyo-branchial Appendage of Ammocœtes with the Opercular Appendage of Eurypterus, Thelyphonus, &c. Meaning of the VIIth Nerve.

Seeing, then, how easily the IXth and Xth nerves in Ammocœtes correspond to the mesosomatic nerves to the branchial appendages in Limulus, and therefore to the corresponding nerves in such an animal as Eurypterus, we may with confidence proceed to the consideration of the VIIth nerve, and anticipate that it will be found to innervate a mesosomatic appendage in front of the branchial appendages, and yet belonging to the branchial group; in other words, if the VIIth nerve is to fit into the scheme, it ought to innervate a structure or structures corresponding to the operculum of Limulus or of Thelyphonus, &c. Now we see in figs. 5 and 8 the nature of the operculum in Eurypterus and in Thelyphonus, Phrynus, &c. It is in reality composed of two parts, a median and anterior portion which bears on its under surface the external genital organs, and a posterior part which bears branchiæ; so that the operculum of these animals may be considered as a genital operculum fused to a branchial appendage, and therefore double. It is absolutely startling to find that the branchial segment immediately in front of the glosso-pharyngeal segment in Ammocœtes (fig. 3) consists of two parts, of which the posterior, the hyo-branchial, is gill-bearing, while the anterior carries on its under surface the pseudo-branchial groove of Dohrn, which continues as a ciliated groove up to the opening of the thyroid gland.

Again, the comparison of the ventral surfaces of Eurypterus and Ammocœtes (cf. fig. 5 and fig. 8) brings to light a complete coincidence of position between the median tongue of the operculum in the one animal and the median plate of muco-cartilage in the other animal, which separates in so remarkable a manner the cartilaginous basket-work of each side, and bears on its under surface the thyroid gland. Finally, Miss Alcock has shown that not only the hyo-branchial, but also the thyroid part of this segment, is innervated by the VIIth nerve; so that every argument which has forced us to the conclusion that the glosso-pharyngeal and vagus nerves are the nerves which originally supplied branchial appendages equally points to the conclusion that the facial nerve originally supplied the opercular appendage—an appendage which closed the branchial chamber in front, which consisted of two parts, a branchial and a genital, probably indicating the fusion of two segments; and that the thyroid gland belonged to the genital operculum, just as the branchiæ belonged to the branchial operculum. This interpretation of the parts supplied by the facial nerve immediately explains why Dohrn is so anxious to make a thyroid segment in front of the branchial segments, and why a controversy is still going on as to whether the facial supplies two segments or one.

What, then, is the thyroid gland? Of all the organs found in the vertebrate, with perhaps the single exception of the pineal eye, there is no one which so clearly is a relic of the invertebrate ancestor as the thyroid gland. This gland, important as it is known to be in the higher vertebrates, remains of much the same type of structure down to the fishes, and even to Petromyzon; suddenly, when we pass to the Ammocœtes, to that larval condition so pregnant with invertebrate surprises, we find that the thyroid has become a large and important organ,

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[1] Gegenbauer, 'Anat. Untersuch. eines Limulus,' *Abhandl. der Naturf Gesellsch. in Halle*, 1858.

totally different in structure from the thyroid of all other vertebrates, though resembling the endostyl of the Tunicates.

The thyroid of Ammocœtes may be described as a long tube, curled up at its posterior end, which contains in its wall, along the whole of its length, a peculiar glandular structure, confined to a small portion of its wall.

A section through this tube is given in fig. 7, and shows how this glandular structure possesses no alveoli, no ducts, but consists of a column of elongated cells arranged in a wedge-shaped manner, the apex of the wedge being in the lumen of the tube; each cell contains a spherical nucleus, situated at the very extreme

FIG. 7.



MUCO-CARTILAGE

BRANCHIAL        ,,

OPERCULUM

Thyroid (Ammocœtes)　　　　　　　　　　Thyroid (Scorpion)
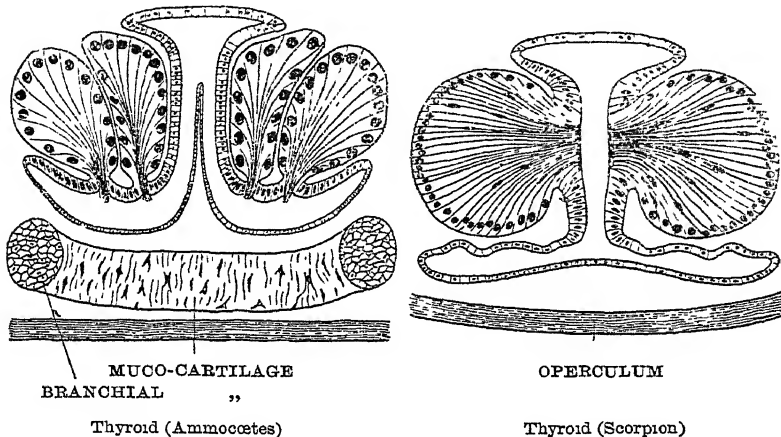
end of the cell, farthest away from the lumen of the tube. Such a structure is different form that of any other vertebrate gland. Its secretion is not in any way evident. It certainly does not secrete mucus or take part in digestion, and for a long time I was unable to find any structure which resembled it in the least degree, apart, of course, from the endostyl of the Tunicates.

Guided, however, by the considerations already put forward, and feeling therefore convinced that in Eurypterus there must have been a structure resembling the thyroid gland underneath the median projection of the operculum, I proceeded to investigate the nature of the terminal genital apparatus underlying the operculum in the different members of the scorpion family, and reproduce here (fig. 8) the figures given by Blanchard [1] of the appearance of the terminal male genital organs in Phrynus and Thelyphonus. Emboldened by the striking appearance of these figures, I proceeded to cut sections through the operculum of the European scorpion, and found that that part of the genital duct which underlies the operculum, and that part only, contains within its walls a glandular structure which resembles the thyroid gland of Ammocœtes in a remarkable degree. A section is represented in fig. 7, and we see that under the operculum in the middle line is situated a tube, the walls of which in one part on each side are thickened by the formation of a gland with long cells of the same kind as those of the thyroid; the nucleus is spherical, and situated at the farther end of the cell, and the cells are arranged in wedges, so that the extremities of each group of cells come to a point on the surface of the inner lining of the tube. This point is marked by a small round opening in the internal chitinous lining of the tube. These cells form a column along the whole length of the tube, just as in the thyroid gland, so that the chitinous lining along that column is perforated by numbers of small round

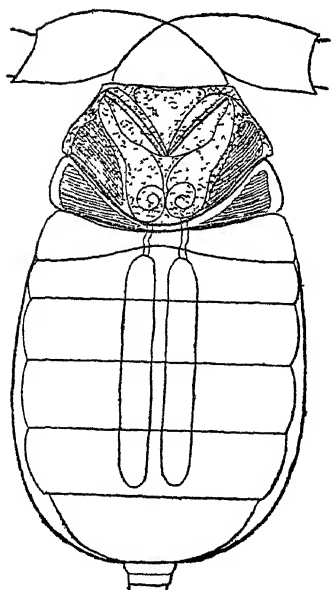[1] Blanchard, *L'Organisation du Règne Animal*

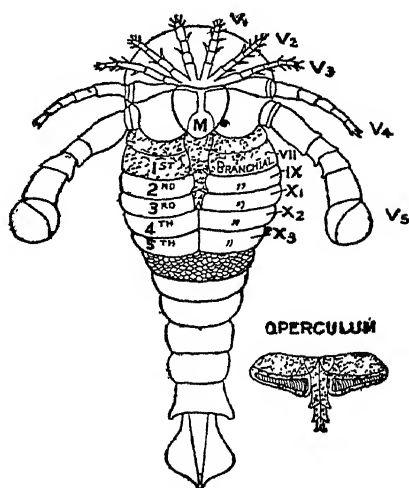Fig. 8. Comparison of the ventral surface of the branchial region
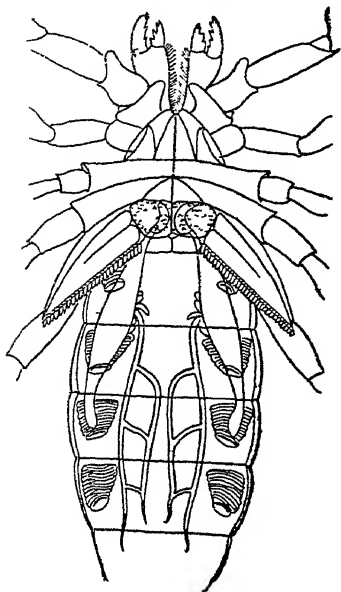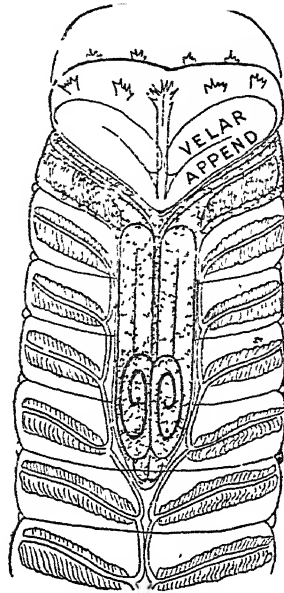


ANDROCTONUS.
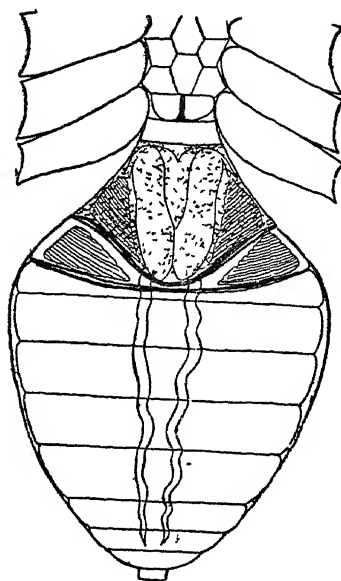
PHRYNUS.

THELYPHONUS.

EURYPTERUS

AMMOCŒTES.

In all figures the opercular appendage is marked out by its dotted appearance

holes. This glandular structure is not confined to the male scorpion, but is found also in the female, though not so well developed.

So characteristic is the structure, so different from anything else, that I have no hesitation in saying that the thyroid of Ammocœtes is the same structurally as the thyroid of the scorpion, and that, therefore, in all probability the median projection of the operculum in the old forms of scorpions, such as Eurypterus, Pterygotus, Slimonium, &c., covered a glandular tube of the same nature as the thyroid of Ammocœtes.

We see, then, that the structures innervated by the VIIth, IXth, and Xth nerves are absolutely concordant with the view that the primitive vertebrate respiratory chamber was formed from the mesosomatic appendages of such a form as Limulus by a slight modification of the method by which the respiratory apparatus of Thelyphonus and other Arachnids has been formed, according to Macleod. The anterior limit of this chamber was formed by the operculum, the basal part of which formed a septum which originally separated the branchial from the oral chamber.

### Comparison of the Oral Chamber of Ammocœtes with that of Eurypterus. Meaning of the Vth Nerve.

Passing now to the oral chamber—*i.e.* to the visceral structures innervated by the Vth nerve—we find, as already suggested, distinct evidence in Ammocœtes of the presence of the modified prosomatic appendages of the original Eurypterus-like form. The large velar appendage is the least modified, possessing as it does the arthropod tubular muscles, a blood system of lacunar blood-spaces, and a surface covered with a regular scale-like pattern, formed by cuticular nodosities, similar to that found on the surface of Eurypterus and other scorpions. The velar appendages show, further, that they are serially homologous with the respiratory appendages, in that they have been utilised to assist in respiration, their movements being synchronous with the respiratory movements.

The separate part of the Vth nerve which supplies the velar appendage passes within it from the dorsal to the ventral part of the animal, and then, as Miss Alcock has shown, turns abruptly forward to supply the large median tentacle. This extraordinary course leads directly to the conclusion that this median tentacle, which is in reality double, constitutes, with the velum of each side, the true velar appendages.

Again, on each side of the middle line there are in Ammocœtes four large tentacles, each of which possesses a system of muscles, muco-cartilage, and blood-spaces, precisely similar to the median ventral tentacle already mentioned. Each of these is supplied, as Miss Alcock has shown, by a separate branch of the motor part of the Vth nerve (see fig. 6), and each branch is comparable with the branch supplying the large velar appendage.

That such tentacles are not mere sensory papillæ surrounding the mouth, but have a distinct and important morphological meaning, is shown by the fact that they are transformed in the adult Petromyzon into the remarkable tongue and suctorial apparatus: a modification of oral appendages into a suctorial apparatus which is abundantly common among Arthropods.

Finally, the Vth nerve innervates the visceral muscles of the lower and upper lips of Ammocœtes. In order, then, for the story to be complete, the homologues of the lower and upper lips must also be found in the system of prosomatic appendages of forms like Limulus and Eurypterus. The lower lip, like the opercular or thyroid appendage, possesses a plate of muco-cartilage, and, as already mentioned, falls into its natural place as the metastoma of the old Eurypterus-like form, by the enlargement and forward growth of which the oral chamber of Ammocœtes was formed. The meaning of the upper lip will be considered with the consideration of the old mouth tube  The comparison of the metastoma of Eurypterus with the lower lip of Ammocœtes demonstrates the close resemblance between the oral chambers of Eurypterus and Ammocœtes. In order to obtain the condition of affairs in Ammocœtes from that in Eurypterus, it is only necessary that the metastoma should increase in size, and that the last oral appendage, the large oar-appendage, should follow the example of the other oral appendages, and be withdrawn into the oral cavity, and so form the velar appendage.

Thus we see that, just as the mesosomatic appendages of Limulus can be traced into the branchial and thyroid appendages of Ammocœtes through the intermediate stage of forms similar to Eurypterus, so also the prosomatic appendages and chilaria of Limulus can be traced into the velar and tentacular appendages and lower lip of Ammocœtes through the intermediate stage of forms similar to Eurypterus.

3  *Lastly comes the ontogenetic test.*  The concordant interpretation of the origin of the motor part of the Vth, of the VIIth, IXth, and Xth nerves given by the anatomical and phylogenetic tests must explain and be illustrated by the facts of the development of Ammocœtes.

We see :—

1. The oral chamber of Ammocœtes is known in its early stage by the name of the stomatodæum, and we find, as might be anticipated, that it is completely separated at first from the branchial chamber by the septum of the stomatodæum.

2. This septum is the embryological representative of the basal part of the operculum, and demonstrates that originally the operculum separated the oral and branchial chambers.

3. Subsequently these two chambers are put into communication by the breaking through of this septum, illustrating the communication between the two chambers by the separation of the median basal parts of the operculum.

4. The velar appendages, the tentacular appendages, the lower lip, all form as out-buddings, just as the homologous locomotor appendages are formed in arthropods.

5. The branchial bars are not formed by a series of inpouchings in a tube of uniform thickness, but, as Shipley[1] has pointed out, by a series of ingrowths at

[1] *Loc cit.*

regular intervals; in other words, the embryological history represents a series of buddings—*i.e.* appendages within the branchial chamber similar to the buddings within the oral chamber—and does not indicate the formation of gill-pouches by the thinning of an original thick tube at definite intervals.

6. The communication of the branchial chamber with the exterior by the formation of the gill-slits represents a stage in the ancestral history which is conceivable, but cannot at present be explained with the same certainty as most of the embryological facts of vertebrate development. I can only say that Strubel[1] has pointed out, and I can confirm him, that after the young Thelyphonus has left the egg, and is on its mother's back, before the moult which gives it the same form as the adult, the gills and gill-pouches are fully formed, but do not as yet communicate with the exterior.

7. The branchial cartilages in the Ammocœtes are formed distinctly before the auditory capsules and trabeculæ, illustrative of the fact that they alone are formed in Limulus.

*Comparison of the Auditory Apparatus of Ammocœtes with the Flabellum of Limulus. Meaning of the VIIIth Nerve.*

The correctness of a theory is tested in two ways ·—(1) It must explain all known facts; and (2) it ought to bring to light what is as yet unknown, and the more it leads to the discovery of new facts, the more certain is it that the theory is true. So far, we see that the prosomatic and mesosomatic regions of the body in Limulus and the scorpions are comparable with the corresponding regions of Ammocœtes as far as their locomotor and branchial appendages are concerned, and that, therefore, a satisfactory explanation is given of the peculiarities of the Vth, VIIth, IXth, and Xth nerves. In all vertebrates, however, there is invariably found a special nerve, the VIIIth nerve, entirely confined to the innervation of the special sense-organs of the auditory apparatus. It follows, therefore, that if my theory is true the VIIIth nerve must be found in such forms as Limulus and its allies, and that, therefore, a special sense-organ, probably auditory in nature, must exist between the prosomatic and mesosomatic appendages, at the very base of the last prosomatic appendage. At present we know nothing about the nature or locality of the hearing apparatus of Limulus. It is, therefore, all the more interesting to find that in the very position demanded by the theory, at the base of the last prosomatic appendage, is found a large hemispherical organ, to which a movable spatula-like process is attached, known by the name of the *flabellum.* This organ is confined to the base of this limb; it is undoubtedly a special sense-organ, being composed mainly of nerves, in connection with an elaborate arrangement of cells and innumerable fine hairs, which are thickly imbedded in the chitin of the upper surface of the spatula  The arrangement of these cells and hairs is somewhat similar to that of various sense-organs described by Gaubert,[2] and supposed to be auditory. When the animal is at rest this sensory surface projects upwards and backwards into the crack between the prosomatic and mesosomatic carapaces, so that while the eyes only permit a look-out forwards and sidewards, and the whole animal is lying half buried in the sand, any vibrations in the water around can still pass through this open crevice, and so reach the sensory surface of this organ.

Finally, the most striking and complete evidence that this sense-organ of Limulus is homologous with the auditory capsule of Ammocœtes is found in the fact that in each case the nerve is accompanied into the capsule by a diverticulum of the liver and generative organs. (See dotted substance in figs. 4 and 6.)  In Limulus the liver and generative organs, which surround the central nervous system from one end of the body to the other, do not penetrate into any of the appendages, with the single exception of the *flabellum.*

In Ammocœtes the peculiar glandular and pigmented tissue which surrounds

[1] Strübel, *Zool. Anzeiger,* vol xv. 1892.
[2] Gaubert, *Ann. d  Sci  Nat., Zool*, 7th ser., tome 13, 1892.

the brain and spinal cord, and has already been recognised as the remains of the liver and generative organs, does not penetrate into the velar or other appendages, but is found only in the auditory capsule, where it enters with and partly surrounds the auditory nerve.

The coincidence is so startling and unexpected as to bring conviction to my mind that in the *flabellum* of Limulus we are observing the origin of the vertebrate auditory apparatus ; and it is, to say the least of it, suggestive that in Galeodes the last locomotor appendage should carry the extraordinary racquet-shaped organs which Gaubert has shown to be sense-organs of a special character, and that in the scorpion a large special sense-organ of a corresponding character, viz. the pecten, should be found which, from its innervation, as given by Patten,[1] appears to belong to the segment immediately anterior to the operculum, rather than to that immediately posterior to it.

*Comparison of the Olfactory Organ of Ammocœtes with the Camerostome of Thelyphonus. Meaning of the 1st Nerve. Also comparison of the Hypophysis with the Mouth-tube of Thelyphonus.*

In precisely the same way as the theory has led to the discovery of a special sense-organ in Limulus and its allies which may well be auditory, so also it must lead to the discovery of the olfactory apparatus of the same group, for here also, just as in the case of the auditory apparatus, we are at present entirely in the dark.

The olfactory organ in such an animal as Thelyphonus ought to be innervated from the supra-œsophageal ganglia, and ought to be situated in the middle line, in front of the mouth. The mouth is at the anterior end in these animals, the lower lip or hypostoma (see fig 9) being formed by the median projecting flanges of the basal joints of the two pedipalpi, above, in the middle line, is a peculiar median appendage called the camerostome. Still more dorsal we find in the median line the rostrum, with the median eyes near its extremity, and laterally on each side of the camerostome. and dorsal to it, are situated the powerful cheliceræ, which are considered by some authorities to represent antennæ. Of these parts the camerostome is certainly innervated from the supra-œsophageal ganglia, and upon cutting sagittal and transverse sections in a very young Thelyphonus we find that the surface is remarkably covered with very fine sense-hairs, arranged with great regularity and connected with a conspicuous mass of large cells. Upon making transverse sections through this region we see that the camerostome projects into the orifice of the mouth, and that its sense-epithelium forms, together with a similar epithelium on the lower lip, a closed cavity surrounded by a thick hedge of fine hairs. Here, then, in the camerostome of Thelyphonus is a special sense-organ which, from its position and its innervation, may well be olfactory in function, or at all events subserve the function of taste.

Upon comparing this organ with the olfactory organ of Ammocœtes we see a most striking resemblance in general arrangement and structure

Just as the mouth tube of Thelyphonus is formed of two parts, the pedipalp and camerostome, so, according to Kuppfer, the nasal tube of Ammocœtes is composed of two parts, the upper lip and the olfactory protuberance. Of these two parts we see that the upper lip, or hood, like the pedipalp, is innervated by the Vth nerve, or nerve of the prosomatic appendages, while the olfactory protuberance, like the camerostome, is innervated by the 1st nerve. Kuppfer's investigations show us further (fig. 9) how the olfactory protuberance is at first free, is directed ventralwards, and lies at the opening of the hypophysial tube ; how afterwards, by the forward and upward growth of the upper lip to form the hood, the nasal tube is formed, with the result that the nasal opening lies on the dorsal surface just in front of the pineal eye. Kuppfer, like Dohrn and Beard, looks upon this hypophysial tube as indicating the palæostoma, or original mouth of the vertebrate, a view which harmonises absolutely with my theory, and receives the simplest of explanations from it, for, as you see on the screen, sections through the mouth tube

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[1] Patten, *Quart Journ of Micr. Sci* vol. xxxi. 1890.

of Thelyphonus are word for word the same as sections through the nasal tube of
Ammocœtes; here in the one section is the projecting camerostome, here is the
corresponding projection of the olfactory protuberance, here is the sense-epithelium
of the lower lip or hypostoma, here is the sense-epithelium of the upper lip or
hood. Here, as fig. 9 shows, the mouth tube passes in the ventral middle
line to where it turns dorsalwards into the middle of the conjoined nervous mass

FIG. 9.
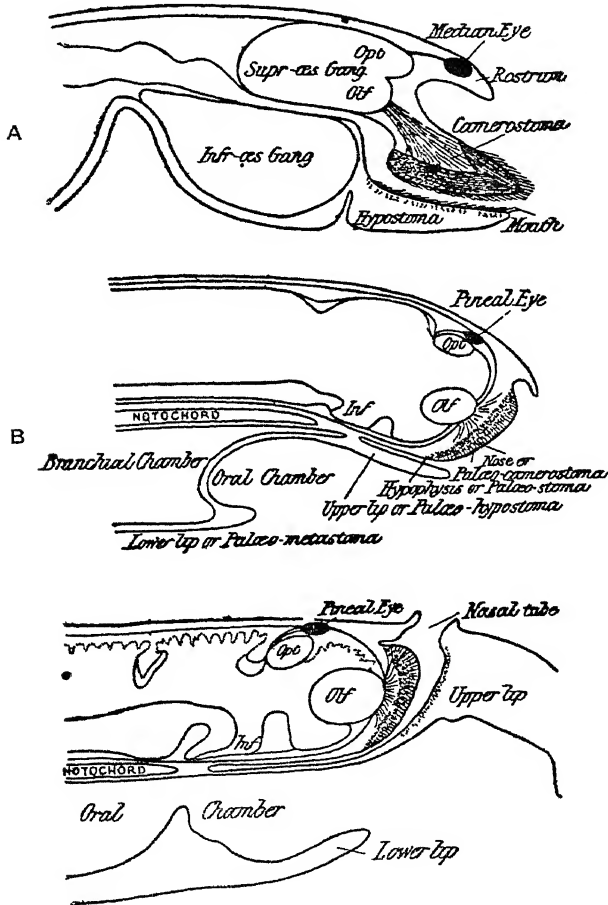
A — Median sagittal section through head of young Thelyphonus.
B.— ,, ,, ,, ,, ,, ,, Ammocœte (after Kuppfer)
C — ,, ,, ,, ,, ,, full-grown Ammocœte (after Kuppfer.)

of the supra- and infra-œsophageal ganglia. There the nasal tube ends blindly at
the spot where the infundibular tube lies on the surface of the brain.

Further, the topography of corresponding parts is absolutely the same in the
two animals: in the dorsal middle line the rostrum, with the two median eyes near
its extremity; in the corresponding position the two pineal eyes, below this, in the
middle line, the camerostome; corresponding to it in the Ammocœtes the olfactory

protuberance, then the modification of the median projections of the foremost ventral appendages—the pedipalpi—to form the hypostoma, in the corresponding position the upper lip or hood of Ammocœtes, which forms the hypostoma as far as the hypophysial tube or palæostoma is concerned, but an upper lip as far as the new mouth is concerned. The muscles of this upper lip belong all to the splanchnic and not to the somatic group, and are innervated by the appropriate nerve of the prosomatic appendages, viz the motor part of the Vth. Ventral to the pedipalpi in Thelyphonus there is nothing, ventral to the corresponding lip in the Ammocœtes is the lower lip, and we have seen that, although such a structure is absent in the land scorpions of the present day, it was present in the sea scorpions of old time, was known as the metastoma, and is supposed to be a forward growth which started at the junction of the prosoma with the mesosoma. Precisely corresponding to this we see from Kuppfer that the lower lip of Ammocœtes is a forward growth from the junction of the stomatodæum with the respiratory chamber.

We see then, so far, that the comparison of the vertebrate nervous system with the conjoined central nervous system and alimentary canal of the arthropod has led to a perfectly consistent explanation of almost all the peculiarities of the head region of Ammocœtes. We have solved the segmentation of the skull and the mysteries of the cranial nerves, for we have found that the cranial segmentation of the vertebrate can be reduced to the segmentation of the prosomatic and mesosomatic regions of the Limulus, that the cranial skeleton arose from the modified internal chitinous skeleton of the Limulus, that the new mouth was formed by the forward growth of the metastoma, leading to the formation of an oral chamber, while the old mouth remained as the hypophysial tube, guarded by its olfactory and taste organs.

Search as we may in the prosomatic and mesosomatic regions of scorpion-like animals, there are but few points left for elucidation; among these the most important are, 1, the fate of the cœlomic cavities and coxal gland, 2, the fate of the heart; 3, the fate of the external chitinous covering.

*Comparison of the Head Cavities of the Vertebrate with the Prosomatic and Mesosomatic Cœlomic Spaces of Limulus.*

A recent paper by Kishinouye [1] on the development of Limulus enables us to compare the cœlomic cavities in the head region of a vertebrate with those of the prosomatic and mesosomatic segments of Limulus, and we see that the comparison is wonderfully close; for whereas each mesosomatic segment possesses a cœlomic cavity, just as each of the segments of the branchial chamber supplied by the vagus, glossopharyngeal, and facial nerves possesses a cœlomic cavity, this is not the case with the prosomatic segments. In these latter the first cœlomic cavity is a large præoral one, common to the segment of the first appendage and all the segments in front of it; the segments belonging to the second, third, and fourth appendages have no cœlomic cavities formed in them, the second cœlomic cavity belongs to the segment of the fifth appendage. Similarly in the vertebrate in the region corresponding to the prosoma there are only two head cavities recognised, viz. the 1st præoral head cavity of Balfour and V. Wijhe; and 2nd or mandibular head cavity, associated especially with the Vth nerve According to my view the motor part of the Vth nerve represents the locomotor prosomatic appendages of Limulus, and we see that already in Limulus the three foremost of these appendages do not form cœlomic cavities

In fact, the agreement in the formation and position of the cœlomic cavities in the head region of the vertebrate and in the prosomatic and mesosomatic regions of Limulus could not well be more exact; further, these cavities agree in this, that in neither case are they permanent; both in the vertebrate and in the arthropod they are supplanted by vascular spaces.

[1] Kishinouye, *Journ of Coll. of Sci. Tokio*, vol. v. 1891.

## *Comparison of the Pituitary Gland with the Coxal Gland of Limulus.*

In connection with the second cœlomic cavity in Limulus is found an ancient gland, partially degenerated according to some views, which was probably excretory in function and has been considered as homologous to the crustacean green glands. In a precisely corresponding position, and presenting a structure fairly similar to that of the coxal gland of Limulus, we find in Ammocœtes and in other vertebrates the pituitary gland. How far this gland tissue is developed in connection with the mandibular head cavity I do not know, but I venture to suggest that the complete evidence of its homology with the coxal gland will be found in its developmental connection with the walls of the 2nd or mandibular head cavity.

## *Comparison of the Vertebrate Heart and Ventral Aorta with the Ventral Longitudinal Branchial Sinuses of Limulus and its Allies.*

The heart of the vertebrate presents two striking peculiarities, which make it different from all invertebrate hearts: first, its developmental history is different; and, secondly, it is at first essentially a branchial rather than a systemic heart. The researches of Paul Mayer [1] have shown that the subintestinal vein, from which in the fishes the heart and ventral aorta arise, is in its origin double, so that in all vertebrates the heart and ventral aorta arise from two long veins which are originally situated on each side of the middle line. By the formation of the head fold these come together ventrally, coalesce into a single tube to form the subintestinal vein and heart, still remaining double as the two ventral aortæ with their branchial branches into each gill, as is well shown in the case of Ammocœtes.

It is a striking coincidence that in Limulus and the Scorpions two large venous collecting sinuses are found situated in the same ventral position, for the same purpose of sending blood to the branchiæ, as already described for the vertebrate; still more striking is it to find, according to the researches of Milne Edwards and Blanchard, that these longitudinal sinuses have already begun to function as branchial hearts, for they are connected with the pericardium by a system of transparent muscles, described by Milne Edwards and named by Lankester veno-pericardiac muscles. These muscles are hollow, both near the vein and near the pericardium, so that the blood in each case fills the cavity, and, as they contract with the heart, that part of them in connection with the venous collecting sinus already functions, as pointed out by Milne Edwards and Blanchard, as a branchial heart.

By this theory, then, even the formation of the vertebrate heart is prevised in Limulus, and I venture to think that in Ammocœtes we see the remnant of the old dorsal single heart of the arthropod in the form of that peculiar elongated organ composed of fattily degenerated tissue which lies between the spinal cord and the dorsal median skin.

## *Comparison of the Cuticular and Laminated Layers of the Skin of Ammocœtes with Chitinous Layers.*

The external epithelial cells of Ammocœtes possess a remarkably thick cuticular layer. The striated appearance of this layer is due to a number of pores through which the glandular contents of the cells are poured when the surface is made to secrete. That this striated appearance is due to true porous canals, just as in chitin, and not to a series of rods, is easily seen by the inspection of sections, and also by watching the secretion through them of rose-coloured granules when the living cell is stained with methylene blue. The surface layer of this cuticular layer, according to Wolff,[2] resists reagents in the same manner as chitin.

[1] Mayer, *Mitth. a d. Zool St. zu Neapel*, vol. vii.
[2] Wolff, *Jen. Zeitschr.* vol. xxiii.

Internal to the epithelial cells of the skin of Ammocœtes is a remarkable layer of tissue, generally called connective tissue. It resembles, however, histo-logically, in the Ammocœtes, a section through chitin most closely ; the layers are perfectly regular and parallel , cells are found in it with great sparseness, and it is not until after transformation, when it is altered and invaded by new cell elements, that it can be looked upon as at all resembling connective tissue. It resembles chitin in its reaction to hypochlorite of soda. In order to completely dissect off this laminated layer from an Ammocœtes, all that is necessary is to place the animal in a weak solution of hypochlorite of soda, and in a short time it entirely disappears, bringing to view the muscles, branchial cartilages, pigment, front dorsal part of the central nervous system, &c., in a most striking manner. At present I am puzzled that so manifest a chitinous covering should lie internal to the epithelial cells of the surface ; such a position is not, however, unknown among invertebrates, and may be accounted for in various ways

For the sake of clearness I will sum up before you in the form of a table the corresponding parts in Ammocœtes and in Limulus and its allies, as far as I have discussed them up to the present, from which you will see that there is not a single organ which is present in the prosomatic and mesosomatic regions of Limulus and its allies which is not found in the corresponding situation and of corresponding structure in Ammocœtes.

*Table of Coincidences between Limulus and its Allies, and between Ammocœtes and Vertebrates.*

| LIMULUS AND ITS ALLIES. | AMMOCŒTES AND VERTEBRATES. |
|---|---|
| *Central Nervous System.* | |
| Supra-œsophageal ganglia | Cerebral hemispheres. |
|   Optic part | Optic thalami, ganglia habenulæ, &c. |
|   Olfactory part | Olfactory lobes. |
| Œsophageal commissures | Crura cerebri. |
| Infra-œsophageal ganglia | Epichordal brain. |
|   Prosomatic ganglia | Hind brain, cerebellum, post-corp quadrig. |
|   Mesosomatic ganglia | Medulla oblongata. |
| Ventral chain. | |
|   Metasomatic ganglia | Spinal cord. |
| *Alimentary Canal.* | |
| Cephalic stomach | Ventricular cavities of brain. |
| Straight intestine | Central canal of spinal cord. |
|   Terminal part | Neurenteric canal. |
| Œsophagus | Infundibular tube and saccus vasculosus. |
| Mouth tube | Hypophysial tube, later nasal canal. |
| Liver | Part of subarachnoideal glandular tissue. |
| *Appendages and Appendage Nerves.* | |
| Prosomatic or locomotor append- | Appendages of oral chamber or stoma-todæum. |
|   Foremost appendages | Upper lip and tentacles. |
|   Last appendages | Velar appendage and median ventral tentacle. |
|   Metastoma | Lower lip. |
| Nerves of prosomatic appendages | Various branches of Vth nerve. |
| Mesosomatic or branchial append- | Appendages of branchial chamber. |
|   Opercular appendages | Appendage innervated by VIIth nerve. |
|    Genital part | Thyroid gland and pseudo-branchial groove. |
|    Branch. part | Hyobranchial. |
|    Basal part | Septum of stomatodæum |
|   Branchial appendages | Branchial appendages innervated by IXth and Xth nerves. |
| *Special Sense Organs and Nerves.* | |
| Lateral eyes and optic nerves | Lateral eyes and optic nerves. |
| Median eyes and nerves | Pineal eyes and nerves. |

| | |
|---|---|
| Camerostoma and olfactory nerves | Olfactory organ and Ist nerve. |
| Flabellum and nerve . . . | Auditory organ and VIIIth nerve. |
| Epimeral nerves to surface of pro-soma and mesosoma | Sensory part of Vth nerve. |
| *Internal and External Skeleton* | |
| Internal skeleton | |
|     Branchial cartilages . | Branchial cartilages. |
|     Entapophysial cartilaginous ligaments . . | Subchordal cartilaginous ligaments. |
|     Fibro-massive tissue (fore-runner of cartilage or 'Vorknorpel'). . | Muco-cartilage or 'Vorknorpel.' |
| External skeleton | |
|     Chitinous layer . . | Cuticular layer on surface of body and subepithelial laminated layer |
| *Excretory Organs and Cœlomic Cavities.* | |
| Coxal gland . . . . . | Pituitary gland |
| 1st head cavity, præoral . . | 1st head cavity, præoral. |
| 2nd head cavity Cavity of pro-somatic segments . . | 2nd head cavity, mandibular. |
| Cavities to each mesosomatic segment . . . . . | Cavities of hyoid and branchial segments. |
| *Heart and Vascular System.* | |
| Dorsal heart . . . . | Column of fatty tissue dorsal to spinal cord. |
| Longitudinal venous sinuses | Heart and ventral aorta |
| Lacunar blood spaces of appendages . . . | Lacunar blood spaces in velar and branchial appendages. |

### The Possible Meaning of the Notochord.

Although we can say that every structure and organ in the prosomatic and mesosomatic regions of Limulus, &c., is to be found in the head region of Ammocœtes, we cannot assert the reverse proposition, that every organ in the head region of Ammocœtes is to be found in Limulus, &c., for we find a notable exception in the case of the notochord, a structure which is *par excellence* a vertebrate structure, and has in consequence given the current name to the group. Such a structure is clearly not to be found in Limulus and its allies; it has evidently arisen in connection with the formation of the vertebrate alimentary canal from the oral and branchial chambers, and it evidently at one time possessed a functional significance, for the lower we descend in the vertebrate scale the more conspicuous it becomes.

Unfortunately we know nothing of the condition of the notochord in the early extinct fishes, so that we are reduced to the embryological method of enquiry in our endeavours to find out the meaning of this organ. This method appears to point to the origin of the notochord from a tube connected with the alimentary canal, originally therefore an accessory digestive tube; the reasons why such a view has been put forward are, first, the origin of the notochord from hypoblast; secondly, the evidence that it is to a certain extent tubular; and thirdly, that it is an unsegmented tube extending from the oral to the anal regions of the body. Another argument, to my mind stronger than any other, is based on the principle that nature repeats herself, and if, therefore, we find the same proliferation of cells in the same place forming a series of solid notochordal rods, we may fairly argue that we are observing a series of repetitions of the same process for the same object. Now the formation of the head region of Petromyzon shows that at first a median proliferation of hypoblastic cells occurs to form the notochord, which then separates off from the hypoblast; later on a similar proliferation takes place to form the subnotochordal rod, which similarly separates off from the hypoblast; later still, at the time of transformation, a third median proliferation of the cells of the hypoblast takes place, to form a solid rod of cells. This solid rod then commences to hollow out at the end nearest the intestine, and the hollowing out

process extends gradually to the oral end, until a hollow tube is formed connecting the mouth with the intestine.   In this way the new gut of the adult Petromyzon is formed from a solid median rod of cells closely resembling in its formation the original notochord.

I put it forward therefore as a suggestion, that in the ancient times when the merostomata were lords of creation and the competition was keen among these ancient arthropod forms, in which the nervous system was so arranged that increase of brain substance tended more and more to compress the food channel, and therefore to compel to the suction of liquid food instead of the mastication of solid, accessory digestive apparatuses were formed, partly in connection with the formation of the oral and respiratory chambers, and partly by means of the formation of the notochord.   Of these accessory methods of digestion the former became permanent, while the latter becoming filled up with the peculiar notochordal tissue became a supporting structure, still showing by its unsegmented character its original function.   That a tube formed from the external surface either as notochord or as the respiratory portion of the alimentary canal in Ammocœtes should be capable of acting as a digestive tube is clear from the researches of Miss Alcock,[1] for she has shown that the secretion of the skin of Ammocœtes easily digests fibrin in the presence of acid.   Such a secretion, like the similar secretion of the carapace of Daphnia and other crustaceans, was originally for the purpose of keeping the skin clean.

The evidence which I have put before you is in agreement with the conclusion that the fore gut of the vertebrate arose gradually from a chamber formed by the lamellar branchial appendages, which functioned also as a digestive chamber.   By the growth of the lower lip, or metastoma, and the modification of the basal portion of the last locomotor appendage, which basal part was inside the lower lip, into a valvular arrangement like the velum, the animal was able to close the opening into the respiratory chamber and feed as blood-sucker in the way of the rest of its kind, or, when living food was scarce, keep itself alive by the organic material taken into its respiratory chamber with the muddy water in which it lived.

### The Possible Formation of the Vertebrate Spinal Region.

It remains to briefly indicate the evidence as to the formation of the rest of the alimentary canal and the spinal region of the body.

The problems connected with the formation of this region are of a different nature from those already considered in connection with the cranial region.

In the cranial region the variation that has taken place within the vertebrate group and in the course of the formation of the vertebrate is, on the whole, of the nature called by Bateson substantive, i.e. increase or suppression of parts, while throughout the parts remain constant in their relations to each other.   It matters not whether it is frog, fish, bird, or mammal we are considering; we always find the same cranial nerves supplying the same segments.   When we consider the spinal cord and its immediate junction with the cranial region, this is no longer so; here we find a repetition of similar segments, with great variation in the amount of that repetition; here we find the characteristic feature is meristic variation rather than substantive, and so indetermined is the vertebrate in this respect that even now the same species of animal varies in the number of its segments and in the arrangement of its nerves.   In this part of the vertebrate body this repetition is seen not only in the central nervous system and its nerves, but also in the excretory organs, so that embryology teaches us that the vertebrate body has grown in length by a series of repetitions of similar segments formed between the head end and the tail end; such lengthening by repetition of segments has been accompanied by the elongation of the unsegmented gut, of the unsegmented notochord, and of the unsegmented neural canal.

To put it shortly, all the evidence points to and confirms the view so strongly urged by Gegenbauer, that the head region is the oldest part and the spinal

[1] Alcock, *Proc. Camb. Phil. Soc.* vol. vii. 1891.

region an afterthought, that the attempt so often made to find vertebræ and spinal nerves in the cranial region is an attempt to put the cart in front of the horse—to obtain youth from old age. We may, it seems to me, fairly argue from the sequence of events in the embryology of vertebrates that the primitive vertebrate form was chiefly composed of the head region, and that between the head and the tail was a short body region. In other words, the respiratory chamber and the cloacal region were originally close together, just as would be the case in Limulus if the branchial appendages formed a closed chamber. According, then, to my view, there would be no difficulty in the respiratory chamber opening originally into the cloacal region, *i.e.* the same cloacal region into which the neurenteric canal already opened. The short junction tube thus formed would naturally elongate with the elongation of the body, and, as it originally was part of the respiratory chamber, it equally naturally is innervated by the vagus nerve. This, then, is the explanation of that most extraordinary fact, viz. that a nerve essentially branchial should innervate the whole of the intestine except the cloacal region. Whether this is the true explanation of the formation of the mid-gut of the vertebrate cannot be tested directly, but certain corollaries ought to follow : we ought to find, on the ground that the sequence of the phylogenetic history is repeated in the embryo, that, 1, the growth in length of the embryo takes place between the cranial and sacral regions by the addition of new segments from the cranial end; 2, the formation of the fore-gut and hind-gut ought to be completed while the mid-gut is still an undifferentiated mass of yolk cells ; 3 the cloacal region ought to be innervated from the sacral nerves, while the stomach, mid-gut and its appendages, liver and pancreas, ought to be innervated from the vagus.

The first proposition is a well-known embryological fact. The second proposition is also well known for all vertebrates, and is especially well exemplified in the embryological development of Ammocœtes, according to Shipley. The third proposition is also well known, and has received valuable enlargement in the recent researches of Langley and Anderson.[1] Further, we see that in this part of the body the ancestor of the vertebrate must have had a cœlomic cavity the walls of which were innervated, not from the mesosomatic nerves or respiratory nerves, but from the metasomatic group of nerves ; and in connection with this body cavity there must have existed a kidney apparatus, also innervated by the metasomatic nerves ; with the repetition of segments by which the elongation of the animal was brought about the body cavity was elongated, and the kidney increased by the repetition of similar excretory organs. All, then, that is required in the original ancestor in order to obtain the permanent body cavity and urinary organs characteristic of the vertebrate is to postulate the presence of a permanent body cavity in connection with a single pair of urinary tubes in the metasomatic region of the body. As yet I have not worked out this part of my theory, and am therefore strongly disinclined to make any assertions on the subject. I should like, however, to point out that, according to Kishinouye,[2] a permanent body cavity does exist in this part of the body in spiders, known by the name of the stercoral pocket ; into this cœlomic cavity the excretory Malphigian tubes open.

### *The Palæontological Evidence.*

It is clear, from what has already been said, that the palæontological evidence ought to show, first, that the vertebrates appeared when the waters of the ocean were peopled with the forefathers of the Crustacea and Arachnida, and, secondly, the earliest fish-like forms ought to be characterised by the presence of a large cephalic part to which is attached an insignificant body and tail.

Such was manifestly the case, for the earliest fish-like forms appear in the midst of and succeed to the great era of strange proto-crustacean animals, when the sea swarmed with Trilobites, Eurypterus, Slimonium, Limulus, Pterygotus, Ceratioceras, and a number of other semi-crustacean, semi-arachnid

[1] Langley and Anderson, *Journ. of Physiology*, vols. xviii., xix.
[2] Kishinouye, *Journ. of Coll. of Sci. Tokio*, vol. iv 1890, vol. vi. 1894.

creatures. When we examine these ancient fishes we find such forms as Pteraspis, Pterichthys, Astrolepis, Bothriolepis, Cephalaspis, all characterised by the enormous disproportion between the extent of the head region and that of the body. Such forms would have but small power of locomotion, and further evolution consisted in gaining greater rapidity and freedom of movements by the elongation of the abdominal and tail regions, with the result that the head region became less and less prominent, until finally the ordinary fish-like form was evolved, in which the head and gills represent the original head and branchial chamber, and the flexible body, with its lateral line nerve and intestine innervated by the vagus nerve, represents the original small tail-like body of such a form as Pterichthys.

Nay, more, the very form of Pterichthys and the nature of its two large oar-like appendages, which, according to Traquair, are hollow, like the legs of insects, suggest a form like Eurypterus, in which the remaining locomotor appendages had shrunk to tentacles, as in Ammocœtes, while the large oar-like appendages still remained, coming out between the upper and lower lips and assisting locomotion. The Ammocœtes-like forms which in all probability existed between the time of Eurypterus and the time of Pterichthys have not yet been found, owing possibly to the absence of chitin and of bone in these transition forms, unless we may count among them the recent find by Traquair of Palæospondylus Gunni.

The evidence of palæontology, as far as it goes, confirms absolutely the evidence of anatomy, physiology, phylogeny, and embryology, and assists in forming a perfectly consistent and harmonious account of the origin of vertebrates, the whole evidence showing how Nature made a great mistake, how excellently she rectified it, and thereby formed the new and mighty kingdom of the Vertebrata.

### Consideration of Rival Theories.

In conclusion I would ask, What are the alternative theories of the origin of vertebrates? It is a strange and striking fact how often, when a comparative anatomist studies a particular invertebrate group, he is sure to find the vertebrate at the end of it: it matters not whether it is the Nemertines, the Capitellidæ, Balanoglossus, the Helminths, Annelids, or Echinoderms; the ancestor of the vertebrate is bound to be in that particular group. Verily I believe the Mollusca alone have not yet found a champion. On the whole I imagine that two views are most prominent at the present day—(1) to derive vertebrates from a group of animals in which the alimentary canal has always been ventral to the nervous system; and (2) to derive vertebrates from the segmented group of animals, especially annelids, by the supposition that the dorsal gut of the latter has become the ventral gut of the former by reversion of surfaces. Upon this latter theory, whether it is Dohrn or van Beneden or Patten who attempts to homologise similar parts, it is highly amusing to see the hopeless confusion into which they one and all get, and the extraordinary hypotheses put forward to explain the fact that the gut no longer pierces the brain. One favourite method is to cut off the most important part of the animal, viz. his supra-œsophageal ganglia, then let the mouth open at the anterior end of the body, turn the animal over, so that the gut is now ventral, and let a new brain, with new eyes, new olfactory organs, grow forward from the infra-œsophageal ganglia. Another ingenious method is to separate the two supra-œsophageal ganglia, let the mouth tube sling round through the separated ganglia from ventral to dorsal side, then join up the ganglia and reverse the animal. The old attempts of Owen and Dohrn to pierce the dorsal part of the brain with the gut tube either in the region of the pineal eye or of the fourth ventricle have been given up as hopeless. Still the annelid theory, with its reversal of surfaces, lingers on, even though the fact of the median pineal eye is sufficient alone to show its absolute worthlessness.

Then, as to the other view, what a demand does that make upon our credulity! We are to suppose that a whole series of animals has existed on the earth, the development of which has run parallel with that of the great group of segmented animals, but throughout the group the nervous system has always been dorsal to the alimentary canal. Of this great group no trace remains, either alive at the

present day or in the record of the rocks, except one or two aberrant, doubtful forms, and the group of Tunicates and Amphioxus, both of which are to be looked upon as degenerate vertebrates, and indeed are more nearly allied to the Ammocœtes than to any other animal. This hypothetical group does ,not attempt to · explain any of the peculiarities of the central nervous system of vertebrates; its advocates, in the words of Lankester, regard the tubular condition of the central nervous system as in its origin a purely developmental feature, possessing no phylogenetic importance. Strange power of mimicry in nature, that a tube so formed should mimic in its terminations, in its swellings, in the whole of its topographical relations to the nervous masses surrounding it the alimentary canal of the other great group of segmented animals so closely as to enable me to put before you so large a number of coincidences.

Just imagine to yourselves what we are required to believe ! We are to suppose that two groups of animals have diverged from a common stock somewhere in the region of the Cœlenterata, that each group has become segmented and elongated, but that throughout their evolution the one group has possessed a ventral mouth, with a ventral nervous system and a dorsal gut, while in the other—the hypothetical group—the mouth and gut have throughout been ventral and the nervous system dorsal. Then we are further to suppose that, without being able to trace the steps of the process, the central nervous system in the final members of this hypothetical group has taken on a tubular form of so striking a character that every part of this dorsal nerve-tube can be compared to the dorsal alimentary tube of the other great group of segmented animals. The plain, straightforward interpretation of the facts is what I have put before you, and those who oppose this interpretation and hold to the inviolability of the alimentary canal are, it seems to me, bound to give a satisfactory explanation of the vertebrate nervous system and pineal eye. The time is coming, and indeed has come, when the fetish-worship of the hypoblast will give way to the acknowledgment that the soul of every individual is to be found in the brain, and not in the stomach, and that the true principle of evolution, without which no upward progress is possible, consists in the steady upward development of the central nervous system.

# British Association for the Advancement of Science.

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# ADDRESS

### TO THE

# BOTANICAL SECTION

### BY

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### *Present Position of Morphological Botany.*

THE object of modern morphological botany (the branch of our science to which I propose to limit my remarks) is the accurate comparison of plants, both living and extinct, with the object of tracing their real relationships with one another, and thus of ultimately constructing a genealogical tree of the vegetable kingdom. The problem is thus a purely historical one, and is perfectly distinct from any of the questions with which physiology has to do.

Yet there is a close relation between these two branches of biology; at any rate, to those who maintain the Darwinian position For from that point of view we see that all the characters which the morphologist has to compare are, or have been, adaptive. Hence it is impossible for the morphologist to ignore the functions of those organs of which he is studying the homologies. To those who accept the origin of species by variation and natural selection there are no such things as morphological characters pure and simple. There are not two distinct categories of characters—a morphological and a physiological category— for all characters alike are physiological. 'According to that theory, every organ, every part, colour, and peculiarity of an organism must either be of benefit to an organism itself, or have been so to its ancestors. . . . Necessarily, according to the theory of natural selection, structures either are present because they are selected as useful, or because they are still inherited from ancestors to whom they were useful, though no longer useful to the existing representatives of those ancestors.'[1]

The useful characters may have become fixed in comparatively recent times, or a long way back in the past. In the latter case the character in question may have become the property of a large group, and thus, as we say, may have become morphologically important.

For instance, parasitic characters, such as the suppression of chlorophyll, are equally adaptive in Dodder and in the Fungi. In Dodder, however, such characters are of recent origin, and of little morphological importance, not hindering us from placing the genus in the natural order Convolvulaceæ; while in Fungi equally adaptive characters have become the common property of a great class of plants.

Then, again, the existence of a definite sporophyte generation, which is the great character of all the higher plants, is in certain Fungi inconstant, even among members of the same species.

Although there is no essential difference between adaptive and morphological

Lankester, *Advancement of Science*, p 307.

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characters, there is a great difference in the morphologist's and the physiologist's way of looking at them. The physiologist is interested in the question how organs work ; the morphologist asks, what is their history ?

The morphologist may well feel discouraged at the vastness of the work before him. The origin of the great groups of plants is perhaps, after all, an insoluble problem, for the question is not accessible either to observation or experiment.

All that we can directly observe or experiment upon is the occurrence of variations—perhaps the most important line of research in biology, for it was the study of variation that led Darwin and Wallace to their grand generalisation. Many observers are working to-day in the spirit of the great masters, and it is certain that their work will be fruitful in results. It is evident, however, that such investigations can at most only throw a side light on the historical question of the origin of the existing orders and classes of living things. The morphologist has to attack such questions by other methods of research.

The embryological method has so far scarcely received justice from botanists. A great deal of what is called embryology in botany is not embryology at all, but relates to pre-fertilisation changes. Of real embryology—that is to say, the development of the young plant from the fertilised ovum—there is much less than we might expect. Thus no comparative investigation of the embryology of either Dicotyledons or Monocotyledons has ever been carried out, our knowledge being entirely based on a few isolated examples.

In the cases which have been investigated perhaps excessive attention has been devoted to the first divisions of the ovum, the importance of which, as Sachs long ago showed, has been overrated, while the later stages, when the differentiation of organs and tissues is actually in progress, have been comparatively neglected.

The law of recapitulation (or repetition of phylogeny in ontogeny) has been very inadequately tested in the vegetable kingdom. Whatever its value may be, it is certainly desirable that the development of plants as well as animals should be considered from this point of view ; and this has so far been done in but very few cases. M. Massart, of Brussels, has made some investigations with this object on the development of seedlings and of individual leaves. He is led to the conclusion that examples of recapitulation are rare among plants.[1]

So far, at least, embryological research has only yielded certain proof of recapitulation in a few cases, as in the well-known example of the phyllode-bearing acacias, in which the first leaves of the seedling are normal, while the later formed ones gradually assume the reduced phyllode form.

A less familiar example is afforded by *Gunnera*. Here, as is well known, the mature stem has a structure totally different from that of ordinary Dicotyledons, and much resembling that characteristic of most Ferns. In most species of *Gunnera* there are a number of distinct vascular cylinders in the stem, instead of one only, and there is never the slightest trace, so far as the adult plant is concerned, of the growth by means of cambium, which is otherwise so general in the class. The seedling stem, however, is not only monostelic below the cotyledons, but in this region, though nowhere else, shows distinct secondary growth. Thus, if we were in any doubt as to the general affinities of *Gunnera*, owing to its extraordinary mature structure, we should at once be put on the right track by the study of the embryonic stem, which alone retains the characteristic dicotyledonous mode of growth.

It is only in a few cases, however, and for narrow ranges of affinity, that the doctrine of recapitulation has at present helped in the determination of relationships among plants. Beyond this, conclusions based on embryology alone tend to become merely conjectural and subjective. In fact, all comparative work, in so far as it is limited to plants now living, suffers under the same weakness that it can never yield certain results, for the question whether given characters are relatively primitive or recently acquired is one upon which each naturalist is left to form his own opinion, as the origin of the characters cannot be observed.

[1] 'La Récapitulation et l'Innovation en Embryologie Végétale,' *Bull. de la Soc. roy. de Bot. de Belgique*, vol. xxxiii., 1894.

To determine the blood-relationships of organisms it is necessary to decipher their past history, and the best evidence we can have (when we can get it) is from the ancient organisms themselves. The problem of the morphologist is an historical one, and contemporary documentary evidence is necessarily the best. It is palæontology alone which can give us the real historical facts.

### ANATOMICAL CHARACTERS.

In judging of the affinities of fossil plants we are often compelled to make great use of vegetative characters, and more particularly of characters drawn from anatomical structure. It is true that in many cases we do so because we cannot help ourselves, such anatomical features being the only characters available in many of the specimens as at present known. But the value of the method has been amply proved in other cases where the reproductive structures have also been discovered, and are found to fully confirm the conclusions based on anatomy. I need only mention the great groups of the Lepidodendreæ and the Calamites, in each of which the anatomical characters, when accurately known, put us at once on the right track, and lead to results which are only confirmed by the study of the reproductive organs

In this matter fossil botany is likely to react in a beneficial way on the study of recent plants, calling attention to points of structure which have been passed over, and showing us the value of characters of a kind to which systematists had until recently paid but little attention. At present, owing to the work of Radlkofer, Vesque, and others, anatomical characters are gradually coming into use in the classification of the higher plants, and in some quarters there may even be a tendency to over-estimate their importance. Such exaggeration, however, is only a temporary fault incident to the introduction of a comparatively new method. In the long run nothing but good can result from the effort to place our classification on a broader basis. In most cases the employment of additional characters will doubtless serve only to further confirm the affinities already detected by the acumen of the older taxonomists. There are plenty of doubtful points, however, where new light is much needed ; and even where the classification is not affected it will be a great scientific gain to know that its divisions are based on a comparison of the whole structure, and not merely on that of particular organs.

The fact that anatomical characters are adaptive is undeniable, but this applies to all characters, such difference as there is being merely one of degree. Cases are not wanting where the vegetative tissues show greater constancy than the organs of reproduction, as, for example, in the Marattiaceæ, where there is a great uniformity in anatomical structure throughout the family, while the sporangia show the important differences on which the distinction of the genera is based. It is in fact a mistake to suppose that anatomical characters are necessarily the expression of recent adaptations. On the contrary, it is easy to cite examples of marked anatomical peculiarities which have become the common property of large groups of plants.

For instance, to take a case in which I happen to have been specially interested, the presence of bast to the inside as well as to the outside of the woody zone is a modification of dicotyledonous structure which is in many groups, at least of ordinal value. The peculiarity is constant throughout the orders Onagraceæ, Lythraceæ, Myrtaceæ, Solanaceæ, Asclepiadaceæ, and Apocynaceæ, not to mention some less important groups. In other families, such as the Cucurbitaceæ and the Gentianeæ, it is nearly constant throughout the order, but subject to some exceptions. Among the Compositæ a similar, if not identical, peculiarity appears in some of the sub-order Cichoriaceæ, but is here not of more than generic value. In *Campanula* the systematic importance of internal phloëm is even less, for it appears in some species and not in others. Lastly, there are cases in which a similar character actually appears as an individual variation, as in *Carum Carvi*, and, under abnormal conditions, in *Phaseolus multiflorus*.

These latter cases seem to me worthy of special study, for in them we can

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trace, under our very eyes, the first rise of anatomical characters which have else-where become of high taxonomic importance. A comparative study of the anatomy of any group of British plants, taking the same species growing under different conditions, would be sure to yield interesting results if any one had the patience to undertake it.

Enough has been said to show that a given anatomical character may be of a high degree of constancy in one group while extremely variable in another, a fact which is already perfectly familiar as regards the ordinary morphological charac-ters. For example, nothing is more important in phanerogamic classification than the arrangement of the floral organs as shown in ground-plan or floral diagram. Yet Professor Trail's observations, which he has been good enough to communicate to me, show that in one and the same species, or even individual, of *Polygonum*, almost every conceivable variation of the floral diagram may be found.

There is, in fact, no 'royal road' to the estimation of the relative importance of characters; the same character which is of the greatest value in one group may be trivial in another; and this holds good equally whether the character be drawn from the external morphology or from the internal structure.

Our knowledge of the comparative anatomy of plants, from this point of view, is still very backward, and it is quite possible that the introduction of such charac-ters into the ordinary work of the Herbarium may be premature; certainly it must be conducted with the greatest judgment and caution. We have not yet got our data, but every encouragement should be given to the collection of such data, so that our classification in the future may rest on the broad foundation of a com-parison of the entire structure of plants.

In estimating the relative importance of characters of different kinds we must not forget that characters are often most constant when most adaptive. Thus, as Professor Trail informs me, the immense variability of the flowers of *Polygonum* goes together with their simple method of self-fertilisation. The exact arrange-ment is of little importance to the plant, and so variation goes on unchecked. In flowers with accurate adaptation to fertilisation by insects such variability is not found, for any change which would disturb the perfection of the mechanism is at once eliminated by natural selection.

### HISTOLOGY.

I propose to say but little on questions of minute histology, a subject which lies on the borderland between morphology and physiology, and which will be dealt with next Tuesday far more competently than I could hope to treat it. Last year my predecessor in the presidency of this Section spoke of a histological dis-covery (that of the nucleus, by Robert Brown) as 'the most epoch-making of events' in the modern history of botany. The histological questions before us at the present day may be of no less importance, but we cannot as yet see them in proper perspective. The centrosomes, those mysterious protoplasmic particles which have been supposed to preside over the division of the nucleus, and thus to determine the plane of segmentation, if really permanent organs of the cell, would have to rank as co-equal with the nucleus itself. If, on the other hand, as some think, they are not constant morphological entities, but at most temporary structures differentiated *ad hoc*, then we are brought face to face with the question whether the causes of nuclear division lie in the nucleus itself or in the surrounding protoplasm.

Nothing can be more fascinating than such problems, and nothing more difficult. We have, at any rate, reason to congratulate ourselves that English botanists are no longer neglecting the study of the nucleus and its relation to the cell. For a long time little was done in these subjects in our country, or at least little was published, and botanists were generally content to take their information from abroad, not going beyond a mere verification of other men's results. Now we have changed all that, as the communications to this Section sufficiently testify.

Nothing is more remarkable in histology than the detailed agreement in the structure and behaviour of the nucleus in the higher plants and the higher

animals, an agreement which is conspicuously manifest in those special divisions which take place during the maturation of the sexual cells. Is this striking agreement the product of inheritance from common ancestors, or is the parallelism dependent solely on similar physical conditions in the cells? This is one of the great questions upon which we may hope for new light from the histological discussion next week.

## ALTERNATION OF GENERATIONS.

We have known ever since the great discoveries of Hofmeister that the development of a large part of the vegetable kingdom involves a regular alternation of two distinct generations, the one, which is sexual, being constantly succeeded—so far as the normal cycle is concerned—by the other which is asexual. This alternation is most marked in the mosses and ferns, taking these words in their widest sense, as used by Professor Campbell in his recent excellent book. In the Bryophyta, the ordinary moss or liverwort plant is the sexual generation, producing the ovum, which, when fertilised, gives rise to the moss-fruit, which here alone represents the asexual stage. The latter forms spores from which the sexual plant is again developed.

In the Pteridophyta the alternation is equally regular, but the relative development of the two generations is totally different, the sexual form being the insignificant prothallus, while the whole fern-plant, as we ordinarily know it, is the asexual generation.

The thallus of some of the lower Bryophyta is quite comparable with the prothallus of a fern, so as regards the sexual generation there is no difficulty in seeing the relation of the two classes; but when we come to the asexual generation or sporophyte the case is totally different. There is no appreciable resemblance between the fruit of any of the Bryophyta and the plant of any vascular Cryptogam.

There is thus a great gap within the Archegoniatæ; there is another at the base of the series, for the regular alternation of the Bryophyta is missing in the Algæ and Fungi, and the question as to what corresponds among these lower groups to the sporophyte and oophyte of the higher Cryptogams is still disputed.

Now as regards this life-cycle, which is characteristic of all plants higher than Algæ and Fungi, there are two great questions at present open. The one is general: are the two generations, the sporophyte and the oòphyte, homologous with one another, or is the sporophyte a new formation intercalated in the life-history, and not comparable to the sexual plant? The former kind of alternation has been called homologous, the latter antithetic. This question involves the *origin* of alternation; its solution would help us to bridge over the gap between the Archegoniatæ and the lower plants. The second problem is more special: has the sporophyte of the Pteridophyta, which always appears as a complete plant, been derived from the simple and totally different sporophyte of the Bryophyta, or are the two of distinct origin?

At present it is usual, at any rate in England, to assume the antithetic theory of alternation. Professor Bower, its chief exponent, says:[1] 'It will also be assumed that, whatever may have been the circumstances which led to it, antithetic alternation was brought about by elaboration of the zygote [*i.e.* the fertilised ovum] so as to form a new generation (the sporophyte) interpolated between successive gametophytes, and that the neutral generation is not in any sense the result of modification or metamorphosis of the sexual, but a new product having a distinct phylogenetic history of its own.' In his essay on 'Antithetic as distinguished from Homologous Alternation of Generations in Plants,'[2] the author describes the hypothetical first appearance of the sporophyte as follows: 'Once fertilised, a zygote might in these plants [the first land plants] divide up into a number of portions (carpospores), each of which would then serve as a starting-point of a new individual.'

[1] 'Spore-producing Members,' *Phil. Trans.* vol. clxxxv. B. (1894) p. 473.
[2] *Annals of Botany*, vol. iv. (1890), p. 362.

On this view, the sporophyte first appeared as a mere group of spores formed by the division of the fertilised ovum. Consequently the inference is drawn that all the vegetative parts of the sporophyte have arisen by the 'sterilisation of potentially sporogenous tissue.' That is to say, there was nothing but a mass of spores to start with, so whatever other tissues and organs the sporophyte may form must be derived from the conversion of spore-forming cells into vegetative cells. Professor Bower has worked out this view most thoroughly, and as the result he is not only giving us the most complete account of the development of sporangia which we have ever had, but he has also done much to clear up our ideas, and to show us what the course of evolution ought to have been if the assumptions required by the antithetic theory were justified.

Without entering into any detailed criticism of this important contribution to morphology, which is still in progress, I wish to point that we are not, after all, bound to accept the assumption on which the theory rests. There is another view in the field, for which, in my opinion, much is to be said. The antithetic theory is receiving a most severe test at the friendly hands of its chief advocate Should it break down under the strain we need not despair, for another hypothesis remains which I think quite equally worthy of verification.

This is the theory of Pringsheim, according to which the two generations are *homologous* one with another, the oophyte corresponding to a sexual individual among Thallophytes, the sporophyte to an asexual individual. To quote Pringsheim's own words:[1] 'The alternation of generations in mosses is immediately related to those phenomena of the succession of free generations in Thallophytes, of which the one represents the neutral, the other the sexual plant.' Further on [2] he illustrates this by saying: 'The moss sporogonium stands in about the same relation to the moss plant as the sporangium-bearing specimens of *Saprolegnia* stand to those which bear oogonia, or as, among the Florideæ, the specimens with tetraspores are related to those with cystocarps.' This gets rid of the intercalation of a new generation altogether; we only require the modification of the already existing sexual and asexual forms of the Thallophytes.

The sudden appearance of something completely new in the life-history, as required by the antithetic theory, has, to my mind, a certain improbability. *Ex nihilo nihil fit.* We are not accustomed in natural history to see brand-new structures appearing, like morphological Melchizedeks, without father or mother. Nature is conservative, and when a new organ is to be formed it is, as every one knows, almost always fashioned out of some pre-existing organ. Hence I feel a certain difficulty in accepting the doctrine of the appearance of an intercalated sporophyte by a kind of special creation.

We can have no direct knowledge of the origin of the sporophyte in the Bryophyta themselves, for the stages, whatever they may have been, are hopelessly lost. In some of the Algæ, however, we find what most botanists recognise as at least a parallel development, even if not phylogenetically identical.[3] In *Œdogonium*, for example, the oöspore does not at once germinate into a new plant, but divides up into four active zoospores, which swim about and then germinate. In *Coleochæte* the oospore actually becomes partitioned up by cell-walls into a little mass of tissue, each cell of which then gives rise to a zoospore.

In both these genera (and many more might be added) the cell-formation in the germinating oöspore has been generally regarded as representing the formation of a rudimentary sporophyte generation. If we are to apply the antithetic theory of alternation to these cases, we must assume that the zoospores produced on germination are a new formation, intercalated at this point of the life-cycle. But is this assumption borne out by the facts? I think not. In reality nothing new is intercalated at all. The 'zoospores' formed from the oöspore on germination are identical with the so-called 'zoogonidia,' formed on the ordinary vegetative plant at all stages of its growth.

In science, as in every subject, we too easily become the slaves of language.

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[1] *Gesammelte Abhandlungen*, II. p. 370.                    [2] *Ibid.* p. 371.
[3] See Bower, *Antithetic Alternation*, p. 361.

By giving things different names we do not prove that the things themselves are different. In this case, for example, the multiplication of terms serves, in my opinion, merely to disguise the facts. The reproductive cells produced by the ordinary plant of an *Œdogonium* are identical in development, structure, behaviour, and germination with those produced by the oöspore. The term 'zoogonidia' applied to the former is a 'question-begging epithet,' for it assumes that they are not homologous with the 'zoospores' produced by the latter. I prefer to keep the old name zoospore for both, as they are identical bodies.

To my mind the point seems to be this. An *Œdogonium* (to keep to this example) can form zoospores at any stage of its development; there is one particular stage, however, at which they are *always* formed—namely, on the germination of the oöspore. Nothing new is intercalated, but the irregular and indefinite succession of sexual and asexual acts of reproduction is here tending to become regular and definite.

In *Sphæroplea*, as was well pointed out by the late Mr. Vaizey,[1] though his view of alternation was very different from that which I am now putting forward, the alternation is as definite as in a moss, for here, so far as we know, zoospores are only formed on the germination of the fertilised ovum. If *Sphæroplea* stood alone we might believe in the intercalation of these zoospores, as a new stage, but the comparison with *Ulothrix*, *Œdogonium*, *Bulbochæte* and *Coleochæte* shows, I think, where they came from.

The body formed from the oospore is called by Pringsheim the first neutral generation. In *Œdogonium* this has no vegetative development, for the first thing that the oospore does is to form the asexual zoospores, and it is completely used up in the process. In other cases it is not in quite such a hurry, and here the first neutral generation has time to show itself as an actual plant. This is so in *Ulothrix*, a much more primitive form than *Œdogonium*, for its sexuality is not yet completely fixed. Here the zygospore actually germinates, forming a dwarf plant, and in this stage passes through the dull season, producing zoospores when the weather becomes more favourable. On Pringsheim's view the dwarf plant is not a new creation, but just a rudimentary *Ulothrix*, which soon passes on to spore-formation. So, too, with the cellular body formed on the germination of the oospore of *Coleochæte*; this also is looked upon as a reduced form of thallus. On any view this genus is especially interesting, for the sporophyte remains enclosed by the tissue of the sexual generation, thus offering a striking analogy with the Bryophyta.

In the Phycomycetous Fungi—plants which have lost their chlorophyll, but which otherwise in many cases scarcely differ from Algæ—the oöspore in one and the same species may either form a normal mycelium, or a rudimentary mycelium bearing a sporangium, or may itself turn at once into a sporangium (producing zoospores) without any vegetative development. Here it seems certain that Pringsheim's view is the right one, for all stages in the reduction of the first neutral generation lie before our eyes. Nowhere, either here or among the green Algæ, do I see any evidence for the intercalation of a *new* generation or a new form of spore on the germination of the fertilised ovum.

Pringsheim extends the same view to the higher plants. The sporogonium of a moss is for him the highly modified first neutral generation, homologous with the vegetative plant, but here specially adapted for spore-formation. I have elsewhere pointed out[2] that this view has great advantages, for not only does it harmonise exactly with the actual facts observed in the green Algæ and their allies, but it also helps us to understand the astoundingly different forms which the archegoniate sporophyte may assume

It seems to me that Pringsheim was right in regarding the fruit-formation of Florideæ as totally different from the sporophyte-formation of *Coleochæte* or the Bryophyta. The cystocarp bears none of the marks of a distinct generation, for throughout its whole development it remains in the most complete organic connec-

[1] *Annals of Botany*, vol. iv., p. 373.
[2] *Nature*, February 21, 1895.

tion with the thallus that bears it. The whole Floridean process, often so com-
plicated, appears to be an arrangement for effecting the fertilisation of many
female cells as the result of an original impregnation by a single sperm-cell.
There is here still a great field for future research; but in the light of our present
knowledge there seems to be no real parallelism with the formation of a sporophyte
in the higher plants.

The gap between the Bryophyta and the Algæ remains, unfortunately, a wide
and deep one, and it is not probable that any Algæ at present known to us lie at
all near the line of descent of the higher Cryptogams. *Riccia* is often compared
with *Coleochæte*, but it is by no means evident that *Riccia* is a specially primitive
form. In *Anthoceros*, which bears some marks of an archaic character, the sporo-
phyte is relatively well developed. To those who do not accept the theory of
intercalation it is not necessary to assume that the most primitive Bryophyta must
have the most rudimentary sporophyte.

Apart from other differences, Bryophyta differ from most green Algæ in the
fact that asexual spores are *only found* in the generation succeeding fertilisation.
The spores moreover are themselves quite different from anything in Algæ, and
the constancy of their formation in fours among all the higher plants from the
liverworts upwards, is a fact which requires explanation. I should like to sug-
gest to some energetic histologist a comparison of the details of spore-formation
in the lower liverworts and in the various groups of Algæ, especially those of the
green series. It is possible that some light might be thus thrown on the origin of
tetrad-spore-formation, a subject as to which Professor Farmer has already gained
some very remarkable results. On Pringsheim's view some indications of homo-
logy between bryophytic and algal spore-formation might be expected, and any-
how the tetrads require *some* explanation.

The peculiarities of the sporophyte in the Archegoniatæ, as compared with any
algal structures, depend, no doubt, on the acquirement of a terrestrial habit, while
the oöphyte by its mode of fertilisation remains 'tied down to a semi-aquatic life.'[1]
Professor Bower's phrase 'amphibious alternation' expresses this view of the case
very happily, and indeed his whole account of the rise of the sporophyte is of the
highest value, even though we may not accept his assumption as to its origin
*de novo*.

I attach special weight to Professor Bower's treatment of this subject,
because he has shown how the most important of all morphological phenomena
in plants, namely the alternation of generations in Archegoniatæ, may be explained
as purely adaptive in origin. All Darwinians owe him a debt of gratitude for
this demonstration, which holds good even if we believe the sporophyte to be the
modification of a pre-existing body, and not a new formation.

### APOSPORY AND APOGAMY.

We must remember that the theory of homologous alternation has twice
received the strongest confirmation of which a scientific hypothesis is susceptible—
that of verified prediction. In both cases Pringsheim was the happy prophet.
Convinced on structural grounds of the homology of the two generations in
mosses, he undertook his experiments on the moss-fruits, in the hope, as he says,[2] that
he would succeed in producing protonema from the subdivided seta of the mosses,
and thus prove the *morphological* agreement of seta and moss-stem. His experi-
ment, as everybody knows, was completely successful, and resulted in the first
observed cases of *apospory*, *i.e.* the direct outgrowth of the sexual from the asexual
generation

Here he furnished his own verification; in the second case it has come from
other hands. In the paper of 1877, so often referred to, he says (p. 391): 'Here,
however [*i.e.* in the ferns], the act of generation, that is, the formation of sexual
organs and the origin of an embryo, is undoubtedly bound up with the existence
of the spore, *until those future ferns are found* which I indicated as conceivable in

[1] Bower, *Antithetic Alternation*.
[2] *Ges. Abh.* II. p. 407.

my preliminary notice, *in which the prothallus will sprout forth directly from the frond.*'

It is unnecessary to remind English botanists that Pringsheim's hypothetical aposporous ferns are now perfectly well known in the flesh; such cases having been first observed by Mr. Druery and then fully investigated by Professor Bower.

A very remarkable case of direct origin of the oöphyte from the sporophyte has lately been described by Mr. E. J. Lowe, in a variety of *Scolopendrium vulgare.* Here the young fern-plant produced prothalli bearing archegonia as direct outgrowths from its second or third frond. The specimen had a remarkable history, for the young plants were produced from portions of a prothallus which had been kept alive and repeatedly subdivided during a period of no less than eight years. I cannot go into the interesting details here, they will be published elsewhere; but I wish to call attention to the fact that in this case the production of the sexual from the asexual generation, occurring so early in life, has no obvious relation to suppressed spore-formation, and so appears to differ essentially from the cases first described, which occurred on mature plants. I believe Mr. Lowe's case is not an altogether isolated one.

The converse phenomenon—that of apogamy—or the direct origin of an asexual plant from the prothallus without the intervention of sexual organs, has now been observed in a considerable number of ferns, the examples already known belonging to no less than four distinct families· Polypodiaceæ, Parkeriaceæ, Osmundaceæ, and Hymenophyllaceæ. In *Trichomanes alatum* Professor Bower found that apospory and apogamy co-exist in the same plant, the sporophyte directly giving rise to a prothallus, which again directly grows out into a sporophyte; the life-cycle is thus completed without the aid either of spores or of sexual organs. Dr. W. H. Lang who has recently made many interesting observations on apogamy, will, I am glad to say, read a paper on the subject before this section, so I need say no more.

I must, however, express my own conviction that the facility with which, in ferns, the one generation may pass over into the other by vegetative growth, and that in both directions, is a most significant fact. It shows that there is no such hard and fast distinction between the generations as the antithetic theory would appear to demand, and in my opinion weighs heavily on the side of the homology of sporophyte and oöphyte. I cannot but think that the phenomena deserve greater attention from this point of view than they have yet received.

A mode of growth which affords a perfectly efficient means of abundant propagation cannot, I think, be dismissed as merely teratological.

Since the foregoing paragraph was first written Dr. Lang has made the remarkable discovery (already communicated to the Royal Society) that in a *Lastræa* sporangia of normal structure are produced on the prothallus itself, side by side with normal archegonia and antheridia. I cannot forbear mentioning this striking observation, of which we shall hear an account from the discoverer himself.

The strongest advocate of the homology of the prothallus with the fern plant could scarcely have ventured to anticipate such a discovery.

RELATION BETWEEN MOSSES AND FERNS.

Goebel said, in 1882: 'The gap between the Bryophyta and the Pteridophyta is the deepest known to us in the vegetable kingdom. We must seek the starting-point of the Pteridophyta elsewhere than among the Muscineæ: among forms which may have been similar to liverworts, but in which the asexual generations entered from the first on a different course of development.'[1] I cannot help feeling that all the work which has been done since goes to confirm this wise conclusion. Attempts have been made in the most sportsmanlike manner (to adopt a phrase of Professor Bower's) to effect a passage over the gulf, but the gulf is still unbridged. I cannot see anywhere the slightest indication of anything like an intermediate form between the spore-bearing plant of the Pteridophyta and the spore-bearing

Schenk's *Handbuch der Botanik*, vol. ii. p. 401

fruit of the Bryophyta. The plant of the Pteridophyta is sometimes small and simple, but the smallest and simplest seem just as unlike a bryophytic sporogonium as the largest and most complex. On the side of the moss group, *Anthoceros* has been often cited as a form showing a certain approach towards the Pteridophytes, and Professor Campbell in particular has developed this idea with remarkable ingenuity. An unprejudiced comparison, however, seems to me to show nothing more here than a very remote parallelism, not suggestive of affinity.

There is no reason to believe that the Bryophyta, as we know them, were the precursors of the vascular Cryptogams at all. There is a remarkable paucity of evidence for the geological antiquity of Bryophyta, though many of the mosses at any rate would seem likely to have been preserved if they existed. Brongniart said, in 1849, ' The rarity of fossil mosses, and their complete absence up to now in the ancient strata, are among the most singular facts in geological botany ; ' [2] and since that time it is wonderful how little has been added. Things seem to point to both Pteridophyta and Bryophyta having had their origin far back among some unknown tribes of the Algæ. If we accept the homologous theory of alternation, we may fairly suppose that the sporophyte of the earliest Pteridophyta always possessed vegetative organs of some kind. The resemblance between the young sporophyte and the prothallus in some lycopods indicates that at some remote period the two generations may not have been very dissimilar. At least some such idea gives more satisfaction to my mind than the attempt to conceive of a fern-plant as derived from a sterilised group of potential spores.

The Bryophyta may have had from the first a more reduced sporophyte, the first neutral generation having, in their ancestors, become more exclusively adapted to spore-producing functions. I must not omit to mention the idea that the Bryophyta, or at any rate the true mosses, are degenerate descendants of higher forms. The presence of typical stomata on the capsule in some cases, and of somewhat reduced stomata in others, has been urged in support of this view. It is possible ; but if so, from what have these plants been reduced ?

Few people, perhaps, fully realise how absolutely insoluble such a problem as we have been discussing really is. I say nothing as to the mosses, which *may* have arisen relatively late in geological history. The Pteridophyta, at any rate, are known to be of inconceivable antiquity. Not only did they exist in greater development than at present in the far-off Devonian period, but at that time they were already accompanied by highly organised gymnospermous flowering-plants. Probably we are all agreed that Gymnosperms arose somehow from the vascular Cryptogams. Hence, in the Devonian epoch, there had already been time not only for the Pteridophyta themselves to attain their full development, but for certain among them to become modified into complex Phanerogams. It would not be a rash assumption that the origin of the Pteridophyta took place as long before the period represented by the plant-bearing Devonian strata as that period is before our own day. Can we hope that a mystery buried so far back in the dumb past will be revealed ?

It will be understood that I do not wish to assume the *rôle* of partisan for the homologous theory of alternation. Possibly the whole question lies beyond human ken, and partisanship would be ridiculous. But I do wish to raise a protest against anything like a dogmatic statement that alternation of generations *must have been* the result of the interpolation of a new stage in the life-history. ' Let us, in the presence of the greatest mystery in the morphology of plants, at least keep an open mind, and not tie ourselves down to assumptions, though we may use them as working hypotheses.

### HISTOLOGICAL CHARACTERS OF THE TWO GENERATIONS.

There is one histological question upon which I must briefly touch because it bears directly on the subject which we have been considering. I shall say very little, however, in view of the discussion next Tuesday.

[1] *Tableau des Genres de Végétaux Fossiles*, p. 13.

It is now well known that in animals and in the higher plants a remarkable numerical change takes place in the constituents of the nucleus shortly before the act of fertilisation. The change consists in the halving of the number of chromo-somes, those rod-like bodies which form the essential part of the nucleus, and are regarded by Weismann and most biologists as the bearers of hereditary qualities. Thus in the lily the number of chromosomes in the nuclei of vegetative cells is twenty-four; in the sexual nuclei, those of the male generative cell and of the ovum, the number is twelve. When the sexual act is accomplished the two nuclei unite, and so the full number is restored and persists throughout the vegetative life of the next generation. The absolute figures are of course of no importance; the point is, the reduction to one half during the maturation of the sexual cells, and the subsequent restoration of the full number when their union takes place. I say nothing as to the details or the significance of the process, points which have been fully dealt with elsewhere, notably in an elaborate recent paper by Miss E. Sargant.

Now, in animals (so far as I am aware) and in angiospermous plants the reduc-tion of the chromosomes takes place very shortly before the differentiation of the sexual cells. Thus in a lily the reduction takes place on the male side immediately prior to the first division of the pollen mother-cell, so that four cell-divisions in all intervene between the reduction and the final differentiation of the male generative cells. On the female side the reduction in the same plant takes place in the primary nucleus of the embryo-sac, so that here there are three divisions between the reduction and the formation of the ovum. I believe these facts agree very closely with those observed in the animal kingdom, and so far there is no par-ticular difficulty, for we can easily understand that if the number of chromosomes is to be kept constant from one generation to another, then the doubling involved in sexual fusion must necessarily be balanced by a halving.

There are, however, a certain number of observations on Gymnosperms and archegoniate Cryptogams which appear to put the matter in a different light. Overton [1] first showed that in a Cycad, *Ceratozamia*, the nuclei of the prothallus or endosperm all have the half-number of chromosomes. Here then the reduction takes place in the embryo sac (or rather its mother-cell), but a great number of cell-generations intervene between the reduction and the maturation of the ovum. In fact the whole female oöphyte shows the reduced number, while the sporophyte has the full number. The reduction takes place also in the pollen mother-cell. Further observations have extended this conclusion to some other Gymnosperms.

In *Osmunda* among the ferns there is evidence to show that reduction takes place in the spore mother-cell, and that the sexual generation has the half-number throughout. Professor Farmer has found the same thing in various liverworts, and shown that the reduction of chromosomes takes place in the spore mother-cell; and his observations of cell-division in the two generations have afforded some direct evidence that the oophyte has the half-number and the sporophyte the full number throughout. Professor Strasburger fully discussed this subject before Section D at Oxford,[2] and came to the conclusion that the difference in number of chromosomes is a difference between the two generations as such, the sexual generation being characterised by the half-number, the asexual by the full number.

The importance of this conception for the morphologist is that an actual histological difference appears to be established between the two generations, a fact which would appear to militate against their homology. Some botanists even go so far as to propose making the number of chromosomes the criterion by which the two generations are to be distinguished. Considering that the whole theory rests at present on but few observations, I venture to think this both premature and objectionable ; for nothing can be worse for the true progress of science than to rush hastily to deductive reasoning from imperfectly established premises.

The facts are certainly very difficult to interpret. Those who accept the antithetic theory of alternation suppose the sexual generation to be the older, and

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[1] *Annals of Botany*, vol. vii. p 139.
[2] See *Annals of Botany*, vol. viii. p. 281.

that in Thallophytes the plant is always an oöphyte, whether 'actual' or 'potential.' Hence they believe that in Thallophytes the plant should show throughout the reduced number of chromosomes, reduction hypothetically taking place immediately upon the germination of the oöspore. If this were true it would lend some support to the idea of the intercalation of the sporophyte, but at present there is not the slightest evidence for these assumptions. On the contrary, in the only Thallophyte in which chromosome-counting has been successfully accomplished (*Fucus*) Professor Farmer and Mr. Williams find exactly the reverse; the plant has throughout the *full* number of chromosomes; reduction first takes place in the oögonium, immediately before the maturation of the ova, and on sexual fusion the full number is restored, to persist throughout the vegetative life of the plant. *Fucus* is, no doubt, a long way off the direct line of descent of Archegoniatæ, but still it is a striking fact that the only direct evidence we have goes dead against the idea that the sexual generation (and who could call a Fucus-plant anything else but sexual?) necessarily has the reduced number of chromosomes. This fact is indeed a rude rebuff to deductive morphology.

I am disposed to regard the different number of chromosomes in the two generations observed in certain cases among Archegoniatæ not as a primitive but as an acquired phenomenon, perhaps correlated with the definiteness of alternation in the Archegoniatæ as contrasted with its indefiniteness in Thallophytes. In *Fucus*, in flowering plants, and in animals the *soma* or vegetative body has the full number of chromosomes. With these the sporophyte of the Archegoniatæ agrees; it is the oöphyte which appears to be peculiar in possessing the half-number, so that if the evidence points to intercalation at all, it would seem to suggest that the oöphyte is the intercalated generation—obviously a *reductio ad absurdum*. I do not think we are as yet in a position to draw any morphological conclusions from these minute histological differences, interesting as they are.

The question how the number of chromosomes is kept right in cases of apospory and of apogamy is obviously one of great interest, and I am glad to say that it is receiving attention from competent observers.

## SEXUALITY OF FUNGI.

Only a few years ago De Bary's opinion that the fruit of the ascus-bearing Fungi is normally the result of an act of fertilisation was almost universally accepted, especially in this country. Although the presence of sexual organs had only been recorded in comparatively few cases, and the evidence for their functional activity was even more limited, yet the conviction prevailed that the ascocarp is at least the homologue of a sexually produced fruit. The organ giving rise to the ascus or asci was looked upon as homologous with the oögonium of the Peronosporeæ, the supposed fertilising organ either taking the form of an antheridial branch as in that group, or, as observed by Stahl in the lichen *Collema*, giving rise to distinct male cells, or spermatia. More recently there has been a complete revolution of opinion on this point, and a year ago or less most botanists probably agreed that the question of the sexuality of the Ascomycetes had been settled in a negative sense. This change was due, in the first place, to the influence of Brefeld, who showed, in a great number of laborious investigations, that the ascus-fruit may develop without the presence of anything like sexual organs; while Moller proved that the supposed male cells of lichens are in a multitude of cases nothing but conidia, capable of independent germination.

The view thus gained ground that all the higher Fungi are asexual plants, fertilisation only occurring in the lower forms, such as the Peronosporeæ and Mucorineæ, which have not diverged far from the algal stock. The ascus, in particular, is regarded by this school as homologous with the asexual sporangium of a *Mucor*. This theory has been brilliantly expounded in a remarkable book by Von Tavel, which we cannot but admire as a model of clear morphological reasoning, whether its conclusions be ultimately adopted or not.

Still, it must be admitted that the Brefeld school were rather apt to ignore

such pieces of evidence as militated against their views, and consequently their position was insecure so long as these hostile posts were left uncaptured.

Quite recently the whole question has been reopened by the striking observations of Mr. Harper, an American botanist working at Bonn.

Zopf, in 1890,[1] pointed out that up to that time it had not been possible in any Ascomycete to demonstrate a true process of fertilisation by strictly scientific evidence, namely, by observing the fusion of the nuclei of the male and female elements. Exactly the proof demanded has now been afforded by Mr. Harper's observations, for in a simple Ascomycete, *Sphærotheca castagnei*, the parasite causing the hop-mildew, he has demonstrated in a manner which appears to be conclusive the fusion of the nucleus of the antheridium with that of the ascogonium.[2] It is impossible to evade the force of this evidence, for the fungus in question is a perfectly typical Ascomycete, though exceptionally simple, in so far as only a single ascus is normally produced from the ascogonium. It is unnecessary to point out how important it is that Mr. Harper's observations should be confirmed and extended to other and more complex members of the order. In the mean time the few who (unlike your President) had not bowed the knee to Brefeld may rejoice !

It is impossible to pursue the various questions which press upon one's mind in considering the morphology of the Fungi. The occurrence not only of cell-fusion, but of nuclear fusion, apart from any definite sexual process, now recorded in several groups of Fungi, urgently demands further inquiry. Such unions of nuclei have been observed in the basidia of Agarics, the teleutospores of Uredineæ, and even in the asci of the Ascomycetes. That such a fusion is not necessarily, as Dangeard[3] has supposed, of a sexual nature, seems to be proved by the fact that it occurs in the young ascus of *Sphærotheca* long after the true act of fertilisation has been accomplished. It is possible, however, that these phenomena may throw an important side-light on the significance of the sexual act itself.

Another question which is obviously opened up by the new results is that of the homologies of the ascus. The observations of Lagerheim[4] on *Dipodascus* point to the sexual origin of a many-spored sporangium not definitely characterised as an ascus. On the other hand, not only sporangia, but true asci are known to arise in a multitude of cases direct from the mycelium. It is of course possible that as regards the asci these are cases of reduction or apogamy ; on the other hand, it is not wholly impossible that the asci may turn out to be really homologous with a sexual sporangia, even though their development may often have become associated with the occurrence of a sexual act. However this may be, there is at present no reason to doubt that a very large proportion of the Fungi are, at least functionally,

## CHALAZOGAMY.

Among the most striking results of recent years bearing on the morphology of the higher plants, Treub's discovery of the structure of the ovule and the mode of fertilisation in *Casuarina* must undoubtedly be reckoned. The fact that the pollen-tube in this genus does not enter the micropyle, but travels through the tissues of the ovary to the chalaza, thus reaching the base of the embryo-sac, was remarkable enough in itself, and when considered in connection with the presence of a large sporogenous tissue producing numerous embryo-sacs, appeared to justify the separation of this order from other angiosperms. Then came the work of Miss Benson in England, and of Nawaschin in Russia, showing that these remarkable peculiarities are by no means confined to *Casuarina,* but extend also in various modifications to several genera of the Cupuliferæ and Ulmaceæ. They are not, however, constant throughout these families, so that we are no longer able to attach to these characters the same fundamental systematic importance which their first discoverer attributed to them. It is remarkable, however, that these

[1] 'Die Pilze,' *Schenk's Handbuch der Botanik*, Bd. iv. p. 341.
[2] *Berichte der deutschen bot. Gesellschaft*, vol. xiii., January 29, 1896.
[3] *Le Botaniste*, vols. iv. and v.
[4] *Pringsheim's Jahrbuch f. Wiss. Bot.* 1892.

departures from the ordinary course of angiospermous development occur in families some of which have been believed on other grounds to be among the most primitive Dicotyledons.

### Evidence of Descent derived from Fossil Botany.

At the beginning of this Address I spoke of the importance of the comparatively direct evidence afforded by fossil remains as to the past history of plants. It may be of interest if I endeavour to indicate the directions in which such evidence seems at present to point.

It was Brongniart who in 1828 first arrived at the great generalisation that 'nearly all of the plants living at the most ancient geological epochs were Cryptogams,'[1] a discovery of unsurpassed importance for the theory of evolution, though one which is now so familiar that we almost take it for granted. Those palæozoic plants which are not Cryptogams are Gymnosperms, for the angiospermous flowering plants only make their appearance high up in the secondary rocks. Even the Wealden flora, recently so carefully described by Mr. Seward, one of the secretaries to this section, has as yet yielded no remains referable to Angiosperms, though this is about the horizon at which we may expect their earliest trace to be found.

Attention has already been called to the enormous antiquity of the higher Cryptogams—the Pteridophyta—and to the striking fact that they are accompanied, in the earliest strata in which they have been demonstrated with certainty, by well-characterised Gymnosperms. The Devonian flora, so far as we know it, though an early, was by no means a primitive one, and the same statement applies still more strongly to the plants of the succeeding Carboniferous epoch. The palæozoic Cryptogams, as is now well known, being the dominant plants of their time, were in many ways far more highly developed than those of our own age; and this is true of all the three existing stocks of Pteridophyta, Ferns, Lycopods, and Equisetineæ.

We cannot therefore expect any *direct* evidence as to the origin of these groups from the palæozoic remains at present known to us, though it is, of course, quite possible that the plants in question have sometimes retained certain primitive characters, while reaching in other respects a high development. For example, the general type of anatomical structure in the young stems of the Lepidodendreæ was simpler than that of most Lycopods at the present day, though in the older trunks the secondary growth, correlated with arborescent habit, produced a high degree of complexity. On the whole, however, the interest of the palæozoic Cryptogams does not consist in the revelation of their primitive ancestral forms, but rather in their enabling us to trace certain lines of evolution further upward than in recent plants. From the Carboniferous rocks we first learn what Cryptogams are capable of. In descending to the early strata we do not necessarily trace the trunk of the genealogical tree to its base; on the contrary, we often light on the ultimate twigs of extensive branches which died out long before our own period.

In a lecture which I had the honour of giving last May before the Liverpool Biological Society, I pointed out how futile the search for 'missing links' among fossil plants is likely to be. The lines of descent must have been so infinitely complex in their ramification that the chances are almost hopelessly great against our happening upon the direct ancestors of living forms. Among the collateral lines, however, we may find invaluable indications of the course of descent.

Fossil botany has revealed to us the existence in the Carboniferous epoch of a fourth phylum of vascular Cryptogams quite distinct from the three which have come down—more or less reduced—to our own day. This is the group of Sphenophylleæ, plants with slender ribbed stems, superposed whorls of more or less wedge-shaped leaves, and very complex strobili with stalked sporangia. The group to a certain extent combines the characters of Lycopods and Horsetails, resembling the former in the primary anatomy, and the latter, though remotely, in external habit and fructification. Like so many of the early Cryptogams, *Spheno-*

[1] Williamson, *Reminiscences of a Yorkshire Naturalist*, 1896, p. 198.

*phyllum* possessed well-marked cambial growth. One may hazard the guess that this interesting group may have been derived from some unknown form lying at the root of both Calamites and Lycopods. The existence of the Sphenophylleæ certainly suggests the probability of a common origin for these two series.

In few respects is the progress made recently in fossil botany more marked than in our knowledge of the affinities of the Calamarieæ. Even so recently as the publication of Count Solms-Laubach's unrivalled introduction to 'Fossil Botany,' the relation of this family to the Horsetails was still so doubtful that the author dealt with the two groups in quite different parts of his book. This is never likely to happen again. The study of vegetative anatomy and morphology on the one hand, and of the perfectly preserved fructifications on the other, can leave no doubt that the fossil Calamarieæ and the recent Equiseta belong to one and the same great family, of which the palæozoic representatives are, generally speaking, by far the more highly organised This is not only true of their anatomy, which is characterised by secondary growth in thickness just like that of a Gymnosperm, but also applies to the reproductive organs, some of which are distinctly heterosporous. In the genus *Calamostachys* we are, I think, able to trace the first rise of this phenomenon.

The external morphology of the cones is also more varied and usually more complex than that of recent Equiseta, though in some Carboniferous forms, as in the so-called *Calamostachys tenuissima* of Grand' Eury, we find an exactly Equisetum-like arrangement.

The position of the Sigillariæ as true members of the Lycopod group is now well established. The work of Williamson proved that there is no fundamental distinction between the vegetative structure of *Lepidodendron*, which has always been recognised as lycopodiaceous, and that of *Sigillaria*. Secondary growth in thickness, the character which here, as in the case of the Calamodendreæ, misled Brongniart, is the common property of both genera. Then came Zeiller's discovery of the cones of *Sigillaria*, settling beyond a doubt that they are heterosporous Cryptogams. A great deal still remains to be done, more especially as to the relation of *Stigmaria* to the various types of lycopodiaceous stem. At present we are perhaps too facile in accepting *Stigmaria ficoides* as representing the underground organs of almost any carboniferous Lycopod.

We are now in possession of a magnificent mass of data for the morphology of the palæozoic lycopods, and have perhaps hardly yet realised the richness of our material. I refer more especially to specimens with structure, on which, here as elsewhere, the scientific knowledge of fossil plants primarily depends.

It is scarcely necessary to repeat what has been said so often elsewhere, that the now almost universal recognition of the cryptogamic nature of Calamodendreæ and Sigillariæ is a splendid triumph for the opinions of the late Professor Williamson, which he gallantly maintained through a quarter of a century of controversy.

Perhaps, however, the keenest interest now centres in the Ferns and fern-like plants of the carboniferous epoch. No fossil remains of plants are more abundant, or more familiar to collectors, than the beautiful and varied fern-fronds from the older strata. The mere form, and even the venation of these fronds, however, really tell us little, for we know how deceptive such characters may be among recent plants. In a certain number of cases, discovery of the fructification has come to our aid, and where sori are found we can have no more doubt as to the specimens belonging to true Ferns. The work of Stur and Zeiller has been especially valuable in this direction, and has revealed the interesting fact that a great many of these early Ferns showed forms of fructification now limited to the small order Marattiaceæ. I think perhaps the predominance of this group has been somewhat exaggerated, but at least there is no doubt that the marattiaceous type was much more important then than now, though it by no means stood alone. In certain cases the whole fern-plant can be built up. Thus Zeiller and Renault have shown that the great stems known as *Psaronius*, the structure of which is perfectly preserved, bore fronds of the *Pecopteris* form, and that similar *Pecopteris* fronds produced the fructification of *Asterotheca*, which is of a marat-

tiaceous character.   Hence, for a good many Carboniferous and Permian forms there is not the slightest doubt as to their fern-nature, and we can even form an idea of the particular group of Ferns to which the affinity is closest.

I will say nothing more as to the true Ferns, though they present innumerable points of interest, but will pass on at once to certain forms of even greater importance to the comparative morphologist.

A considerable number of palæozoic plants are now known which present characters intermediate between those of Ferns and Cycadeæ.   I say *present intermediate characters*, because that is a safe statement; we cannot go further than this at present, for we do not yet know the reproductive organs of the forms in question.

In *Lyginodendron*, the vegetative organs of which are now completely known, the stem has on the whole a cycadean structure, the anatomy, which is preserved with astonishing perfection, presents some remarkable peculiarities, the most striking being that the vascular bundles of the stem have precisely the same arrangement of their elements as is found in the leaves of existing Cycads, but nowhere else among living plants   The roots also, though not unlike those of certain ferns in their primary organisation, grew in thickness by means of cambium, like those of a Gymnosperm.   On the other hand, the leaves of *Lyginodendron* are typical fern-fronds, having the form characteristic of the genus *Sphenopteris*, and being probably identical with the species *S. Hœninghausi*. Their minute structure is also exactly that of a fern-frond, so that no botanist would doubt that he had to do with a Fern if the leaves alone were before him.

This plant thus presents an unmistakable combination of cycadean and fern-like characters.   Another and more ancient genus, *Heterangium*, agrees in many details with *Lyginodendron*, but stands nearer the ferns, the stem in its primary structure resembling that of a *Gleichenia*, though it grows in thickness like a cycad.   These intermediate characters led Professor Williamson and myself to the conclusion that these two genera were derived from an ancient stock of Ferns, combining the characters of several of the existing families, and that they had already considerably diverged from this stock in a cycadean direction.   I believe that recent investigations, of which I hope we shall hear more from Mr. Seward, tend to supply a link between *Lyginodendron* and the more distinctly cycadean stem known as *Cycadoxylon*.

*Heterangium* first appears in the Burntisland beds, at the base of the carboniferous system; from a similar horizon in Silesia, Count Solms-Laubach has described another fossil, *Protopitys Bucheana*, the vegetative structure of which also shows, though in a different form, a striking union of the characters of Ferns and Gymnosperms.   Count Solms shows that this genus cannot well be included among the Lyginodendreæ, but must be placed in a family of its own, which, to use his own words, ' increases the number of extinct types which show a transition between the characters of Filicineæ and of Gymnosperms, and which thus might represent the descendants in different directions of a primitive group common to both.' [1]

Another intermediate group, quite different from either of the foregoing, is that of the Medulloseæ, fossils most frequent in the Upper Carboniferous and Permian strata.   The stems have a remarkably complicated structure, built up of a number of distinct rings of wood and bast, each growing by its own cambium. Whether these rings represent so many separate primary cylinders, like those of an ordinary polystelic Fern, or are entirely the product of anomalous secondary growth, is still an open question, on which we may expect more light from the investigations of Count Solms.   In any case, these curious stems (which certainly suggest in themselves some relation to Cycadeæ) are known to have borne the petioles known as *Myeloxylon* which have precisely the structure of cycadean petioles. [2]

Renault has further brought forward convincing evidence that these *Myeloxylon* petioles terminated in distinctly fern-like foliage, referable to the form-genera

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[1] *Bot. Zeitung*, 1893, p. 207.
[2] Seward, *Annals of Botany*, vol. vii. p. 1.

*Alethopteris* and *Neuropteris*. Hence it is evident that the fronds of these types, like some specimens of *Sphenopteris*, cannot be accepted as true Ferns, but may be strongly suspected of belonging to intermediate groups between Ferns and Cycads.

It is not likely (as has been repeatedly pointed out elsewhere) that any of these intermediate forms are really direct ancestors of our existing Cycads, which certainly constitute only a small and insignificant remnant of what was once a great class, derived, as I think the evidence shows, from fern-like ancestors, probably by several lines of descent.

One of the greatest discoveries in fossil botany was undoubtedly that of the Cordaiteæ—a fourth family of Gymnosperms, quite distinct from the three now existing, though having certain points in common with all of them. They are much the most ancient of the four stocks, extending back far into the Devonian. Nearly all the wood of Carboniferous age, formerly referred to Coniferæ under the name of *Dadoxylon* or *Araucarioxylon*, belonged to these plants. Thanks chiefly to the brilliant researches of Renault and Grand' Eury, the structure of these fine trees is now known with great completeness. The roots and stems have a coniferous character, but the latter contain a large, chambered pith different from anything in that order. The great simple lanceolate or spatulate leaves, sometimes a yard long, were traversed by a number of parallel vascular bundles, each of which has the exact structure of a foliar bundle in existing Cycadeæ. This type of vascular bundle is evidently one of the most ancient and persistent of characters. Both the male and female flowers (*Cordaianthus*) are well preserved in some cases. The morphology of the former has not yet been cleared up, but the stamen, consisting of an upright filament bearing 2–4 long pollen-sacs at the top, is quite unlike anything in Cycadeæ; a comparison is possible either with *Gingko* or with the Gnetaceæ.

In the female flowers—small cones—the axillary ovules appear to have two integuments, a character which resembles Gnetaceæ rather than any other Gymnosperms. Renault's famous discovery of the prothallus in the pollen-grains of *Cordaites* indicates the persistence of a cryptogamic character; but it cannot be said that the group as a whole bears the impress of primitive simplicity? though it certainly combines in a remarkable way the characters of the three existing orders of the Gymnosperms.

There is one genus, *Poroxylon*, fully and admirably investigated by Messrs. Bertrand and Renault, which from its perfectly preserved vegetative structure (and at present nothing else is known) appears to occupy an intermediate position between the Lyginodendreæ and the Cordaiteæ. The anatomy of the stem is almost exactly that of *Lyginodendron*, the resemblance extending to the minutest details, while the leaves seem to closely approach those of *Cordaites*. *Poroxylon* is at present known only from the Upper Carboniferous, so we cannot regard it as in any way representing the ancestors of the far more ancient Cordaiteæ. The genus suggests, however, the possibility that the Cordaiteæ and the Cycadeæ (taking the latter term in its wide sense) may have had a common origin among forms belonging to the filicinean stock. It is also possible that the Cordaiteæ, or plants allied to them, may in their turn have given rise to both Coniferæ and Gnetaceæ.

It is unfortunate that at present we do not know the fructification of any of the fossil plants which appear to be intermediate between ferns and Gymnosperms. Sooner or later the discovery will doubtless be made in some of these forms, and most interesting it will be. M. Renault's *Cycadospadix* from Autun appears to show that very cycad-like fructifications already existed in the later Carboniferous period, and numerous isolated seeds point in the same direction, but we do not know to what plants they belonged.

I think we may say that such definite evidence as we already possess decidedly points in the direction of the origin of the Gymnosperms generally from plants of the Fern series rather than from a lycopodiaceous stock.

I must say a few words before concluding on the cycad-like fossils which are so striking a feature of mesozoic rocks, although I feel that this is a subject with

which my friend Mr. Seward is far more competent to deal. Both leaves and trunks of an unmistakably cycadean character are exceedingly common in many mesozoic strata, from the Lias up to the Lower Cretaceous. In some cases the structure of the stem is preserved, and then it appears that the anatomy as well as the external morphology is, on the whole, cycadean, though simpler, as regards the course of the vascular bundles, than that of recent representatives of the group.

Strange to say, however, it is only in the rarest cases that fructifications of a truly cycadean type have been found in association with these leaves and stems. In most cases, when the fructification is accurately known, it has turned out to be of a type totally different from that of the true Cycadeæ, and much more highly organised. This is the form of fructification characteristic of *Bennettites*, a most remarkable group, the organisation of which was first revealed by the researches of Carruthers, afterwards extended by those of Solms-Laubach and Lignier. The genus evidently had a great geological range, extending from the Middle Oolite (or perhaps even older strata) to the Lower Greensand. Probably, all botanists are agreed in attributing cycadean affinities to the Bennettiteæ, and no doubt they are justified in this. Yet the cycadean characters are entirely vegetative and anatomical; the fructification is as different as possible from that of any existing cycad, or, for that matter, of any existing Gymnosperm. At present, only the female flower is accurately known, though Count Solms has found some indications of anthers in certain Italian specimens. The fructification of the typical species, *B. Gibsonianus*, which is preserved in marvellous perfection in the classical specimens from the Isle of Wight, terminates a short branch inserted between the leaf-bases, and consists of a fleshy receptacle bearing a great number of seeds seated on a long pedicel with barren scales between them. The whole mass of seeds and intermediate scales is closely packed into a head, and is enclosed by a kind of pericarp formed of coherent scales, and pierced by the micropylar terminations of the erect seeds. Outside the pericarp, again, is an envelope of bracts which have precisely the structure of scale-leaves in cycads. The internal structure of the seeds is perfectly preserved, and strange to say, they are nearly, if not quite, exalbuminous, practically the whole cavity being occupied by a large dicotyledonous embryo.

This extraordinary fructification is entirely different from that of any other known group of plants, recent or fossil, and characterises the Bennettiteæ, as a family perfectly distinct from the Cycadeæ, though probably, as Count Solms-Laubach suggests, having a common origin with them at some remote period. The Bennettiteæ, while approaching Angiosperms in the complexity of their fruit, retain a filicinean character in their ramenta, which are quite like those of ferns, and different from any other form of hair found in recent Cycadeæ. Probably the bennettitean and cycadean series diverged from each other at a point not far removed from the filicinean stock common to both.

I hope that the hasty sketch which I have attempted of some of the indications of descent afforded by modern work on fossil plants may have served to illustrate the importance of the questions involved and to bring home to botanists the fact that phylogenetic problems can no longer be adequately dealt with without taking into account the historical evidence which the rocks afford us.

Before leaving this subject I desire to express the great regret which all botanists must feel at the recent loss of one of the few men in England who have carried on original work in fossil botany. At the last meeting of the Association we had to lament the death, at a ripe old age, of a great leader in this branch of science, Professor W. C. Williamson. Only a few weeks ago we heard of the premature decease of Thomas Hick, for many years his demonstrator and colleague. Mr Hick profited by his association with his distinguished chief, and made many valuable original contributions to palæobotany (not to mention other parts of botanical science), among which I may especially recall his work, in conjunction with Mr. Cash, on *Astromyelon* (now known to be the root of Calamites), on the leaves and on the primary structure of the stem in Calamites, on the structure of *Calamostachys*, on the root of *Lyginodendron*, and on a new fossil probably allied to *Stigmaria*. His loss will leave a gap in the too thin ranks of

fossil-botanists; but we may hope that the subject, now that its importance is beginning to be appreciated, will be taken up by a new generation of enthusiastic investigators.

## CONCLUSION.

To my mind there is a wonderful fascination in the records of the far-distant past in which our own origin, like that of our distant cousins the plants, lies hidden. If any fact is brought home to us by the investigations of modern biology, it is the conviction that all life is one: that, as Nägeli said, the distance from man to the lowest bacterium is less than the distance from the lowest bacterium to non-living matter.

In all studies which bear on the origin and past history of living things there is an element of human interest—

'Hence, in a season of calm weather,
Though inland far we be,
Our souls have sight of that immortal sea
Which brought us hither,'

The problems of descent, though strictly speaking they may often prove insoluble, will never lose their attraction for the scientifically guided imagination.